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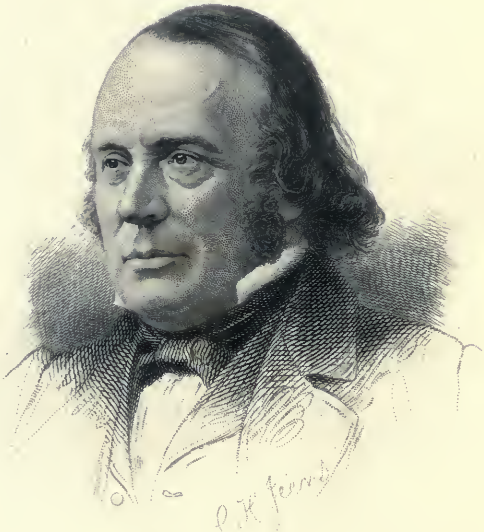

Louis Agassiz

Engraved by C. H. Jeens from a Photograph.

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NATURE

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

VOLUME XIX.

NOVEMBER 1878 to APRIL 1879

> "*To the solid ground*
> *Of Nature trusts the mind which builds for aye.*"—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

*"To the solid ground
Of Nature trusts the mind which builds for aye."*—WORDSWORTH

THURSDAY, NOVEMBER 7, 1878

SANITARY ENGINEERING

Sanitary Engineering. By Baldwin Latham. Second Edition. (E. and F. N. Spon, 1878.)

IN the introduction to this book great stress is laid on the necessity for sanitary measures being thoroughly carried out in all towns and dwellings; one might suppose that this was fully admitted on all sides, but we have no doubt that every medical officer of health throughout the country could easily give numberless instances of the greatest possible neglect and callousness on the subject. While all admit the necessity of efficient sanitary works and are generally quite ready to attribute to defective arrangements illness occurring in a neighbour's house or another town, each individual seems to ignore the possibility of a terrible punishment falling on him for his own neglect. He should recollect that the punishment which must sooner or later overtake him cannot be moderated by the clemency of a chairman of Quarter Sessions or the gentler feelings of a jury, but is ruthlessly administered by the inexorable laws of nature.

Mr. Baldwin Latham doubtless finding it almost useless to preach to people on the necessity of taking care of their own and their neighbours' health very wisely tries an appeal to them through their pockets, and shows the amount of actual pecuniary saving from improvements in the sanitary condition of a community. The town of Croydon is taken as an instance; in this case the average mortality from 1848-55 inclusive was 24·03 per thousand, while that for the twenty years since 1855, when the sanitary works were nearly completed, has averaged 19·56, showing a saving of 4·47 per thousand. But this is not all that is to be looked for; there is evidence that at times the mortality of Croydon proper is considerably increased by an impure water supply, and from the lower mortality of Norwood it appears that a still further improvement could be obtained by the adoption of suitable measures. Multiplying the average saving in the rate of mortality by the population and by the assumed value of labour per head taken at 19*l.* 10*s.* per annum after a deduction of nearly one-half for persons of an unsuitable age for work, the author obtains 413,395*l.* as the value of the saving from the lessened number of deaths in ten years on an average population of 43,912. The saving in cases of sickness not resulting in death is taken at 1*l.* per case on twenty-five times the number of deaths, that being the estimated ratio of cases of illness to deaths; this gives 98,150*l.* as the result, and to this is added the cost of funerals saved, 3,926 at 5*l.* each equal to 19,630*l.*, or a total saving of 531,375*l.* It would appear to us more correct to leave out this last item, as the expense though saved for the present must be regarded as a deferred charge and must be incurred sooner or later. The works having cost in this case 267,665*l.* there remains as a dividend for the twenty years an amount in the aggregate equal to nearly twice the capital. This in the days of discarded gas and failing banks ought, unaided by the arguments of zymotic disease, to persuade the ratepayer to seek an investment in sanitary progress.

A great number of very useful tables are embodied in the text; those of the velocity and flow in pipes and sewers from p. 91 to 153 will be found of great service to the sanitary engineer, being calculated over a much greater range than in other books on the subject, and having been extended in the present edition. We should suggest with reference to tables 29, 30, 31 that a very useful addition to make in a future edition would be a table of areas and hydraulic mean depths with other fractional depths of flow besides one-third and one-half full, and that the use of the velocity and discharge tables would be facilitated by giving the corresponding fall in feet per mile side by side with the given rate of inclination expressed in a numerical ratio.

A chapter is devoted to the question of the admission of rainfall into sewers; the reasons for its exclusion as far as practicable are stated to be (1) to increase the manurial value of the sewage; (2) to obviate the inconvenience attending the purification of a large and uncertain volume of sewage in times of rainfall; and (3) to give to the streams of the country the natural volume of water due to the rainfall within their collecting area, and the adoption of this course receives but partial

recommendation. We believe the author has omitted by far the most important reason, viz., the deposit of heavy road detritus caused by the admission of storm waters, which retards the free flow of the sewage and retains a mass of decomposable matter in the sewers quite sufficient to account for the abominable effluvium emitted by the gullies and ventilators of the London system. A reason advanced for the admission of surface water into sewers given by the author, and to which much weight is attached, is the fact that it was found by the analyses of Prof. Way that the washings from the streets of London resulting from rainfall were equal in impurity to average sewage. If the surface of our streets is permitted to become so filthy that, even when diluted with rain-water, the product is only suited for admission into the sewer, it would surely be better to turn more attention to the collection and carting away of the filth rather than to get it washed away out of sight where its presence will not be remarked until the next dry weather renders it painfully apparent. It is somewhat to be regretted that the author has not devoted a small amount of space to a subject having so important a bearing on the sanitary condition of a town and the successful operation of a system of sewerage as the scavenging of streets, especially when we consider how much improvement in this respect is needed in the metropolis. We believe that in London and other large towns, the saving in the destruction of clothing would at least pay for the proper cleansing of the streets without making any allowance for the saving of time and discomfort in locomotion.

The chapters dealing with the properties of materials and the construction of sewers, contain much useful information derived from the author's own experience and other sources, and may be consulted with much advantage by those engaged on works not only of this, but of other descriptions.

The much vexed question of sea outfalls and the influence of tidal currents on the selection of site is discussed, but it seems a pity that where ignorance and prejudice demand this mode of disposal, the author should not have laid stress upon the necessity of abstracting much of the solid matters held in suspension, thus much diminishing what is becoming an intolerable nuisance in many sea-side places. In giving so much importance to dilution with tidal water, it should have been borne in mind that this takes place in the direction of the breadth and depth of the volume of liquid discharged; but in the case of the solids floating on the surface, only in the former direction, and in both cases very slowly, as may be seen by an inspection of the metropolitan outfalls. It is impossible in the space at our disposal to notice the numerous details described and illustrated; the plates of all the more important are carefully drawn and well executed, no trouble being spared to make them thorough working drawings, while a sufficient number of woodcuts are introduced amply to illustrate the text. There are other books on this subject of a more popular nature, containing most of the information required by those who desire a general knowledge of the subject freed from too technical matters, but this is undoubtedly the best book hitherto published in this country for the student of practical sanitary science and for the engineer who requires a thorough treatment in detail of that branch of his practice.

THE NAPLES ZOOLOGICAL STATION

Mittheilungen aus der zoologischen Station zu Neapel, zugleich ein Reperatorium für Mittelmeerkunde. Erste Band, I. Heft. (Leipzig: 1878.)

SINCE the foundation of the Zoological Station at Naples, nearly one hundred naturalists have worked in the laboratory connected with it, and a goodly number of papers, which have resulted from their labours, are scattered through the biological periodicals of almost all the civilised nations of Europe. Gratifying as this success must be to Dr. Dohrn, the founder of the institution, he does not show himself inclined to repose on his laurels, but aims at still further extending the scope of the station by starting two publications in connection with it. One of these, of which we have the first number before us, is published in octavo size, and, as we learn from the preface, is intended for smaller papers, and general notes on the habits of animals living in the Aquarium, and other zoological topics. It will, moreover, be the medium for recording the systematic observations now being carried on by the permanent staff at the station. The second publication will be in quarto size, and will bear the title "Fauna u. Flora des Gulfes von Neapel und der angrenzenden Meeresbezirke." As its name indicates it will consist of fully illustrated monographs of the various groups of animals found in the Bay of Naples or adjoining seas. The parts may be purchased separately, or may be subscribed for by the payment of 1*l.* yearly. The contents of the first part of the "Mittheilungen" promise very well. Dr. Schmidtlein, who manages the public aquarium, contributes three short papers. One of them gives an interesting account of the habits of a large number of the various animal forms living in the aquarium. A second deals with the periodic appearances of pelagic animals in the Bay of Naples during the two past years, and the third is a list of the breeding times of the marine forms inhabiting the Neapolitan seas. Dr. Hugo Eisig, the general manager of the station, contributes a paper of very great importance on the segmental organs of the Capitellidæ. He shows that, in some species of this group, it is normal for several segmental organs to be present in a single segment, and that the number of these organs present in a segment increases in passing from before backwards. Dr. Eisig compares the segmental organs in Annelids with the segmental tubes in Vertebrata, and points out how closely the arrangement he has found in the Capitellidæ agrees with that described by Dr. Spengel in some Amphibia. There is an illustrated paper by Dr. Meyer on some points of crustacean anatomy, and two botanical papers by Drs. Falkenberg and Smitz. Dr. Dohrn himself communicates some observations on the Pycnogonidæ, in which he adduces a large amount of evidence to prove that the view as to the number of their appendages put forward by him some years ago, which was subsequently attacked by Semper, is, in all essential points, correct.

The number as a whole is very creditable to the zoological station, and we may congratulate the founder upon the continued prosperity of the institution, as evinced by its ever-increasing activity in all directions.

F. M. B.

OUR BOOK SHELF

Theorie der algebraischen Gleichungen. Von Dr. Jul. Petersen. xii. and 335 pp. (Kopenhagen, 1878.)

THE author tells us that this work owes its origin to the lectures he has given on the theory of equations at the Copenhagen Polytechnic School. In the preparation of it he has made use of J. A. Serrêt's "Cours d'Algèbre Supérieure," Todhunter's "Theory of Equations," and Jordan's "Traité des Substitutions." The first section treats of equations in general; Cap. I. general properties of algebraic equations; Cap. II. relations between the coefficients and roots; Cap. III. on elimination, describing the methods of Labatie, of Euler, of Sylvester, of Bezout, and of Poisson; Cap. IV. the transformation of equations. The second section is devoted to the algebraic solution of equations, viz., of the cubic (the methods of Hudde, Lagrange, Tschirnhausen, and Euler); of the biquadratic (the methods of Lagrange, Descartes, and others); the binomial equation, the Quintic, the breaking-up of a rational polynomial into rational factors, Abelian equations (a long chapter, including the division of a circumference into seventeen equal parts, and the reduction of the equation $x^{13} = 1$).

The third section is on the Numerical Solution of Equations: Cap. I., on the Separation of Roots (Descartes', Budan's, Rolle's, Sturm's, and Newton's theorems); Cap. II., the Calculation of the Roots in Numerical Equations (interpolation, of Newton's Method of approximation, also Lagrange's and Horner's methods). The fourth part, which treats of Substitution in four chapters: Cap. I. Substitution in General; Cap. II. (a long chapter, including the theorems of Lagrange and Cauchy, alternate, transitive, and intransitive groups, linear substitutions, &c.); Cap. III. Galois' Theory (this has not found its way into English text-books; Prof. H. J. S. Smith classes Galois, for early precocity, with Pascal and Gauss); Cap. IV. Applications of Galois' Theory (Abelian equations, the Galois and the Hessian equations).

This bare enumeration of the principal articles will show that this carefully-written treatise takes up some ground which has not yet been opened out or even alluded to in our common text-books on equations.

The Botany of Three Historical Records, Pharaoh's Dream, The Sower, and the King's Measure. By A. Stephen Wilson. (Edinburgh: David Douglas, 1878.)

THIS is a curious little book, the author's aim being to throw what light he can, either by comparison or suggestion, upon the probability of the plants referred to in these Scripture records being this or that species of cereal. Mr. Wilson seems to have given a good deal of consideration to each of the above questions, which, as he says in his preface, have only one bond of connection between them, namely, "a common basis in the botany of the cereal grasses." Notwithstanding the pains the author has evidently given to each of the subjects, we cannot but think that it will prove of but little value, the points advanced being by no means conclusive, and even the subjects in themselves being of small importance. It may be of some value to know whether the cereals "stand in the same alimentary relationship to mankind as they did when Joseph laid up the surplus of the plenteous years in the granaries of Egypt," because such a knowledge, if it could be proved, would show the progress made in developing the productive resources of these grasses, but whether the plant in Pharaoh's dream was *Triticum compositum*, or any other species of *Triticum*, is perhaps of little moment to mankind at the present time. As an illustraiton of what is to our mind mere speculation, we quote the following from p. 6:—"The wheats of 'Minnith,' in the Belka (Ezek. xxvii.) grown by the farmers of Judah and Israel, seem to have been in demand in the corn-market of Tyre. Probably Minnith was a remarkably good locality for wheat, so that when the husbandman in other districts got seed from this place they called it Minnith wheat."

The author's summing-up of this his first "Historical Record," namely, that "seven ears of corn came up upon one stalk," is that it "may be wrong, and probably is wrong, whereas the reading here proposed, that seven ears of corn came up upon one *stock*, while probably expressing the full meaning, can only err by defect, and must necessarily be right, as embracing an essential morphological fact common to all varieties of corn."

The Commercial Products of the Sea; or, Marine Contributions to Food, Industry, and Art. By P. L. Simmonds. With thirty-two illustrations. (London: Griffith and Farran, 1879.)

THIS is the first example this year we have had of a work antedated, in this case by more than two months. We cannot possibly see what is gained by this; is it meant to make readers of future years believe that a work was published a year later than it really was? If this is so, is it quite honest and respectable—to put it in the mildest possible form? When one gets over Mr. Simmond's extraordinary and often misleading style (for which we commit him to the tender mercies of the literary Dr. Birch), it is found that his work contains a great mass of useful and curious information, showing great diligence in the collection of facts, if not much skill in putting them together. Mr. Simmonds' work is divided into three parts, dealing with food-products obtained from the sea, marine contributions to industry, and marine contributions to art. Detailed accounts and statistics are given of the various fisheries of the world, under the first head; under the second head the sponge fisheries are dealt with, oils, isinglass, shells, seaweed, marine salt, and other products; and under part iii. tortoise-shell, mother-of-pearl, coral, and amber. It will thus be seen that the work has a wide range; it shows how much has been done, and how much yet remains to be done by science, to make the most of the products with which the waters swarm. Altogether the work contains much useful and interesting information in a handy form.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

A Question raised by the observed Absence of an Atmosphere in the Moon

IT is known that there is physical evidence of an absence of atmosphere in the moon. It would appear reasonable to conclude that the moon at one time had an atmosphere; for, according to the generally-accepted principles of Laplace, which make the sun and members of the solar system to have a common nebulous origin, it would seem very extraordinary if the particular offshoot of the common nebula which formed the moon had no gaseous constituent in it. If we admit, therefore, as probable that the moon at one time had an atmosphere, the question naturally suggests itself as to what has become of it. Various surmises have been hazarded in reply to this. I would venture to submit the following as a possible explanation, which, as far as it goes, is based on accepted principles:—It is known to be a demonstrated fact in connection with the established kinetic theory of gases that the velocities of the molecules of a gas vary among themselves from zero to an *indefinitely* great

velocity, *i.e.*, a velocity to which apparently no limits can be set. It is true that the molecules which in the accidents of collision among themselves acquire these enormous velocities, have been mathematically proved to be relatively few in number, the greater number of the molecules possessing velocities approaching the mean value. But it would seem to follow necessarily that molecules situated in the top stratum of any atmosphere, and which acquire these enormous (indeterminable) velocities, can sometimes overcome gravity, and be projected into space, so as not to return; as it is a known fact that only a *finite* velocity is required to effect this result. I have therefore to suggest that by this cause the moon's atmosphere has gradually disappeared. It is probable, no doubt, that it would take a vast period of time to have brought about this result, but we have an almost unlimited time at disposal. It might possibly be asked, How is it that the earth's atmosphere has not shared the same fate? In answer to this I would reply, first, that the value of gravity on the earth is known to be very much greater than on the moon, and second, that possibly (for aught we can tell) part of the earth's atmosphere may have thus disappeared; or the earth's atmosphere may be less dense at present than at one time, for anything we can say to the contrary. It would seem a curious fact to note in connection with this that there would be apparently grounds for inferring that the *constitution* or composition of the earth's (or any other planet's) atmosphere might have changed from the above cause, as evidently the lighter gaseous constituents, whose molecules acquire in the accidents of collision the highest velocities, would be first dissipated into space in the above manner. Thus, for example, any trace of that very prevalent constituent of the universe, hydrogen, that might have at one time existed in the earth's atmosphere, would have tended to become relatively rapidly eliminated, as the molecules of hydrogen are known to possess a normal velocity about four times as great as that of the constituent molecules of the earth's atmosphere.[1] It might be said that changes so great as those above indicated are scarcely realisable, but then it should be kept in view that we have an almost limitless range of time to draw on, and it is generally admitted to be very important to take the effect of time into due consideration, as, for example, is done in the case of geology, where mountain ranges are recognised by incontrovertible physical proof to have been carved out by the slow disintegrating action of rain and atmospheric influences prevailing through countless centuries. The gradual disappearance of an atmosphere (earth's or moon's) under the above cause might possibly be compared in slowness of operation to the other cosmical changes that the solar system is known to be undergoing, such as the gradual approach of the earth to the sun (and of the moon to the earth) through the friction of the material media in space, the accomplished stoppage of the moon's axial rotation by tidal action on its mass, and the gradual diminution of the earth's rotative velocity from the same cause. These slow changes, imperceptible in the range of human experience, become important in large time epochs, and it becomes desirable in the interests of truth, in tracing back events, to give due weight to these time epochs. In suggesting the above explanation, I have endeavoured to confine myself strictly within the limits of mathematically proved facts as a basis to draw deductions upon, and I should be glad to accept any criticisms that might be offered, either with the view to point out a difficulty or confirm the truth.

London, October S. TOLVER PRESTON

Remarkable Local Colour-variation in Lizards

THE following extract from a letter received some months since from Baron de Basterot, of Rome (a Fellow of the Geological Society of London), records an interesting case of local colour-variation, about which some of your correspondents may be able to give us further information:—

"Capri is a mass of the usual yellowish-white Apennine limestone, forming precipitous cliffs nearly all round the island. At its southern extremity are three high and nearly inaccessible rocks called I Faraglioni, one of which, pierced by a natural arch, has been frequently depicted by artists. Two of these rocks are completely detached from the mainland, and, I need hardly add, uninhabited.

"On the island, and on the first of the Faraglioni rocks which is connected with it, the lizards are of the usual species so common in Italy—coloured grey, mixed with more or less green. On the two outward Faraglioni rocks, which are completely separated from the shore, their colour is totally different. The back is of a blue so dark as to appear nearly black; the sides of a brilliant blue, like lapis-lazuli; the belly light whitish-blue, with a very slight tinge of green.

"An English gentleman whom I met in Capri had several of these lizards alive, which had become quite tame in the course of a couple of months. I believe he intends bringing them to England. He is of opinion that they differ in colour only from the lizards of the island, and that, though very different in appearance, they are the same species.

"Whether this be so, or whether they are specifically different, their presence on these isolated rocks and their total absence on the island is equally remarkable."

ALFRED R. WALLACE

Termites kept in Captivity by Ants

WHEN entomologising in Portugal in 1877, in the neighbourhood of Cintra, I found the nest of *Formica nigra* under a stone. On my turning it over there was, as usual, great consternation in the community, and I discovered that it was evidently caused by the fear lest a colony of *Termes lucifugus*, which the Formicas had enslaved, should escape. The Nigras instantly began seizing the Termites, driving them underground by the nearest orifices, in the meantime wrenching and pulling off their wings in the most unceremonious manner. I observed a large number of wings lying in heaps here and there in the nest as if this treatment had been practised before. In the nest there was also a great number of Termite larvæ. The great object of the owners of the "location" seemed to be to get these larvæ underground as speedily as possible. The ants fell on them with great impetuosity, seizing them anyhow and anywhere, dragging them against the most strenuous opposition (their behaviour strikingly contrasting with the meekness of their winged fellows) into the nearest apertures of the underground home. Very often this opposition resulted in a long and stern fight, in which the larvæ were often badly wounded, being deprived sometimes of their antennæ, sometimes of half their jaws, and not seldom killed outright. Occasionally, however, the larvæ were victorious, beating off the Formicas, in which case they (the larvæ) did not make off, but remained perambulating the nest. I saw one larva drawn at the end of a long fight by its antenna, while it strenuously held on to a small ball of earth which had proved a vain anchorage for its feet, for larva and clod together were dragged across the top of the nest (made by the impression of the stone) five or six inches, up the side, 1½ inch, and away among the grass, where, losing the ball of earth, it seized a stalk so firmly that its abductor could not drag it farther, whereupon, after reconnoitring the ground for a little distance the latter disappeared, but returned shortly with a companion, with whose aid the larva was detached. This done, the helper returned home while the abductor proceeded with his prisoner till lost to view in the grass, some twelve or fourteen inches from the spot whence it originally started.

In the same neighbourhood I watched for some time a nest of *Formica ligniperda*. An injured female was placed in the nest, but no assistance was rendered, while it crawled along towards the nearest orifice leading underground. At the spot where this individual was injured some of the fluid of its body which had oozed out was eagerly lapped up by the others; some even applied their mouths to the wounds on the body. During the operation of lapping the maxillæ were kept perfectly still, and the antennæ close to the side of the head "feel-feeling" the ground with the tips, as if to discover the spot where the liquid was to be found. Every now and then, however, they were extended at right angles to the body, as if to obtain a more general survey of things, and then immediately returned to their previous position. On several of those which were busy lapping I poured some spirits of wine. They instantly became stupefied, and for a time motionless. When in this condition they were

[1] The realisation of a possible diversity at a former epoch in the constitution and density of the earth's atmosphere raises rather a curious question in connection with the known diversity of the plants and animals that formerly inhabited the globe, as compared with those at present existing. It might be observed that admitting the possibility of the former existence of an atmosphere on the moon, it would seem to follow that an interchange of molecules between the two atmospheres (those of the earth and moon) must have taken place at one time to a certain extent under the above cause, though the considerably less value of gravity on the moon compared with the case of the earth would facilitate the passage of molecules away from the moon and render correspondingly difficult the passage in the reverse direction.

visited by many of their fellow colonists, who, having cursorily examined them, fell to touching them with their antennæ on the abdomen, reminding me much of a mesmerist making passes over a victim. The effect was almost electrical. I was surprised to see the incapables at once begin to rally. After stretching their legs and moving their antennæ they moved along slowly for one or two steps and then went along as if nothing had happened. Others came and drank of the spirit not quite evaporated, but did not seem to suffer any bad effects. I buried a member of the community as it was in the act of carrying off a larva. Although many came and looked on none took compassion or attempted to relieve their friend. A small heap of larvæ, however, which I pressed down into the soft earth with my pencil, thereby injuring some of them badly, was disinterred, and every individual carried into a place of safety.

A stranger placed in the nest was very soon set upon, and before long its head was travelling on a direction opposite to that of its abdomen. The headsman's reward was a long draught of blood from the severed abdomen.

On my turning over the stone at first, the larvæ were exposed, but were soon begun to be carried off. Some of the workers were certainly busybodies, fussing about, pretending to do a great deal, while in fact they were shirking their fair share of the household duties. They would rush at the larvæ, seize one and be off with it in a great hurry, but they had not gone far (not even always in the direction of the entrance) before they changed their minds, threw down their load to return for another helpless infant, which was treated in the same way, being carried generally in a direction contrary to the previous, and dropped down anywhere, sometimes beyond the limits of the nest altogether.

My observations with regard to ants dropping intentionally or jumping from small heights do not quite agree with Sir John Lubbock's, but they are not yet full enough to give in detail. I hope to have fuller opportunities for the investigation of the habits of this most interesting class in the Malayan Archipelago, whither I am now bound.

Meantime I hope these few notes may have some interest for the readers of NATURE. HENRY O. FORBES

S.S. *Celebes*, off Naples, October 18

Colour-blindness

HAS it been suggested that the traditional blindness of Homer may have been—in the absence among the ancients of a specific name for colour-blindness—*merely* the colour-blindness for which Dr. Pole makes out so good a case? To readers ignorant of Daltonism, blindness must have appeared the only explanation of a glaringly misapplied colour-epithet. It is at least clear that the author of the Homeric poems was not always blind in the modern sense of the word.

Brighton, November 1 CLEMENTINA BLACK

THE conclusion of Dr. Pole's valuable paper will doubtless stir up many to investigate the question whether or not dichromatism was the rule at an early stage of human vision.

Will you allow me to adduce, towards the solution of this question, the evidence of a literature, which though not nearly so ancient as the Greek, goes back further than that of many European nations. I mean the Irish. I find in some of the earliest works in that language an ambiguity in the application of adjectives of colour very similar to that noticed in the Homeric writings by Mr. Gladstone. *Glas*, for instance, is used, indifferently, apparently, for green, grey, and blue. *Uaithne* is used to indicate the colour of grass, and also that of the human eye. *Dearg* is employed to denote the colour of wine, and also that of clay. *Ruadh* (red) is similarly ambiguous.

182, Adelaide Road, N.W. EDMUND MCCLURE

Carrier-Pigeons

IN NATURE (vol. xviii. p. 682) it is stated that carrier pigeons are being "turned to useful account" in a new direction in Germany, for Consul Ward writes to the Foreign Office "that the successful results attained by the establishment of communication between the two Eider lightships and the Port of Tönning, in Schleswig, by these means has led to its organisation" elsewhere. This mode of communication is, however, not new, as carrier-pigeons were employed early in this century as a means of communication with the Bell Rock Lighthouse, as mentioned in my late father's "Account" of that work. The pigeons passed between the lighthouse and the shore—a distance of eleven miles in eleven minutes. The employment of these birds, however, was, I suppose, found to be more curious than convenient, for they have long since ceased to be employed. The pigeons were presented to the establishment by the late Sir Samuel Brown, R.N. THOMAS STEVENSON

Edinburgh

Globular Lightning

As the curious phenomenon known by the above name seems to be attracting some attention just now, I venture to send you the following details, which, though of rather ancient date, are still, owing to their startling character, very fresh in my memory.

I think it was in the year 1866, in the beginning of the month of August, that I was walking in the garden when the atmosphere became exceedingly oppressive (there had previously been a very long drought), and thinking by the appearance of the sky, which looked lurid and threatening, that a storm was coming on, I made for the house. As I was going up our front steps some rain-drops fell, which were the largest I ever saw. I had just reached the dining-room and was standing near the window, which looks north, when I saw a large ball of fire, which appeared to me, looking at it as I did from a distance, to be the size of a globe such as is used in schools, descend towards the earth. In descending it struck the church, which is immediately opposite our house, and brought with it a number of slates and part of a stone cross, making a terrific noise. There was a flash of lightning soon after, followed by a moderately loud clap of thunder, but nothing more. As there were not at that time any houses near to ours I did not hear the occurrence mentioned by any one. The noise, though extremely loud, was not at all like thunder. The illumination of the rooms by the ball of fire was seen by two other persons in the house. CHARLOTTE HARE

St. John's Road, Putney, S.W.

Speaking-Trumpets

THE antiquity of the speaking-trumpet may be proved upon far higher authority than that of the imaginative Athanasius Kircher. It is literally as old as the Pyramids. While examining Lepsius's great work upon ancient Egypt for my "History of Music" I noted two examples among the plates of the fourth dynasty of Egypt (see Lepsius's "Denkmäler," Dyn. 4, Abt. 2, Blätter 27 and 30). The Egyptian speaking-trumpets seem to have been some five feet or more in length, and too wide in diameter to have been blown by the mouth. They are conical, and lack the contraction near the mouth-end which is so observable in their war-trumpets.

WM. CHAPPELL

Toughened Glass

MY own experience supports the necessity for caution in using Bastie's toughened glass. Shortly after its introduction I had some graduated measures, and although they were sufficiently tough to bear the shock consequent on falling five or six feet to the ground, yet after a time some short scratches appeared on their surface, and these rapidly spreading till they nearly covered the whole of the glass, when but a slight touch was sufficient to make the measure fly into fragments. One placed on a shelf subject to rather rapid change of temperature, without any handling or apparent cause, broke up suddenly into tiny pieces, behaving, indeed, as if it were a Rupert's bomb.

Northampton, October 29 G. C. DRUCE

POTTERY AT THE PARIS EXHIBITION

THE extensive collections of pottery at the Paris Exhibition brought together from so many countries, is of high interest from a technical, as well as from an

art point of view. All that is now being done in pottery manufacture, all that has lately been achieved in the way of progress, has been here illustrated. An examination, even a rapid one, shows at once how far in advance of other countries England and France are.

Leaving all strictly art questions out of consideration, it is instructive to notice how the technical processes of manufacture impose limits on an artist's scope, and how these scopes have been widened by recent discoveries. It is not intended in this short note to do more than allude to the more important of these, and before doing so it is worth while mentioning that for domestic purposes English earthenware is still unapproached.

The *pâte-sur-pâte* decoration, so largely used in England and in France, is a good illustration of how a process in itself confines the artist's power within certain limits.

The nature of the ornamentation consists in applying by the brush, and modelling with tools, raised decorations of "paste," which is often, for the sake of artistic effect, in high relief. The paste is of much the same composition as the body on which it is applied, and requires a similar temperature to convert it into China, *i.e.*, 1,800° C. The colours which can be used for staining this paste must therefore also be capable of being produced at this heat, and the result is an entirely new range of ceramic colours. It is fortunate for the success of this style of decoration that the colours obtained are harmonious, of a subdued tone. They are quite unlike any that can be produced at a lower heat. Apart from the artist's manipulation, which may vary much in delicacy, the general effect of the production is almost wholly the natural result of the process, and is therefore not due entirely to the artist.

Another and distinct application of raised decoration is very largely represented in the French court. It was first used at Bourg-la-Reine some ten years ago, and is now made in many other localities. It consists of painting in clay on earthenware with pallet and brush in various gradations of relief, somewhat like impasto. The heat for firing is comparatively moderate, and the range of colours that can be employed is very wide.

The difficulties of painting *under glaze* are by degrees being overcome, and one manufacturer has, for the first time, produced gilding *under glaze*. The coloured glazes shown are rich and brilliant, and are well worth the particular notice of those who have paid attention to their production.

The organisation of the Sèvres manufactory and the fact that it is under the direction of a chemist of repute lead to expectations of discovery resulting from the research here carried out. And as a fact discoveries of no small value have been made of late years. Besides such discoveries as of compounds yielding new colours, there are some which take rank as new processes.

For example, the late François Richard, an artist on the staff of the manufactory, found that a large proportion of the enamel colours can be made which will bear a temperature of 600° C.—a higher temperature than has been hitherto supposed possible. This higher temperature now employed fuses and softens the glaze; the colours painted on it blend with it so that, on cooling, there is produced that softness and brilliancy hitherto characteristic of *pâte-tendre* decoration. This process has been named the *demi-grand feu*. A great benefit arising from this discovery is that many vases damaged in firing, which would formerly have been abandoned, can now be preserved, as the accidents which so often happen in firing can be repaired. Defects in glaze and colour can be concealed, as, during an exposure to this *demi-grand feu*, they are at melting-point, and new glaze, when added, becomes so fused that no line of junction is visible. This process also gets over a difficulty that had long been felt in decorating kaolin, or *pâte-dur*, china, with a pleasing result, as the colours were always crude and harsh, being *on* the glaze. When a soft effect was required recourse was had to the less durable and more costly *pâte-tendre* as a body on which to paint.

A new method for decorating porcelain where gold alone is employed has been invented by M. Réjoux. Formerly, when porcelain was decorated with gilding alone, the ornamentation was limited to the production of a pattern by the greater or less relief of the gold, and by its being burnished or left dead. Even the thinnest part of the gilding was opaque. No delicate effects could be produced, and the style was suited rather for an abundant display of barbaric wealth than for refinement of expression.

The new process enables the gilding to be put on so thinly and transparently that the most delicate effects of light and shade can be produced. It is, however, applicable only to vases of *gros bleu* colour, that is, to vases coloured with oxide of cobalt. Upon this ground the pattern is drawn with a pigment composed of oxide of aluminium. This is then subjected to a firing which fuses the oxides together, and a brown surface results. This surface is found to be more suitable than any other known for the reception of the gold paste, which can be laid on in a thin film, and then, further, by subsequent removal, can be made to give great transparency. This property of the brown surface is not destroyed by being tinted before the gilding, so that it is possible to tint it with different colours which shall show through. A further variation is very frequently obtained by changing the tone of the gold by mixing it with alloys. This admirable effect of transparency has not been produced by any other means, and the first piece made (with many subsequent) is exhibited.

Another of the processes invented at Sèvres is that of enamelling on *pâte-tendre* body. In this the colours are applied in powder in the same way as in enamelling on metal, and are fused at a very low temperature. They have more body and are more decided than are those produced by the older enamelling process. On some of the vases shown at the Exhibition the white ground seen is not that of the creamy *pâte-tendre* body, but the pearl white of the stanniferous enamel.

Other recent improvements which should find a place in a more technical and exhaustive notice have been illustrated at the Exhibition. It may be mentioned that other nations are striving to adapt some of the traditionally recognised styles and their method of manufacture.

We may, in conclusion, refer to a small but important exhibition of porcelain, *allant-au-feu*, useful for laboratory as well as for domestic use. It is a very good white, thin and hard, and will bear high temperatures if the changes are not too sudden.

SUN-SPOTS, ATMOSPHERIC PRESSURE, AND THE SUN'S HEAT

THE question whether the atmospheric pressure varies with the spotted surface of the sun was noticed by me in a paper on the Isobars within the British Isles. I could not, however, find any appearance of a decennial law in the yearly mean pressures: such a relation presented itself however in the varying directions of the isobars (*Proc.* Roy. Soc., 1877, p. 599). The yearly mean pressures in our latitudes are subject to large irregular variations, and several decennial periods would be requisite before these could be neutralised in the decennial means. As the irregularities are much smaller within the tropics, I did not fail to examine the yearly means for India which were in my possession at the time; and I found their variations very small and apparently without any relation to the decennial period. Mr. F. Chambers's interesting letter to NATURE (vol. xviii. p. 567) has

induced me to make a more careful study of the Indian observations at present before me.

Mr. C. Chambers had noticed previously the appearance of "a periodicity of very small range, which nearly corresponds in duration with the decennial sun-spot period ("The Meteorology of the Bombay Presidency." London, 1878, p. 12). It is obvious, whatever may be the results derived for a single station, that they can have little value if contradicted by those obtained elsewhere. I have shown that from Singapore to Simla the *daily* mean pressure varies in nearly the same way, the maxima as well as the minima occurring simultaneously or nearly so over all India (*Proc.* Roy. Soc., 1876, p. 24). Are then the *years* of maximum and minimum pressure also the same over India? The question has considerable interest independently of the existence of a decennial law. The following table contains the differences (Δ B) of yearly mean barometric height in *thousandths of an inch* from the means derived from the whole series of observations at each station. I possess at present unfortunately only an incomplete series for Trevandrum and the variations for 1841-48, and for 1853-64 are those from the means for each of these series of years, the barometers having been different. I have also added the differences of yearly mean temperature in *tenths* of a degree Fahr. from the means for each series of years during which the thermometer preserved the same position. The different series are separated by bars ——.

Year.	ΔB				ΔT			
	Singapore.	Trevan-drum.	Madras.[1]	Bombay.[2]	Singapore.	Trevan-drum.	Madras.[1]	Bombay.[2]
1841	0	0	—	—	− 1	− 2	—	—
1842	− 4	+ 6	− 5	—	+ 2	+ 1	+ 1	—
1843	− 5	− 11	− 7	—	+ 3	0	− 6	—
1844	− 3	− 1	− 3	—	− 4	− 2	0	—
1845	+ 11	+ 19	+ 19	—	+ 1	+ 7	+ 7	—
1846	—	+ 13	+ 14	—	—	+ 5	+ 2	—
1847	—	− 13	− 8	− 11	—	− 8	− 2	− 6
1848	—	− 9	− 7	− 3	—	− 1	− 2	+ 2
1849	—	—	− 23	− 10	—	—	+ 2	0
1850	—	—	− 7	0	—	—	− 2	+ 3
1851	—	—	− 6	− 12	—	—	− 5	− 1
1852	—	—	+ 5	− 3	—	—	− 4	0
1853	—	+ 3	+ 6	+ 6	—	—	+ 2	+ 3
1854	—	− 2	+ 2	− 4	—	—	+ 4	+ 4
1855	—	+ 15	+ 20	+ 16	—	—	+ 5	+ 5
1856	—	+ 5	—	− 2	—	—	—	+ 3
1857	—	—	—	0	—	—	—	− 3
1858	—	—	—	+ 4	—	—	—	− 1
1859	—	+ 2	—	+ 4	—	—	—	0
1860	—	− 6	—	− 4	—	—	—	− 2
1861	—	− 11	—	− 11	—	—	—	− 9
1862	—	− 19	—	− 25	—	—	—	+ 4
1863	—	− 11	—	− 16	—	—	—	− 4
1864	—	+ 23	—	+ 24	—	—	—	− 3
1865	—	—	—	+ 3	—	—	—	+ 8
1866	—	—	—	+ 14	—	—	—	− 9
1867	—	—	—	+ 16	—	—	—	− 6
1868	—	—	—	+ 28	—	—	—	− 1
1869	—	—	—	+ 6	—	—	—	+ 7
1870	—	—	—	− 11	—	—	—	− 2
1871	—	—	—	− 3	—	—	—	+ 9
1872	—	—	—	− 13	—	—	—	− 3

[1] There is an error of − 20 in the mean pressure given in the Madras observations for 1846 (here corrected).

[2] The variations for Bombay are taken from Mr. C. Chambers's "Meteorology of the Presidency of Bombay," pp. 11 and 32.

An examination of the quantities ΔB will show that all the principal deviations from the mean pressures occur in the same years and have the same sign at all the stations. The agreement of the variations for Trevandrum and Bombay in the six years 1859-64 is also very remarkable.

We see that there was a maximum of pressure in 1845 at the first three stations, a minimum at Madras and Bombay in 1849 (best marked at the former station), a maximum at the last three stations in 1855; a minimum in 1862, and a maximum in 1864 at Trevandrum and Bombay (the latter station showing a maximum also in 1868). These means, then, confirm in a general way for all India the result obtained by Mr. C. Chambers for Bombay,[1] that on the whole the mean atmospheric pressure is greatest near the epochs of minimum, and least near the epochs of maximum sun-spots, though there is no exact agreement between the two classes of variations from year to year.

And as the maximum pressure in 1855, shown at all the three stations, is considerably before the epoch of minimum sun-spots, it would be difficult to conclude that the epochs of one phenomenon lag on those of the other.

I consider this result to be one of very great importance. The exactness with which the mean barometric height has been obtained with standard instruments in first-class observatories, and the general agreement of the years of maxima and minima over India, give a weight to it which cannot be extended to an element so variable with locality as the rainfall. Although the barometric variations are not sufficient to explain those of the amount of rainfall (which is so different in different years, and for the same years at the different stations), yet they give a probability to the existence of similar laws in the variations of the meteorological elements which I believe was previously wanting.

Mr. F. Chambers's note was written chiefly with the object of deducing from the barometric means evidence that the emission of solar heat is greatest in years of most sun-spots. He says: "It is well known that in Central Asia the annual variation of the barometric pressure is greater than in any other portion of the globe, and it is universally admitted that this variation is due to the great variation of temperature between summer and winter, the barometer being low when the temperature is high, and *vice versâ*. If, therefore, the absolute heat of the sun is subject to considerable variations, we ought to find the barometric pressure in Central Asia responding to those variations just as it does to the annual variations of temperature; in other words, the summer barometric minimum should be lowest in those years when the sun is hottest, and the winter maximum should be highest in those years when the sun is coldest."

As I do not admit that the annual barometric oscillation is due to variation of temperature, I do not accept the conclusion: for though there is an apparent relation between the two, within certain geographical limits, and under certain local conditions (which are altogether independent of the heat emitted by the sun), there is no evidence that the one is *due* to the other. The true conclusion is that the local conditions which are favourable to a large oscillation of the monthly mean atmospheric pressure are favourable to a large oscillation of the monthly mean temperature: always within certain geographical limits, for beyond these the relation does not hold.

The subject is, I think, a very interesting one, and merits consideration. The relation of the oscillations of the monthly means will be seen from the ranges in the following table:—

[1] They also add another epoch, 1845.

	Lat. N.	Long. E.	Ranges of Monthly Means.[1]		
			Barometer.	Thermometer.	Bar. oscillation for 1°.
	° ′	°	in.	° F.	in.
Singapore	1 19	104	0·064	3·1	0·021
Trevandrum ...	8 30	77	0·112	4·8	0·023
Madras	13 4	80	0·299	11·8	0·025
Bombay	18 54	73	0·285	11·4	0·025
Pekin	39 54	117	0·777	52·8	0·015
Catherinenburg ...	56 50	60	0·310	64·8	0·005

I have not the means for Calcutta, but they confirm the relation shown for the other Indian stations. At Madras and Bombay the barometric oscillation corresponding to 1° F., attains a maximum, it has diminished considerably at Pekin, and at Catherinenburg it cannot be said to exist; for though the minimum pressure occurs near the epoch of maximum temperature, the time of maximum pressure may be said to extend over the seven months, October to April, during which the monthly mean temperature varies 37° F. Also at Hobarton (42° S.) the relation does not exist, though the range of monthly mean temperature is nearly 20°.

When we examine the individual cases in India more in detail, several facts present themselves which are opposed to any relation of cause and effect between the two phenomena. Thus, as regards the epochs of maximum of one variation, and minimum of the other, though both show considerable approximation to the solstices, yet there are some marked differences. At Bombay, where the barometric oscillation is greatest for a given oscillation of temperature, the epoch of maximum pressure *precedes* by nearly a month that of minimum temperature, while the epoch of minimum pressure occurs more than a month *after* that of maximum temperature. We find also that the relation of increasing oscillation of pressure with increasing oscillation of temperature which holds within certain limits for different places does not hold at the same place. That is to say, years of largest annual variation of monthly mean temperature are not years of largest variation of monthly mean pressure, and in some cases (as at Trevandrum) the range of monthly mean temperature may be nearly twice as great in one year as in another.[2]

No better example, however, of the relation of the two oscillations to *local* conditions can be given than that presented by the variations at two stations within sixty English miles of each other, Trevandrum within four miles of the sea on the western side of the Ghats, and Pallamcottah on the eastern side, nearly in the centre of the burning plain of Tinnevelly, and about 13′ north of Trevandrum. The monsoon, which commences in the end of April on the western side of the Ghats, is not felt at Pallamcottah; the annual range of temperature is thus much greater than at Trevandrum. The following are the mean ranges at Pallamcottah, derived from three years' observations by Col. W. H. Horsley, of the Madras Engineers, to whose kindness I owe them.[3] The Trevandrum ranges are placed beside them for comparison:—

	Lat. N.	Long. E.	Ranges.		
			Barometer.	Thermometer.	Bar. range for 1° F.
	° ′	° ′	in.	°	in.
Pallamcottah ...	8 44	77 48	0·241	10·3	0·023
Trevandrum ...	8 31	77 0	0·112	4·8	0·023

It will be seen that the ranges at Pallamcottah are more than double those at Trevandrum. The consequence of this remarkable fact is that, with an equal mean pressure at the two stations, the monthly mean pressure at Pallamcottah must be 0·065 inches greater in January and 0·065 inches less in June than at Trevandrum, and that the oscillations appear independent of the laws of equilibrium of pressure in gases.[1]

That the higher pressure in January at Pallamcottah does not depend on the lowness of the temperature in that month is proved by the fact that at Trevandrum, sixty miles distant, the mean temperature for January is more than 4° F. lower, and the pressure, instead of being higher on this account, is really more than half a tenth of an inch of mercury *lower* than at Pallamcottah for the same level.

The question which is now particularly before us refers to the yearly mean pressures. As the exact height (above sea-level) and index-error of Col. Horsley's barometer are unknown to me, and the direction of mean isobars can be determined with any accuracy only from observations with standard instruments, whose heights above the sea-level have been ascertained by levelling, I have employed for their determination observations made at the three observatories of Trevandrum, Madras, and Bombay, which fulfil the requisite conditions. The following are the mean pressures and mean temperatures at the stations; I have added a near approximation to the mean pressure at Singapore.[2]

		Barometer.		Thermometer.
		in.		
Singapore ...	...	29·904	...	80·3 Fahr.
Trevandrum	...	29·878	...	78·4 ,,
Madras ...	...	29·864	...	83·0 ,,
Bombay ...	...	29·846	...	79·0 ,,

When the latitudes and longitudes given previously are considered, it will be seen that the mean pressure *diminishes* from the equator at the rate of 0·003 inch for each degree of latitude; and that the yearly mean isobars run parallel to the equator (this conclusion is independent of the mean for Singapore). It will be seen also that the yearly mean isotherms do not lie parallel to the equator, their directions being determined chiefly by local conditions.

As the difference of latitudes of Trevandrum and Pallamcottah is only 13′, the difference of yearly mean pressures should, by the preceding result, not exceed 0·001 inches, while the mean temperatures differ by nearly 7° Fahr.[3]

We may conclude, then, that, whatever may be the apparent relation of the annual oscillations of pressure

[1] From means of five years' observations at Singapore and Catherinenburg, six years at Pekin, twenty-six years at Bombay, eight years at Trevandrum, and fourteen years at Madras.

[2] When the twenty-six years' observations at Bombay are placed in two groups, one with the largest, the other with the smallest annual variation of monthly mean temperature, I find the following ranges of the monthly means, derived in each case from the means of thirteen years' observations:—

Ranges of	Temperature.	Pressure.
	°	in.
Maximum	12·6	0·295
Minimum	9·9	0·289
Difference	2·7	0·006

The difference of ranges of pressure for 2°·7 at Bombay at the rate of 0·025 in. for 1° should be 0·067 in. The difference of pressure found is in the right direction, but is not one-eleventh of what it should be if the two were related as cause and effect, or even if the two were exact measures of variation of a single cause.

[3] The approximate height of Pallamcottah above the sea-level is 125 feet; that of the Trevandrum barometer being 193 feet.

[1] I do not enter at present into the consideration of the pressure-oscillation from 10 A.M. to 4 P.M., which appears to show a similar relation to the temperature-oscillation.

[2] The mean pressure at Singapore is corrected by + 0·020 in. for reduction to sea-level, a correction which it is believed is near the truth. The mean temperatures have received no reduction to sea-level at three stations as the heights are small, from 20 to 37 feet. That for Trevandrum has been *increased* by 0°·6 F. (height 193 feet) as an approximate reduction to the height of the other stations. The Madras barometric mean is corrected for index-error by − 0·005. See Introd. Madras Obs. 1851-55.

[3] Since by the annual oscillations 1° Fahr. is equivalent at both stations to 0·023 inches of mercury, if Mr. Chambers' analogy held, the yearly mean pressures should differ by 7 × 0·023 = 0·161 inches. The mean barometric pressure, corrected approximately for height at Pallamcottah, is very nearly the same as at Trevandrum. This is independent of the conclusions from the directions of the isobars.

and temperature, this relation does not hold for the yearly means; that is to say, it does not follow that a high yearly mean pressure should indicate a low yearly mean temperature, and *vice versâ.*

We may, however, arrive at a satisfactory answer to the original question by an examination of the variations of yearly mean temperature at the different stations. It must be remembered that the relation of the oscillations of monthly mean pressure to those of monthly mean temperature are deduced from the variations shown by a thermometer four or five feet from the ground. Does the thermometer at any station, or at a combination of stations, show a high yearly mean temperature with a low yearly mean pressure, and *vice versâ?* I have given the variations of yearly mean temperature (ΔT) at different stations, and it requires only a cursory examination of them to see that there is no such relation. There is, however, another fact of very great importance to be deduced from the values of ΔT, and that is, *the very great constancy of the yearly mean temperature* at all the stations, in spite of the known considerable variations in the amount of rain and of other meteorological results from year to year.

It may be asked how we can explain the facts which seem to relate the annual oscillations of the mean pressure and mean temperature with the independence of the variations of the yearly means. As an illustration we can suppose that with a strong wind and high temperature the height of the tide may be increased at a certain port, while a following north wind with low temperatures will diminish the height at low water; we should in such a case, especially if the temperature varied with the force of the wind, have a larger oscillation of the water with a larger oscillation of temperature: we would not, however, attribute the high tide to the greater heat; we can also conceive that the mean temperature might, in the case supposed, vary, but the level of the ocean would remain constant. Other illustrations might be suggested.

The conclusions at which I have arrived are:—

1. That the years of greatest and least mean barometric pressure are probably the same for all India.
2. Therefore, that the apparent relation to the decennial period found by Mr. C. Chambers for Bombay holds for all India.
3. That the annual oscillations of monthly mean pressure and monthly mean temperature have nearly a constant ratio in India.
4. That these oscillations depend on local conditions in the same latitude, at places quite near each other, which are independent of the heat emitted by the sun.
5. That the yearly mean isobars run parallel to the equator in India and are independent of local conditions.
6. That the directions of the yearly mean isotherms vary with the local conditions.
7. That there is no relation between the variations of yearly mean temperature and yearly mean pressure.

John Allan Broun

THE SIZE OF THE TIGER

In a work on the tiger, published in 1875,[1] I made the following remarks in reference to the size of the animal:—

"The size of the tiger varies: some individuals attain great bulk and weight, though they are shorter than others which are of a slighter and more elongated form. The statements as to the length they attain are conflicting and often exaggerated; errors are apt to arise from measurements taken from the skin after it is stretched, when it may be 10 or 12 inches longer than before removal from the body. *The tiger should be measured from the nose along the spine to the tip of the tail as he lies dead on the spot where he fell before the skin is removed. One that is* 10 *feet by this measurement is large, and the full-grown male does not often exceed this,* though no doubt larger individuals (males) are *occasionally* seen, and I have been informed by Indian sportsmen of reliability that they have seen and killed tigers over 12 feet in length. The full-grown male Indian tiger, therefore, may be said to be from 9 to 12 feet or 12 feet 2 inches, the tigress from 8 to 10 or *perhaps in very rare instances* 11 *feet* in length, the height being from 3 to 3½, or, rarely, 4 feet at the shoulder."

The point I now especially desire to elucidate as it has been the subject of discussion, but is one that has never yet been satisfactorily settled, is the greatest length the tiger attains.

Jerdon and others say that the average size of a full-grown male tiger is from 9 to 9½ feet in length; and Jerdon remarks that he has not seen any authentic account of a tiger that measured more than 10 feet and 2 or 3 inches.

I agree with Jerdon that 9 to 9½ or 10 and 2 or 3 inches are the lengths attained by the majority of tigers met with; but the occasional occurrence of tigers of upwards of 10 feet 2 or 3 inches (the authenticity of which is doubted) is attested by the evidence of several competent and reliable observers, who are quite aware that the measurements should be those of the animal as he lies where he fell, and before being despoiled of his skin, and that measurements of the skin after removal are deceptive.

I have taken some pains to ascertain the views of those who are most likely to be well informed on the subject, and I add the results of my own observations during considerable experience in Bengal, Oude, and Nepal; it would seem that the evidence wanted by Jerdon is forthcoming, and that tigers above 10 feet 3 inches, 11 feet, and even 12 feet, *are* occasionally met with, and *have been accurately measured.*

I may remark that it is very possible that like boars, and other animals, they may differ in size according to locality, food, and other conditions of life; and that such being the case, it is probable that tigers of one province or district may exceed those of another in size. Indeed I am inclined to believe that such is the case, and that therefore those who contend for the larger may be equally right with those who maintain the smaller measurements. I am rather inclined to agree with Mr. C. Shillingford, who suggests the possible progressive degeneration of the tiger; what, certainly, according to some, obtains in the case of stags in the continuously over-shot deer forests of Scotland, may also be going on in the tiger of the much-hunted jungles of India. However this is a mere suggestion, but be it as it may, the inches of the big tiger are, I think, an ascertained fact, for it can hardly be maintained that the authorities who vouch for it are either mistaken or misinformed, or that they do not know how to measure a tiger accurately.

Sir G. Yule, K.C.S.I., Bengal Civil Service, says: "I never had the luck to fall in with a 12-foot tiger; 11 feet odd inches I have killed twice or thrice. I have heard once, at least, of a 12-foot fellow fairly measured, and I cannot see why there should be any doubt as to the occasional occurrence of such exceptions to the general rule."

Col. George Boileau, Bengal army, says he killed a tiger at Mutearah, in Oude, that was well over 12 feet. He writes:—"I can speak positively as to the size of the tiger—his length was well over 12 feet before the skin was removed. He was, of course, quite an exceptional size, and unequalled, so far as my own experience goes, which extended over seventeen years of constant hunting after the species. My own experience of the size of tigers is that, in the female, the size runs from 8 feet to 9½ feet—the latter exceptionally large; in the male, from 9 feet to 11 feet; a well-grown adult tiger is

[1] Royal Tiger of Bengal, pp. 29, 30.

seldom less than 10 feet in length. I speak of hunting-grounds frequented by myself (chiefly Oude and Nepal Terai), for no doubt the size varies according to locality, abundance of food, and its reverse must of course produce their usual results."

Col. Sleeman, Bengal army, says:—"I don't remember having killed a tiger measuring more than 10 feet 6 inches in his skin, but I have seen skins of tigers 11 feet 6 inches in length, and once, at Dinagepore, in Bengal, over 12 feet. I have the skin of the largest tiger I think I ever saw, and it measures 12 feet 2 inches. This tiger was killed near Jubbulpore, in Central India, by an old Thakoor sixty years of age, and I preserve the skin as a trophy of native pluck and vigour in age."

The skins above alluded to were, no doubt, stretched, and therefore do not prove more than that they were taken from large animals, which may have been probably between 10 and 11 feet in length!

Col. J. Macdonald, Bengal army, Revenue Survey, says:—"The largest tiger I have ever measured out of seventy was 10 feet 4 inches, and out of all these only three have touched 10 feet. But I do believe that tigers have exceptionally reached 12 feet." "The skin of a tiger ten feet in length, as he lies dead, would stretch to nearly twelve feet, but after curing it returns to nearly its normal size. I have often measured the distance between a tiger's marks on the ground; average and large animals are from 4 feet 4 inches to 4 feet 8 inches; well! I once found marks 5 feet 10 inches apart, this must have been the mark of a gigantic beast—the breadth of the impression of the fore paw, and the depth of the impression, showed his great size and weight. This was in the Sunderbunds. Mr. M., of Morel-Gunge, told me that once when going through a narrow creek in the Sunderbunds, he saw a stupendous brute, far exceeding in size anything he had before seen in tigers or could have believed possible. The heaviest male tiger I have seen weighed 448 lbs., the lightest, a tigress, 242 lbs."

The Hon. R. Drummond, B.C.S., late Commissioner, Rohilkund, says:—

"I have never seen a 12-foot tiger. The largest I ever shot was 11 feet 9 inches as he lay on the ground immediately he was shot, and before being padded. I measured him because I was struck with his large size."

F. B. Simson, Esq., B.C.S., says:—"I have killed or been at the death of about 180 tigers; I never actually handled one 11 feet long, but I fully believe that they reach that length occasionally, and every now and then a monster is found. The largest skins by far I have seen, came from China. I give you the exact measurements of several I have killed and fairly measured immediately after death, and before they were padded with dates:—

			Tigers' length. Ft.	In.	Height at Shoulder. Ft.	In.
1855	October	15	9	5	3	6
1856	February	13	10	4	3	8
,,	,,	11	10	11	3	7
1858	March	15	9	1	—	
			Tigresses' length.			
1855	October	14	8	8	3	3
,,	,,	13	8	5	3	5
,,	,,	19	8	11	3	5
,,	November	22	8	10	3	3
1856	October	6	9	4	3	10½
1857	February	8	8	10	3	4

"All these were killed on the churs of the Megna, between Backergunge and Noakhally. In later years I killed tigers in Purneah, Docca, Mymensingh, and Assam, but their exact dimensions were not recorded. I do not remember any exceeding generally in size the measurements I have given. I once killed a tiger which stood almost 4 feet at the shoulder.

"I have often been referred to about hogs. I have taken about 900 first spears, and hunted in nearly every zillah in Bengal, but I never speared the boar that would not have walked under a standard of 3 feet 3 inches. This statement has disappointed many; but the facts are at your service, and you may use my name to authenticate them when you choose."

Major-General Sir H. Green, K.C.S.I., C.B., Bombay, says:—"The biggest tiger I was ever at the killing of was in 1848, near Surat, and it measured, *pegged out*, 12 feet 4 inches. I heard by last mail from Claude Clerk at Hyderabad, who said he had just killed, to his own gun, the biggest tiger *he* had ever seen, as it measured 11 feet 6 inches *before* skinning."

Sir H. Green also writes:—"I inclose a letter from Col. Stewart regarding tigers, and I have made many inquiries about them since, and there can be no doubt that a 12-foot tiger is very rare, although I have no doubt there are instances of that size having been exceeded. I find, by reference to my journal, that I have a record of some I have killed, and that the one I mentioned as 12 feet 4 inches, pegged out, measured, *before* skinning, 11 feet 11 inches. Measures before skinning:—

11 feet 11 inches.
10 ,, 11 ,,
9 ,, 9 ,,
9 ,, 6 ,, .—Tigress.
9 ,, 3 ,,
8 ,, 6 ,, .—Tigress; pulled down my elephant."

Col. D. G. Stewart writes:—"I have never seen or heard of a *bona fide* 12-foot tiger, *i.e.*, as he lay in his skin. The largest I ever saw or killed was, as he lay, 11 feet and ¼ inch. I have personally measured eighty tigers or more of my own shooting, and the dimensions I have given are those of the largest of my victims. I saw a skin in San Francisco, of a Chinese tiger, which might have been 12 feet long in life. I never saw anything Indian to approach it. The Chinese skin was fairly treated, had breadth as well as length, the fur was long and soft. The average size of large males in the Central Provinces I found to be 10 feet 6 inches to 10 feet 8 inches; the tail had a good deal to do with the last two or three inches. The largest tigress I killed was, I think, 9 feet 3 or 4 inches, but I speak from memory. Of two males the girth of the fore-arm of one was 48 inches, the average being 32 to 34 inches. One of the most remarkable measurements is that of the tail where it joins the carcass. I have repeatedly found it in males 12 inches."

The Hon. Sir H. Ramsay, K.C.S.I., C.B., Commissioner, Kumaon, writes: "I have always understood that Bengal tigers are larger than ours in the north-west. The largest tiger I ever killed measured 10 feet 5 inches, and I consider anything above 10 feet a large tiger; a tigress very seldom gets beyond 9 feet. I have heard of Bengal tigers measuring 12 feet. G. tells me his father, a Bengal civilian, shot a tiger that measured 12 feet 4 inches, but I never shot in Bengal."

Mr. C. Shillingford, indigo-planter, Purneah (with whom I have shot many tigers) says: "My experience extends over thirty-five years, during which I have shot more than 200 tigers. In 1849 I shot one of the largest tigers I have ever seen, with a party of four. He measured, *as he fell*, 12 feet 4 inches, was very old, and his marks had become faint; the hair was short, like that of a greyhound. I shot another tiger which measured, as he fell 11 feet 10 inches, and another in 1855, 11 feet 4 inches; several of 10 feet 6 inches and 10 feet. The majority of male tigers seldom exceed 10 feet, and many attain only 9 feet 8 inches or 9 feet 10 inches."

Cumming says he has shot a few over 11 feet, and gives three instances—one at Rohinipore, 11 feet 4 inches: one at Kaliastrich in 1865, of 11 feet 2 inches; and another at Gour in 1871. My nephew has also shot one or two over 11 feet.

I think these very large tigers are rare, and are only to be found in the Ganges churs; I am also inclined to believe

that they are degenerating, as I have not shot large ones for several years: or it may be that there is a keener set of sportsmen now-a-days, and no sooner is a tiger heard of than he is shot. The tigresses are seldom over 8 feet, though I have known some that attained 9 feet to 9 feet 6 inches. Cumming says he has seen the claw-marks of a tiger on a tree 18 feet high. The men who are difficult to convince about the large tigers are those who have shot them in hills and rocky places, and those tigers are of a different class and seldom grow large."

Major Bradford, C.S.I., of the Political Service, says: "10 feet 5 inches was the largest tiger I ever saw; but I sent the question to Martin and inclose his reply and the inclosures to it. I remember hearing of the immense tiger White speaks of."

Col. C. Martin, C.I. Horse, says he shot a tiger at Putulghur 10 feet in length, and alludes to a large tiger shot near Goona by Mr. White, which was measured by Mr. Angelo, and is described as follows by the latter gentleman: "I can remember, beyond all doubt, the length was 12 feet 4 inches from tip of nose to tip of tail; 2 feet 2 inches from ear to ear! The direct breadth of wrist 8 inches, spread of foot 10 inches, heel to withers 4 feet, and the tail was 3 feet in length."

These measurements were recorded in the *Delhi Gazette*, but there is some doubt as to their accuracy; so that they may hardly be regarded as proving more than that the tiger was a very large one. Col. Martin says, in a subsequent letter, "W.'s tiger, which I had always thought 12 feet 4 inches, is no longer to be relied on for scientific inquiry, though it probably exceeded 10 feet."

Lieut. James Ferris, B. Army, says: "I have had a good deal of experience, as I have shot in the Central Provinces, and for several years in Oude and Nepal. The largest tiger I know of was shot by Wilkinson, in 1873, in Nepal, he measured 10 feet 4 inches from tip of nose to tip of tail. Wilkinson, who has shot more tigers than most men in India, told me this was the largest he had ever seen; the largest tiger I ever shot myself I got the same season in Nepal; he measured 10 feet 2 inches—he was considered a monster. The tigers in Lower Bengal may be larger, but in the Central Provinces they are certainly smaller; it depends a great deal on how the tiger is measured."

Gen. Ramsay, Bengal Army, says: "The largest tiger I ever saw I shot in conjunction with Col. Stewart, a fine old sportsman, who died many years ago at Benares. The tiger was not found for some days, when he was discovered dying from loss of blood and starvation. The skin was removed, and measured 12 feet from the nose to end of tail." This skin was no doubt stretched. "A tiger of 10 feet 6 inches is a very fair sized tiger. Tigresses seldom grow so large." General Ramsay adds: "My friend Col. H. Shakspeare writes me that 'the two largest tigers he ever killed were, when brought in and measured, 11 feet 8 inches and 11 feet 6 inches respectively—the latter a tigress.' He does not think he has ever seen larger ones. There probably are tigers that measure 12 feet or more, but they would be very rare."

Mr. F. Buckland has kindly given me the following extract from his "Curiosities," 1866, in regard to a tiger shot by Col. Ramsay, who says that he and Major B. shot a tiger at Huldwana, in the Kumaon Terai, that they estimated to be about twelve years of age, and was of the following dimensions:—

	ft.	in.
Length from nose to end of tail	12	0
,, of tail	3	9
Height from heel to shoulder	3	7
Girth of body behind shoulder	5	3
,, forearm	2	10½
,, neck	3	7
From ear to ear	1	6½
Length of upper canines	0	3
,, lower ,,	0	1¾
,, claws	0	3

On referring to some of my own tiger shooting notes I find that I have recorded the following measurements:—

Oude Terai, 1855

		ft.	in.
1.	Tiger	9	5
2.	,,	8	0
3.	Tigress, very large, pregnant with five cubs, measurement not preserved.		
4.	Tiger	10	0
5.	Tigress, large, but measurement lost		
6.	Tiger	9	0
7.	Tigress	8	10
8.	,,	8	11
9.	,,	8	9
10.	Tiger, cub	5	9
11.	,,	9	7
12.	,,	9	11

Oude Terai, 1857

		ft.	in.
13.	Tigress	8	0
14.	Tiger	8	3
15.	Tigress, with three cubs	8	10
16.	Tiger	Lost measurement.	
17.	Tigress	ditto.	
18.	,,	ditto.	
19.	Tiger	ditto.	
20.	Tigress, very large, pulled G.'s elephant down, lost measurement.		
21.	Tiger killed in Hangua (drive) from a tree, very large.		

Maldah, Bengal, 1870

22.	Tigress	Measurements lost.
23.	,,	

Ulwar

24.	Tiger	Measurement lost.

Purneah, Bengal, 1869

		ft.	in.
25.	Tigress	8	2
26.	Tiger	10	0
27.	Tigress	8	7
28.	,,	Measurement lost.	
29.	,,		
30.	,,		
31.	Tiger	10	8

Purneah, 1871

		ft.	in.
32.	Tigress	9	0
33.	,,	7	6
34.	,,	7	8
35.	,,	8	8

Oude and Nepal, 1871

		ft.	in.
36.	Tigress	8	9
37.	Tiger	9	7
38.	Cub	5	5
39.	,,	5	8½
40.	Tigress	8	7
41.	,,	8	6
42.	Tiger	9	6
43.	Tigress	8	8
44.	Tiger	10	6
45.	,,	7	0
46.	Small cub		
47.	Tiger	9	4
48.	Tigress (I killed her with a single bullet in the neck)	8	11
49.	Her three cubs.	6	11
50.		6	5
51.		6	10

These are all the notes I can lay my hands on at present. The largest tiger was 10 feet 8 inches, the largest tigress, 9 feet. Both were Bengal Purneah tigers. My own experience, therefore, confirms so much of Jerdon's estimate as that the tiger averages from 9 to 9½ feet, the tigress between 8 and 9 feet; but that which he and others doubt, viz., that tigers do occasionally attain the length of upwards of 10 feet 3 inches and even 11 feet or 12 feet, and the tigress up to 10 feet or even more, is

conclusively attested by the evidence of the gentlemen whose statements I have quoted.

I am indebted to Sir Dighton Probyn for an interesting letter from Capt. Gerard, of Goona, a high authority on Indian felidæ. He expresses his belief that tigers seldom if ever exceed Jerdon's measurements, a tiger of 10 feet 1 inch being the length of the largest he has ever killed or seen. He refers to examples of large tigers described by various observers, but he doubts the accuracy of the measurements, which he thinks may have been unintentionally exaggerated. His own experience is very large, his accuracy as an observer well known, and his opinion consequently of much value; but it is hardly sufficient to invalidate that of others who are no less competent to note and record facts, and who certainly give a greater length, as the extreme growth of the tiger, than that within which Capt. Gerard limits him. The matter then stands thus: Mr. C. Shillingford, Col. G. Boileau, and Sir C. Reid,[1] vouch for tigers of over 12 feet. The same gentlemen, with Sir H. Green, Sir G. Yule, the Hon. R. Drummond, Col. D. G. Stewart, Mr. Cumming, and Col. Shakespeare vouch for tigers of 11 feet and upwards. The above, with Col. J. Sleeman, Sir Joseph Fayrer, Mr. B. Simson, and the Hon. Sir H. Ramsay vouch for tigers of 10 feet 5 inches and upwards, all from measurements taken before the animals were skinned. Unless these gentlemen, all of whom are accustomed to shoot and measure tigers, were *mistaken*, the question of length may, I think, be regarded as decided beyond dispute.

In conclusion, after thanking sincerely those gentlemen who have given me information derived from their own experience, I would just say that the mere length of a tiger is not necessarily an indication of its real size. The tail is included in the measurement—so tiger hunters have ruled that it shall be—but the tail is a somewhat variable element; in some it is long, in others short, and it is quite possible that a 9-foot 6-inch tiger with a short tail may be heavier, stronger, and larger than a 10-foot tiger with a long tail. No doubt anything over 10 feet is very large, and those of 11 or 12 are rare and exceptional, even though part of their great length may be assigned to an immensely long tail. But I think that, while making all allowances for errors of measurement—which doubtless are not uncommon, though unintentional—there is still sufficient evidence from accurate measurements to show that tigers may exceed 10 feet 3 inches, and that a few—perhaps rare and exceptional instances—do exceed even 11 and 12 feet.

J. FAYRER

THE TELEPHONE, ITS HISTORY AND ITS RECENT IMPROVEMENTS[1]

II.

IN the preceding article we traced the history and development of the magneto-telephone. This instrument, even if it served no other purpose, has given to physicists a galvanoscope of surpassing delicacy. In the columns of this journal (vol. xvii. p. 343) Prof. Forbes showed how the feeblest thermo-electric currents could be detected by its means, whilst the subsequent discovery of the microphone was but another application of the same fact. This latter instrument and the early history of the carbon telephone we now propose to consider.

In the spring of the present year Mr. W. H. Preece startled every one by announcing that Prof. Hughes, the well-known inventor of the type-printing telegraph, had discovered that a couple of bits of charcoal, or a few fragments of metal in loose contact, when in circuit with a telephone and a voltaic cell, were able to reveal the faintest tremor or even to transmit the sound of the voice itself. Universal interest was excited by this discovery; a direct transformation of sonorous vibrations into electricity was supposed to have been discovered, but soon it became apparent that the explanation originally suggested was untenable, and that the true theory of the microphone was to be found in minute variations of current strength. The quivering of the loose fragments produced variable degrees of contact or of pressure, and the marvellous sensitiveness of the magneto-telephone revealed these otherwise inappreciable fluctuations in the resistance of the circuit.

On account of its sensitiveness, the microphone has been suggested as likely to be of use in auscultation.[1] M. du Moncel gives a form of stethoscopic microphone made by M. Ducretet, and shown in Fig. 2. The microphone pencil, C, rests upon a lower plate of carbon, P, which is adapted to a caoutchouc capsule, T, and this again is connected by a flexible tube to a second capsule, T′, which can be applied to different parts of the body of the patient. Although the sensibility of the microphone can to some extent be regulated by the counterpoise, P O, yet still the objection to this apparatus is its over sensibility, for it reveals every noise or tremor, so that it is difficult to distinguish one sound from another. It is not impossible, however, that this or some other arrangement of the microphone may ultimately be found to yield important results in the hands of a physician who has made himself skilled in its use. At the same time we must bear in mind that, after its employment in a surgical case by Sir H. Thompson, the large expectations that were formed of the microphone as an exploring instrument in surgery have not as yet been fulfilled. We are not aware whether the microphone has been tried by seismologists, or by military men to detect the mining operations of an enemy, though we should fear the same causes that operate against it elsewhere might also occur wherever it is employed. The disturbances to which the instrument is subject are most conspicuous when the microphone is used, as it can be, to transmit speech. Nevertheless a particular arrangement, designed by Mr. Hughes (Fig. 3), gives fair results. The two fragments of carbon are shown at C D, the upper one being attached to a light metal arm A B, controlled by a spring R, the tension of which is regulated by a screw *t*. The whole is inclosed in a light wooden box H I G, surrounded by a second box M J L, the end of which is left open. A single carbon only, may even be used, touching the metal arm, as is shown at E. In this case the carbon is supported by a strip of paper gummed to the bottom of the box. Loud as is the articulation transmitted by means of this arrangement, the noisy roar which accompanies it, from tremors picked up by carbons, render many words quite inaudible in the receiving telephone. So far, in fact, the microphone has not proved a practical instrument; it seems, however, likely to become a useful adjunct in physical or physico-chemical researches. In any case science is indebted to Prof. Hughes for first making known an entirely novel, simple, and delicate instrument for the detection of minute mechanical motions.

We say first making known, for some twelve months before Prof. Hughes published his description of the microphone, an arrangement designed by Mr. Edison was singularly like the microphone in its extreme delicacy to the minutest tremor. A couple of inches of silk ribbon rubbed with plumbago and made stiff with gum, was laid upon two metal supports joined in circuit with a telephone and a small battery. Such an arrangement not only de-

[1] Since writing the above I have been informed by General Sir C. Reid, K.C.B., Bengal Army, that he has shot, and measured before the skin was removed, in the Dhoon a tiger of 12 feet 3 inches.

[1] "The Speaking Telephone, Talking Phonograph, and other Novelties," by G. B. Prescott. Illustrated. (New York: Appletons, 1878.)—"Le Téléphone, le Microphone, et le Phonographe," par Le Comte Th. du Moncel. (Hachette, 1878.) Continued from vol. xviii. p. 700.

[1] *Apropos* of the microphone a literary friend sends me the following extract from an ancient Turkish tract containing an exposition of the Moslem creed:—"He (Allah) hears alike the loudest and gentlest sounds and sees all things, even the walking in a dark night of a black ant on a black stone, and hears the treading of its feet, and this without eyes or ears."

tected the slightest vibration but was capable of transmitting speech with remarkable loudness. Although this result has hitherto been unpublished, an application of the same principle was made by Mr. Edison in his "carbon relay," a reprint account of which was published in the *Telegraphic Journal* for July 1, 1877, and the simple form of this relay (Fig. 4) resembles in construction and in principle the so-called hammer and anvil microphone. Edison's carbon rheostat, lately illustrated in our pages, also depends upon the same principle, namely, the compression of a semi-conductor, such as carbon, increasing its conductivity, a diminution of pressure increasing the resistance. It is true that it has been suggested that a rheostat of this kind was made by M. Clérac in 1865, but so far as we have been able to ascertain, no mention is made of varying pressure in the use of Clérac's instrument, but simply the difference in the resistance of columns of plumbago of varying length.

The curious effect of pressure on the electric resistance of semi-conductors was noticed so long ago as 1856 by M. du Moncel, who wrote as follows in the second edition of his "Exposé des Applications de l'Electricité" (vol. i. p. 246) :—"Une chose assez curieuse, et qui paraît être au premier abord en contradiction avec la théorie que l'on s'est faite de l'électricité, c'est que la plus ou moins grande pression exercée entre les pièces de contact des interrupteurs influe considérablement sur l'intensité du courant qui les traverse. Cela tient souvent à ce que les métaux de l'interrupteur ne sont pas toujours dans un état parfait de décapage au point de contact, mais, peut-être aussi à une cause physique encore mal appréciée." Subsequently, in an elaborate memoir on the electric conductivity of bodies, the same distinguished author wrote :—"La pression exercée sur les electrodes joue un grand rôle sur la conductibilité de celui-ci quand il est susceptible d'une certaine compression, comme les bois, les corps mous et pâteux et ceux qui sont réduits à un grand état de division." In 1873 Edison states (Prescott, p. 223) that he independently discovered "the peculiar property which semi-conductors have of varying their resistance with pressure while constructing some rheostats for artificial cables, in which were employed powdered carbon, plumbago, and other materials in glass tubes."

In January, 1877, Edison states that he first applied to *telephonic* purposes the effect of pressure on carbon. It will be remembered that the principle of Gray's telephone was the variations in the resistance of a liquid, proportional to the motions of a diaphragm vibrated by the voice, but that the decomposition of the liquid by the current introduced practical difficulties in the working of this arrangement. Casting about for a means of overcoming this difficulty, Edison recalled his early observations on the electrical properties of carbon, and, encouraged by his preliminary trials, was led onwards step by step, his unwearied efforts being at last crowned by the construction of his carbon telephone, towards the close of last year. In another article we shall trace the evolution and present achievements of the carbon telephone, an instrument which we believe is destined to have a great future.

In conclusion, it is interesting to note that just as Gray and Bell were both led to the discovery of the principle of an articulating telephone through working at the important problem of multiple telegraphy, or the simultaneous transmission of several different signals on one wire, so, too, Edison travelled in the same direction. In Prescott's work he writes: "Some time in or about July, 1875, I began experimenting with a system of multiple telegraphy which had for its basis the transmission of acoustic vibrations." His transmitters were a chord of tuning-forks maintained in vibration by the action of a current; the current not being interrupted at each vibration, but made to vary in strength: his receivers consisted of electro-magnets acting upon iron diaphragms, which closed the end of a series of resonant tubes. In practice difficulties arose, but the system contained two important features.

These were, first, the employment of undulatory electric currents, an electrode attached to one prong of the transmitting fork being made to vary the resistance of the circuit by vibrating in a vessel of water, an idea which Edison claims to have patented as early as 1873; and the second is the use of an iron diaphragm and adjacent electro-magnet as a receiver. Substituting a membrane moved by the voice, the transmitter becomes the same as that patented by Gray in 1876, and the receiving instrument is similar to that used in the magneto-speaking or Bell's telephone. Hence, if these facts are unassailable, and the latter is at present the subject of litigation in America, to Edison is also due the happy inspiration of first using a thin iron diaphragm as a receiver. At the same time Edison, in a recent letter to the present writer, remarks, "Bell had the merit of discovering such a receiver would act as a transmitter," and likewise in

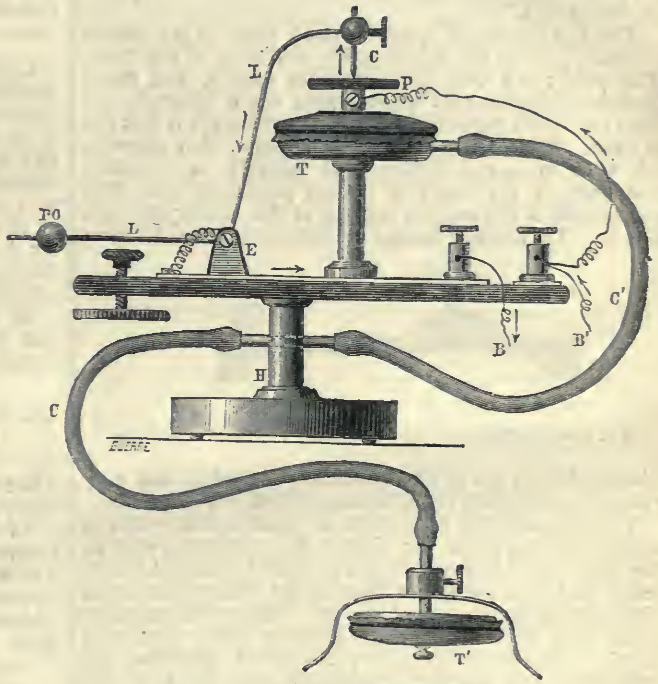

FIG. 2.—Ducretet's stethoscopic microphone.

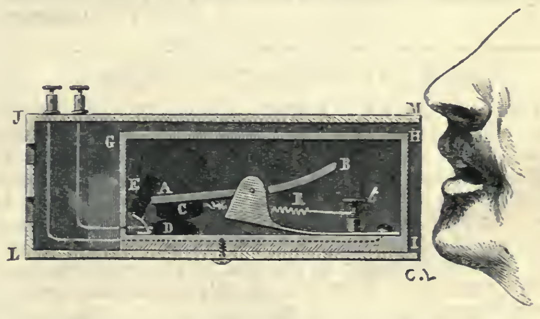

FIG. 3.—Hammer and anvil microphone as arranged for transmitting speech.

Prescott's book, Edison distinctly says: "I can lay no claim to having discovered that conversation could be carried on between one receiver and the other upon the magneto-principle, causing the voice to vibrate the diaphragm. . . . My first attempts at constructing an articulating telephone were made with a Reis transmitter and one of my resonant receivers. My experiments in this direction, which continued until the production of my present carbon telephone, cover many thousand pages of manuscript."

This last incidental remark, which there is no reason to doubt, reveals the indefatigable character of the man. The public see only the successful results, and many doubtless imagine that these spring ready accomplished from the fertile brain of Mr. Edison; the truth is just the reverse. It is a trite, but true observation, that successful work in any direction, and notably in scientific discovery, is the result of patient persistent toil. The public look at the nugget, but not at the labour that has won it. The fields of science are now so well trodden that

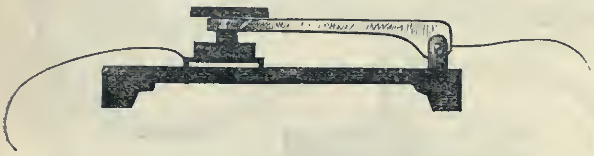

FIG. 4.—Edison's pressure relay resembling one form of microphone.

discoveries are not to be accidentally picked up, but only reward those whose quest is most skilful and diligent.

The extraordinary succession of valuable discoveries in applied science which Mr. Edison has made can only be the offspring of incessant work, profound technical knowledge, and that ready resource under difficulties which characterises a mechanical genius. The conditions under which such a man works are different from those of a purely scientific investigator; the latter publishes his researches and thereby establishes his claim to the priority of the work he has done; the former can publish nothing till the end he has in view is achieved, and the pecuniary benefit accruing from his labours secured by legal processes. And because the reward sought in the two cases is very different, the investigator must often expect to see others reaping the benefit of applications that may be made of his observations, and the inventor ought not to grumble when he finds others claiming credit for work he may previously have done, but for his purpose found it necessary to keep by him unpublished.

W. F. BARRETT

OUR ASTRONOMICAL COLUMN

THE LATE SOLAR ECLIPSE AT WATSON'S STATION.—Prof. Watson made such excellent use of the brief period of totality in the eclipse of July 29, that it will not be without interest to record the circumstances under which he observed. In a communication to M. Mouchez he gives for his position at Separation, Wyoming Territory, latitude 41° 45′ 50″, longitude 2h. 1m. 36s. west of Washington, corresponding to 7h. 9m. 48·1s., west longitude from Greenwich. Prof. Newcomb's corrections to Hansen's place of the moon at this time are − 0·63s. in right ascension, and + 3·3″ in declination; whence if we take 10h. 24m. Greenwich M.T. for a special calculation, we have for the position of the moon, R.A. 8h. 38m. 11·96s., decl. + 19° 5′ 59·3″. Combining this with the sun's place from Leverrier's Tables and the *Nautical Almanac* semi-diameters, there results

Beginning of total eclipse, July 29	...3h. 13m. 32·5	Mean times
Ending	...3h. 16m. 24·0	at Separation.

Thus the duration of totality was 2m. 51·5s.

If for the *Nautical Almanac* values we substitute Leverrier's semi-diameter for sun and deduce the semi-diameter of the moon from her horizontal parallax with Burckhardt's ratio, we find the times of beginning and ending of totality are respectively 3h. 13m. 32·0s. and 3h. 16m. 24·5s., showing a duration of 2m. 52·5s.; we may therefore take 2m. 52s. for the interval which was available to Prof. Watson in his search for intra-mercurial planets. The middle of totality occurred at 3h. 14s. 58·3s. M.T. at Separation, or at 11h. 44m. 41·9s. sidereal time, when the sun's altitude was 44½°, and his hour-angle 46½° W.

CALCULATION OF EXCENTRIC ANOMALIES.—The number of bodies in the minor planet group is now approaching two hundred, yet so far as their orbits have been satisfactorily determined only two or three out of this number have the angle of excentricity, as it is termed, or $\sin^{-1}e$, greater than 20°, which corresponds to $e = 0·342$. More than ten years since Mr. Godward, of the *Nautical Almanac* Office, prepared some tables for the direct computation of the excentric anomaly from the mean to this limit of excentricity. His process is as follows:—

In orbits where the excentricity is not great, M, u, and v being the mean, excentric and true anomalies respectively, and ϕ the angle of excentricity—

$$\tan \tfrac{1}{2} v = \tan^2 (45° + \tfrac{1}{2}\phi) \tan \tfrac{1}{2} M \text{ nearly.}$$

Let M' be an angle such that

$$\tan \tfrac{1}{2} v = \tan\ (45° + \tfrac{1}{2}\phi) \tan \tfrac{1}{2} u$$
$$= \tan^2 (45° + \tfrac{1}{2}\phi) \tan [\tfrac{1}{2} M + \tfrac{1}{2}(M' - M)].$$

Then the Table contains $\tfrac{1}{2}(M' - M)$ for any value of ϕ up to 20°, the arguments being $\tfrac{1}{2} M$ and $\tfrac{1}{2}\phi$.

As an example of the use of this Table, suppose the excentric anomaly of Juno is required for the time to which the elements of the planet are reduced in the Appendix to the *Nautical Almanac* for 1881. The mean anomaly $(= \epsilon - \pi) = 168° \ 39'·43$ and $\tfrac{1}{2}\phi = 7° \ 23'·22$, then

$\tfrac{1}{2} M$	84° 19′·72	
$\tfrac{1}{2}(M' - M)$	— 11·42	from the Table.
$\tfrac{1}{2} M'$	84 8·30	
$\tan \tfrac{1}{2} M'$	0·98858	
$\tan (45° + \tfrac{1}{2}\phi)$	0·11325	
$\tan \tfrac{1}{2} u$	1·10183	
$\tan \tfrac{1}{2} v$	1·21508	
$\tfrac{1}{2} u$	85° 28′·64	
$\tfrac{1}{2} v$	86 30·74	
u	170 57·28	
v	173 1·48	

Here $\tan \tfrac{1}{2} v$ is obtained by adding together the two previous lines, so that there is no subtraction in the operation.

Mr. Godward's Table was printed by the *Nautical Almanac* Office in 1866. It is applicable to all the satellite-orbits showing excentricity, as *Hyperion*, where $\phi = 7° \ 11'$.

THE MINOR PLANETS.—From No. 100 of the *Circular zum Berliner astronomischen Jahrbuch* it appears that the small planet at first announced as No. 190 is proved by Herr Leppig's calculation of its orbit to be identical with No. 94 (*Aurora*); the succeeding discovery therefore takes its number, and for planets found since the beginning of the summer, the numbers, names, and dates will stand thus:—

No. 188	Menippe,	1878,	June 26
,, 189	Phthia,	,,	Sept. 9
,, 190	Ismene,	,,	Sept. 22
,, 191	Kolga,	,,	Sept. 30

Several members of the group as *Dike*, *Medusa*, and others with better determined orbits remain to be virtually rediscovered, and the most interesting of all, from its long period and near approach to the orbit of Jupiter (*Hilda*), was not found at its last opposition. *Atalanta* and *Felicitas* are now nearer the earth than is usual with the minor planets, both being within the mean distance of the earth from the sun; they have the brightness of stars of the tenth magnitude. The following positions are for 12h. M.T. at Berlin, or about 11h. G.M.T.:—

	Atalanta. R.A. h. m. s.	Decl. ° ′	*Felicitas.* R.A. h. m. s.	Decl. ° ′
Nov. 7 ...	1 38 10	+37 50·6	3 11 22	+29 59·1
" 9 ...	1 35 45	37 56·0	3 9 24	30 4·3
" 11 ...	1 33 27	38 0·0	3 7 23	30 8·6
" 13 ...	1 31 18	38 2·8	3 5 23	30 11·9
" 15 ...	1 29 17	38 4·4	3 3 23	30 14·2
" 17 ...	1 27 27	+38 4·9	3 1 24	+30 15·7

GEOGRAPHICAL NOTES

We regret to learn that the Earl of Dufferin will be unable to open the session of the Royal Geographical Society on Monday next, as he has received Her Majesty's commands to attend at Balmoral on that day, but it is hoped that he will be able to preside at the meeting on December 9.

PROF. F. V. HAYDEN, in charge of the Geological Survey of the U.S. Territories, has crossed the Rocky Mountain Divide ten times during the past season. He has explored some of the most noted passes, and among them the celebrated Two-ocean Pass, of which he made a careful chart; an account of this we hope soon to be able to give. We hope also to receive from Prof. Hayden an account of the discovery of recent glaciers in the Wind River Mountains of Wyoming Territory, the first known to exist in the United States east of the Pacific Coast. A fine glacier was observed on the east side of Wind River Peak, and two grand ones on the east side of Fremont's Peak. The latter Dr. Hayden named Upper and Lower Fremont Glaciers. Dr. Hayden took great pleasure in traversing much of the same ground passed over by him in 1860, eighteen years and three months before.

A REUTER'S telegram states that Gen. Severtsoff, the explorer of the Pamir plateau, has returned to St. Petersburg, having visited the unknown districts of Lake Rang-Kul and the Sariz Pamir and Alitchur Pamir plateaux. He reports having found a continuous valley extending from Lake Kara-Kul to the Aksu River. Gen. Sjevertsoff has considerably altered the map of these regions and thrown much light on the geography of the Pamir plateau.

WE are sorry to state that no news from the *Florence* has reached Washington from September 13, the date of the last telegram which Capt. Tyson sent to New York when leaving St. John's, Newfoundland. It is feared that the ship was sunk by the recent heavy gales which raged in this part of the Northern Atlantic a few days after its setting out.

THE London Missionary Society have received intelligence of the arrival of their Tanganyika expedition at Urambo, in Unyamwesi, on July 27; they were to leave that place early in August, and hoped to reach Ujiji by the beginning of September. Mr. Hore in his letter divides his geographical description of the country between Kirasa, forty-five miles east of Mpwapwa, and the capital of Unyamwesi into four sections, each of which furnishes interesting details respecting the region traversed by the party. From Kirasa in S. lat. 6° 42′ 30″, elevation 2,700 feet, to Mpwapwa, lat. 6° 22′, 3,200 feet, they were still in the coast region, the country gradually rising to Mpwapwa along an inclosed plain. As it is approached, the mountains of that range bound the view westward, forming the distinct boundary-line of the maritime region. The waters of the Limbo and of the Mpwapwa stream appear to be mere tricklings left by an immense and irregular flow of water during the rains, which, Mr. Hore suspects, will alter the whole face of the country and reconcile the conflicting accounts we have had about the Gombo Lake. The Chunyo Pass is the back door of the maritime region; a slight descent leads to the plain of the Marenga Mhali, which extends through Ugogo, unless the break of elevated forest and ridge between Kididimo and Nyambwa may be said to divide it into two portions. Assuming this, the first portion, consisting of the Marenga Mhali and Eastern Ugogo, exhibits a similar character throughout, that is, a gently undulating plain, with harsh, thorny, scrubby vegetation and small trees, its monotony broken by small irregular and rugged granite hills. A slightly elevated ridge, with a really beautiful forest, divides the first from the second section of the journey. Descending from the first ridge, the party entered the second section, a flat plain, crusted with a salt deposit, in which tall palm-trees form a new feature. At Mizanga the second section terminated abruptly at a precipitous wall 800 feet high. This wall, or "step," extends north and south, but north of Mizanga it trends away to north-west and west-north-west, which bend the expedition followed, and mounted into the third section or stage of the journey a little beyond Makondoku, the westernmost town of Ugogo. This third section was the vast and elevated forest plateau of Uyanzi and Unyamwesi, extending almost unbroken to nearly the meridian of Unyanyembe. The party here found a comparatively bracing atmosphere, and also reached their highest elevation, 4,400 feet, in the meridian of Jewe-la-Singa. At Uyui (lat. 4° 53′, altitude, 3924 feet) the fourth section was entered, the hills and dales of Unyamwesi, and the country maintained the same character as far as Urambo (lat. 4° 37′ 30″, altitude, 3,815 feet), from which place Mr. Hore wrote that the hills, often little elevated ridges, trend generally north and south, and many of their shoulders had to be crossed. This is the region of the Gombe Nullah. "To the passing traveller," Mr. Hore says, "the driftwood and grass in the trees overhead speak to him of some vast inundation rather than of a stream. The Gombe Nullah is the lowest drain of a vast body of water, whose general direction towards the Malagarasi is indicated by it. . . . This fourth stage has brought us on to the water-shed of the Tanganyika."

THE proceedings of the party which last spring went to New Guinea in the *Colonist* from Australia, have hitherto been shrouded in mystery, though rumours have occasionally reached this country as to their want of success. Recently, however, a leading member of the party has been obliged to go to Cooktown, Queensland, through ill-health, and his report of their proceedings has been furnished to the *Brisbane Courier*. Almost all that has previously been heard of them is that they had formed a camp on the Laloki River. Starting north from this point we learn that they proceeded through open country for eight miles, and struck the Goldie River, where they found the first colours of gold. Twelve miles up this river they crossed and proceeded for two miles in a northerly direction, when they recrossed on finding that the river trended to the east. They then took a north-easterly direction for thirteen miles, partly through dense scrub, and reached what they named the Top Camp, thirty-five miles from the Laloki. They made two journeys up the Goldie, one party going a distance of fifty miles, but found no indications of gold. They saw many villages, some numbering 1,000 inhabitants, and all the natives were friendly. Afterwards the party moved further down the river, and camped near the junction of the Mawmika and the Goldie, the former of which flows

into the latter from the east, about five miles north of the Laloki. From this point a portion of the party travelled eastward, between the Mawmika and the Laloki, towards the Astrolabe range, through open forest country, hoping to reach the table-land seen from the spurs of Mount Owen Stanley in their first journey. After about forty miles they met with magnificent waterfalls (about 150 ft.) on the Laloki, and on the fourth day reached the table-land after great labour. They went along it for twenty miles, when their further advance on horseback was stopped by the scrub. Two of the party remained with the horses, while three others, led by three natives carrying provisions, went through the scrub for thirty miles, and struck the head waters of the Goldie. They then proceeded for six miles in each direction, and found, after their great toil, that they had got entirely away from all indications of gold. They returned to the Laloki camp after an absence of thirty-two days, and since then no exploring work has been done. The members of the expedition still maintain that good gold must exist, and they propose to return to Top Camp, and thence to cross a small range to the north-west, from which the gold found in the Goldie River is supposed to have come; from that point they hope to get to another river supposed to run under Mount Owen Stanley, in the direction of Redscar Bay. The total distance traversed by the party during their three prospecting trips was 370 miles, and throughout the whole of that distance not half a grain of gold was discovered.

EARLY in the summer Mr. J. V. Williams was sent to New Caledonia to inspect the nickel mines there on behalf of an English smelting company, and we learn from an Australian contemporary that he thinks most favourably of their prospects. The mines extend from Noumea northwards, along the coast for 120 miles, and the French authorities, disheartened by the inefficient manner in which mining has hitherto been carried on in the island, are said to be prepared to give an English company all reasonable assistance and encouragement.

FROM a Singapore paper we learn that Baron Overbeck and Mr. Alfred Dent, the promoters of the cession of a portion of Northern Borneo to an English company, to which we referred some few months back, arrived at that port at the end of August, after a visit to the lately-ceded territory. They state that matters are quiet there, and that no disturbances are apprehended. A Ceylon planter who went to report upon the adaptability of the soil of the territory for planting purposes, is of opinion that coffee might be cultivated on the west coast, while on the east coast the sugar-cane and other products which usually flourish in the same soil, would thrive well. Altogether the most sanguine expectations appear to exist as to the productiveness of the country.

THE WERDERMANN ELECTRIC LIGHT

WHILE the world is waiting for the announcement of Mr. Edison's method of splitting up the electric light, Mr. Richard Werdermann, a gentleman well known in connection with electric lighting, seems to have solved the problem, to some extent, at least, and he believes that after further experiments he will be able to divide the current into 50, 100, or even 500 lights. Experimental exhibitions of the light have been given with satisfactory results at the works of the British Telegraph Manufactory, Euston Road.

The chief object of Mr. Werdermann is to demonstrate that a number of lights can be placed in one circuit, the current being produced by an electroplating Gramme machine, having an electromotive force of 4 to $4\frac{1}{2}$ Daniell cells. The principle of Mr. Werdermann's invention is that of keeping a small vertical pencil of carbon in contact with a large disk of the same material. In some earlier experiments he found that when he increased the sectional area of the one carbon and reduced that of the other, he produced an electric light with the carbons in actual contact, a small arc appearing at the point of contact. The small carbon is a pencil 3 mm. in diameter; the upper or negative carbon is a disk of 2 inches diameter and an inch thick. The upper carbon is not consumed, so that the waste takes place only in the lower.

In his lamp he places the disk uppermost with the pencil vertically beneath it, sliding up a metal tube which acts as a guide and contact. The pencil is kept in contact with the disk by means of chains attached to its lower extremity passing over pulleys and down again to a counterweight of about $1\frac{1}{2}$ lb. About $\frac{3}{4}$ in. of the lower carbon appears above the tube, and is rendered incandescent by the passage of the current between it and the disk. This pencil is pointed, and retains this point all the time of burning. It is between this point and the disk that the small electric arc appears which gives the greater part of the light.

At the exhibitions which have been given only ten lights were in circuit at once, Mr. Werdermann having no more lamps at hand. The lights were put in what is called the parallel circuit, that is, they all branched off from one wire of the machine and met again on the other. The lamps were estimated to give a light of forty candles each, and the results obtained were most satisfactory, all the lights burning equally well, giving a beautiful white light, which was perfectly steady. By this system of lighting a large number of lamps could be lighted simultaneously, could be put out, and again re-lit. If one lamp should be extinguished it does not affect the others, except by making them burn slightly brighter; but this effect will be obviated by a switch arrangement for regulating the current of the extinguished lamp. The current produced by the machine being very low in tension, the insulation of the conducting cables could be cheaply and easily maintained.

We may state that two larger lamps were shown of 360 candles each. The effect of the light is not dazzling to the eyes, and it was shown naked; in actual practice a common glass globe, as in the ordinary gas lamps, will be a sufficient protection.

NOTES

WE are requested to state that on the occasion of Prof. Wurtz's Faraday lecture at the Royal Institution on Tuesday next, the visitors' tickets issued by the Chemical Society entitle ladies as well as gentlemen to admittance.

SOME of our readers may be aware that Mr. Alfred Russel Wallace is a candidate for the post of Verderer of Epping Forest. We are sure no one can be better fitted than Mr. Wallace to perform the duties attaching to such an office, and as, so far as we know, no more suitable candidate has appeared, the duty of those who have the filling-up of the appointment is plain.

IT is with regret we announce the death of Mr. Charles R. Thatcher, the well-known conchological collector. It will be remembered that we alluded a few months ago to the number of genera and species of shells lately described, due to the indefatigability of this gentleman, including the unique and wonderful *Thatcheria mirabilis*, described in the *Proceedings* of the Zoological Society by Mr. G. F. Angas, and the *Delphinulopsis Lesourdi*, described by Mr. Bryce-Wright in the *Journal de Conchyliologie*, Paris. Mr. Thatcher started, a few months ago, on a five years' collecting tour, and had made the most perfect arrangements for deep-sea and shallow dredgings. He was attacked suddenly by fever, and died a few days after his arrival at Shanghai. There is no greater loss to conchological science than this gentleman's death, as he was undoubtedly the most successful collector of the day.

PROF. F. V. HAYDEN has recently been elected honorary member of the Société Vaudoise des Sciences Naturelles, Lausanne, Switzerland; Société Malacologique, Brussels, Belgium; and the Geographical Society of Halle, Prussia.

IT is announced that the Sheffield meeting of the British Association will begin, not on August 6, but August 20, 1879.

FROM a Government Minute we have received from Mauritius, we learn that the governor, Sir Arthur Phayre, laid before the council of July 20 an application from the President of the Meteorological Society of Mauritius for a grant of money, in order that the results of the meteorological observations made during many years by Dr. Meldrum, the Government observer, may be published in a form which will be advantageous to science and to navigation, as well as to commerce. The form which it is proposed the publication shall take is twofold, namely: synoptic weather charts for the Indian Ocean, extending over twelve consecutive months, and a storm atlas of the Indian Ocean, exhibiting the principal results which have been obtained respecting the more marked atmospheric disturbances in the Indian Ocean during the last thirty years. The objects are set forth in detail in a report by Dr. Meldrum, of which a copy is annexed to the Minute. The matter was referred to the Finance Committee, who, we trust, will grant the sum asked for. If they have any regard to the interests of navigation and commerce in the Indian Ocean, there can be no doubt as to their decision.

COMMANDER PERRIER has been making some interesting experiments with the Giffard captive balloon for the purpose of comparing the qualities of the best aneroid barometers selected from the Paris Exhibition. Each French maker who obtained a silver medal sent two instruments, which were sent up five or six times, and compared with a standard Fortin barometer placed at the foot of the cable. It was discovered that very few of these barometers recovered their original readings until after the lapse of a considerable time. Some photographs were taken of Paris at this high altitude, and are most interesting, although the motion of the car has caused some want of distinctness in the parts removed from the centre of station.

SOME interesting objects have recently been brought to light from the lake-dwellings in the lake of Neufchatel, and are now exhibited at the Neufchatel Museum. Amongst them are three particularly worthy of notice: (1) a large and extremely well preserved piece of amber; (2) a golden earring of masterly workmanship, of the bronze age; and (3) a canoe cut out from the trunk of a single oak tree in perfect preservation. Its length is 7 metres, its breadth 55 centimetres at the prow, and 65 centimetres at the stern; its depth is 19 centimetres, its total height from 22 to 24, and the thickness of its sides 6 to 8½ centimetres.

NEWS from Panama states that the volcano Cotopaxi is in a state of violent activity. Its crater is surrounded by ice and snow, but the clouds of ashes and smoke rising from it can be seen even at Guayaquil on the shores of the Pacific.

THE *Daily News* Naples Correspondent under date November 2, telegraphs that after numerous variations the activity of the eruption of Vesuvius appeared to be then at its height. The lava flowed into the same ravine into which it fell during the eruption of 1872. The seismographs denoted an approaching increase in the eruption.

TWO fresh shocks of earthquake are reported from Buir (Rhenish Prussia), of which the first was felt for many miles around. It occurred on October 24, at 12.30 A.M., and the second one on the same date at 3.45 P.M.

A VIOLET-COLOURED meteor, with a reddish train, was seen at Stanislas, Austria, on the evening of the 24th in the Great Bear and moving in a northerly direction. It is described as thrice the size of Jupiter.

IN NATURE (vol. xviii. p. 652) we gave an account of a verification of M. Pervouchine's first result (vol. xviii. p. 104). We have now seen a very short and neat verification not only of Pervouchine's first but also of his second result (vol. xviii. p. 456). The author (Mr. John Bridge, M.A.) uses the scale whose radix is 16 instead of the binary scale. He assumes r_n to be the remainder arising from the division of 2^{2^n}, then since $2^{2^{n+1}} = (2^{2^n})^2$, it follows that r_{n+1} is the remainder arising from the division of $(r_n)^2$. Hence the remainders can be successfully calculated. Thus for the first result the divisor is (in scale 16) 1(12)001

$$\therefore r_5 = -5249,\ r_6 = +9(11)4,\ r_7 = -59(10)6,$$
$$r_8 = +(12)5(11)4,\ r_9 = -10(10)(15),\ r_{10} = +1702,$$
$$r_{11} = +(13)(10)(13)6,\ r_{12} = -1.$$

Hence $2^{2^{12}}$ has remainder -1, and therefore $2^{2^{12}}+1$ is divisible $7 \times 2^{14}+1$. The second result is obtained in the same manner, the divisor being $(10)(16)^6+1$, *i.e.*, (10)000001, and the last remainder r_{23}. These verifications have been presented to the London Mathematical Society by the author.

A SUPPLEMENT to No. 37 of the *Boletin de la Institucion libre de Enseñanza* consists of a prospectus giving an account of its aims and of its statutes, which we sketched out in a former notice of the Institution. It also contains a list of officers (among the four honorary professors we notice the names of John Tyndall, de Lóndres, and Charles Darwin, de Lóndres) and of the courses of lectures for the students. We have received, also, a copy of the Vice-Rector's (Montero Rios) address, "Las Elecciones Pontificiales." Among other papers read at the *con. erencias* we note "El Aqua y sus Transformaciones," por D. Francisco Quiroga; "Relaciones entre la Sciencia y el Arte," por D. Federico Rubio; "Teorias Modernas sobre los Funciones Cerebrales," por D. Luis Simano; "La Vida de los Astros," por D. A. G. de Linares. No. 33 has papers on "La Geometria Sintética" (continuation); "Los Principales Publicaciones sobre Plantas Insectivoras" (two by Mr. F. Darwin are noticed), and the catalogue of the "Coleccion de Rocas" in the Natural History Cabinet is proceeded with.

THERE has been opened at Berlin the Telegraphic Museum established by M. Stephan, the General Director of Postal Telegraphs in Germany. The exhibition has been located in two large rooms of the General Post Office at Berlin. This is not the first institution of the kind, a telegraphic museum, and even laboratory, having been established at Tokio by the Japannese Government for the use of the pupils in telegraphic engineering. A number of interesting experiments have been already made in that laboratory under the guidance of Messrs. Ayrton and Perry.

WE have received the *Transactions* of the Norfolk and Norwich Naturalists' Society for 1877-8. The society is now in its tenth year, and is both financially and numerically stronger than at any previous period of its existence, the number of members being nearly 190. A glance at the contents of the part shows that the society is fairly carrying them out. We would particularly call attention to the series of letters on the ornithology of Norfolk, by eminent deceased naturalists, edited by Prof. Newton. The society is making it a feature of its work to rescue all such valuable records from loss. The society is also endeavouring to rescue from oblivion the memory of men who did good work in their time, but are fast being forgotten. The complete meteorological report is also an annual contribution; the observations are recorded by the valuable set of instruments, the property of the Norwich Meteorologica Society. Mr. Stevenson's "Ornithological Notes" are also continued from year to year. The last paper is perhaps the most important: it is part viii. of a carefully compiled fauna

and flora of the county. The subjects of the previous parts have been mammals, reptiles, marine, fresh-water, and land shells, fungi, lepidoptera, flowering plants and ferns, diatomaceæ. Other sections will follow as opportunity occurs. We have also received the *Proceedings* of the Norwich Geological Society, part i., which contains some interesting papers, a creditable Fourth Annual Report of the Lisburn School Association, and a satisfactory Report of the Committee of the Goole Scientific Society, which is in its third year.

WE have received the programmes of the arrangements for the session of the various association societies of Cumberland, of the organisation and activity of which we have had already occasion to speak. The programmes seem to us on the whole very satisfactory.

A FINE statue of Corinthian metal resting upon a marble pedestal has just been found through excavations which are being made at the Ponte Sisto at Rome. The statue is 3 metres high and is slightly damaged; the right arm is entirely broken off; yet it is hoped that the damage is not beyond repair. The general belief among archæologists is that the statue represents the Emperor Probus.

AT Berlin a "Society for Ornithology and the Protection of Birds" has just been formed, under the presidency of Dr. Karl Russ. It consists of about fifty members at present, amongst whom there are numerous eminent ornithologists. The special purposes of the new Society are the discussion and practical testing of ornithological questions in regular meetings, the establishment of frequent ornithological exhibitions on as large a scale as possible, and the delivery of public lectures on the science of birds in all its branches.

M. NICOLLE, the organiser of the Exhibition of Maritime and River Industries, which took place at the Palais de l'Industrie, Champs Elysées, in 1876, has received from the French Government, authority to use the same building in 1879 for an exhibition of science applied to industry. M. Nicolle is now busy making his arrangements for next spring. We understand that a very large place will be devoted to the wonders of electricity. This exhibition is opened to all nations, and scientific exhibitors at the Champ de Mars and the Trocadéro will receive circulars giving details before the close of the exhibition. A sum of 700,000 francs has been voted out of the National subscription for covering the travelling expenses of 5,000 working men to the exhibition. A sum of from 4*l*. to 6*l*., according to distance, has been handed over to each of the chosen delegates, and the railway companies have agreed to take only half of the ordinary fares.

FROM America we have received from the twenty-first to the twenty-eighth annual reports of the New York State Museum of Natural History, edited by the regents of the University of the State of New York. Their contents are of the highest interest, comprising many geological, botanical, and zoological communications of value. Amongst others we note an elaborate paper (with map) on the Niagara and Lower Helderberg groups, their relations and geographical distribution, by Dr. James Hall, the director of the museum; numerous entomological contributions by J. A. Lintner; an account of an ascent of Mount Steward and its barometrical measurement, by Verplanck Colvin; remarks on some peculiar impressions in sandstone of the Chemung group, New York, by Dr. Hall and R. P. Whitfield; notes and observations on the Cohoes Mastodon, by Dr. J. Hall. Each part contains a number of well-executed plates. We may, at a future date, return to some of the contents at greater length. We have also before us the *Transactions* of the Academy of Science of St. Louis (vol. iii. No. 4); the principal contents are a valuable treatise by Prof. G. Seyffarth, entitled "Corrections of the present Theory of the Moon's Motions according to the Classic Eclipses;" a note on the larval characters and habits of the blister beetles, by Charles T. Riley; a synopsis of American firs, by Dr. George Engelmann, and other botanical communications by the same gentleman. Also the following publications of interest for entomologists:—Collecting Butterflies and Moths, by Montagu Browne; Preliminary Studies on the North American Pyralidæ, by A. R. Grote; Butterflies and Moths of North America, by Herman Strecker; Lepidoptera, Rhopaloceres, and Heteroceres, by the same (Nos. 14 and 15).

M. W. DE FONVIELLE writes us that Capt. Howgate has not forgotten the suggestion made by him of utilising for ballooning purposes the magnificent coal seam discovered by our Arctic Expedition on the shores of Lady Franklin Bay. Capt. Howgate has made use of the delays arising from the slowness of Congress in granting the required credit for his Arctic colony, in having experiments made to ascertain whether it is possible to inflate balloons without recurring to the ponderous process of preparing ordinary lighting gas. From a report on the experiments which have been made by Prof. Samuel King in the inflation of the "King Carnival," we learn that great success has been met with. The inflation, we are told, was accomplished successfully throughout in seven hours' time. Gas for the purpose was supplied by what may be termed a five-foot generator of the Lowe pattern or build, which employs the modern method of a steam in conjunction with an air blast. Five charges or turns were required to fill the balloon. The amount of gas produced by each charge or turn approximated 6,000 cubic feet. As will be inferred from the two last statements, the capacity of the balloon is about 30,000 cubic feet. The generator employed could have filled the balloon in less than six hours, and would have done so had not the operations been purposely delayed, which delay was occasioned by a state of weather somewhat unfavourable to the ascension which it was proposed to make. The external dimensions of the generator are, height eleven feet, diameter five feet. It is cylindrical in shape, and has an inside fire-brick lining of about six or seven inches thickness, thus leaving a clear diameter for generating purposes of less than four feet. About ten inches of the height is also taken up by the bottom lining. The gas for the inflation was made from anthracite coal. Steam is passed through the incandescent coal. There comes from the generator an impure hydrogen gas containing carbonic acid and oxide of carbon. The carbonic acid is then removed by a suitable and familiar process, and the carbonic oxide remains with the gas. About forty-six lbs. buoyancy is obtained to every thousand feet of gas. The cylinder of the generator can be made in sections, of cast-iron, if no fire-bricks are used, and of wrought-iron if fire-bricks are used. The sections can be luted with clay and then bolted together. The results of these interesting experiments will be laid before the Geographical Society of Paris and Minister of War of France. M. De Fonvielle inquires whether it is really hopeless to find a liquid which might absorb the largest quantity of carbonic oxide, and restore to the hydrogen gas its natural buoyancy.

IT is stated that a committee is being formed in Paris with a view to a permanent International Exhibition at the Crystal Palace. French exhibitors are invited to transfer their productions from the Champ de Mars to Sydenham, and thus realise the original idea of the Crystal Palace as a cosmopolitan museum and warehouse.

WE have received from Mr. Clifton Ward two papers by him reprinted separately—"Quartz as it Occurs in the Lake District," and "Notes on Archæological Remains in the Lake District."

WE have on our table the following books:—"A Manual of Anthropometry," by Charles Roberts, F.R.C.S. (J. and A. Churchill); "The Art of Scientific Discovery," by G. Gore,

LL.D., F.R.S. (Longmans); "Dogma, Doubt, and Duty," by Charles Hoare (Aston and Mander); "Leisure-Time Studies, Chiefly Biological," by Andrew Wilson, Ph.D., F.R.P.S.E. (Chatto and Windus); "Medicinal Plants," Parts 32 to 35, by Robert Bentley, F.L.S., and Henry Trimen, M.B., F.L.S. (J. and A. Churchill).

THE additions to the Zoological Society's Gardens during the past week include a Toque Monkey (*Macacus pileatus*) from Ceylon, presented by Mrs. Tranchell; two Macaque Monkeys (*Macacus cynomolgus*) from India, presented by Mr. C. Loveless; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. Henry Hands; a Grison (*Galictis vittata*) from South America, presented by Mr. H. Potier; a Common Hedgehog (*Erinaceus europæus*), European, presented by Mr. Edwin Etty Sass; a Common Boa (*Boa constrictor*) from South America, presented by Mr. D. W. Bell; two South American Snakes (*Zamenis hippocrepis*) from South America, presented by Mr. G. H. Hawtayne; two Small-Scaled Mastigures (*Uromastix microlepis*) from Busreh, presented by Capt. Phillips; a Mesopotamian Fallow Deer (*Dama mesopotamica*) from Mesopotamia, deposited; two Japanese Pheasants (*Phasianus versicolor*) from Japan, a Gold Pheasant (*Thaumalea picta*) from China, a Lineated Pheasant (*Euplocamus lineatus*) from Pegu, two Barred-Tailed Pheasants (*Phasianus reevesi*) from North China, a Siamese Pheasant (*Euplocamus prælatus*) from Siam, two Swinhoe's Pheasants (*Euplocamus swinhoii*) from Formosa, a Bewick's Swan (*Cygnus bewickii*) from North Asia, purchased.

ON THE VERTICAL DISTRIBUTION OF THE LIGHT FROM LIGHTHOUSES PLACED AT HIGH ELEVATIONS ABOVE THE SEA-LEVEL

THE strongest beam of rays proceeding from lighthouse apparatus in high towers is sent to the sea horizon, as being the direction in which the light can be seen at the greatest distance in clear weather.

My late brother, Mr. Alan Stevenson, suggested the dipping of dioptric lights below the normal to a plumb line in his Report of December 10, 1839, to the Commissioners of Northern Lighthouses in the following passage:—"A more serious inconvenience in using catadioptric zones is that in very high towers where some correction of the position of the apparatus becomes necessary so as to direct the rays to the horizon, the means of regulating the zones in a manner similar to that used for the mirrors is inapplicable. The adoption of a high point in the flame for the focus of these zones, however, affords a considerable compensation for this defect, and it might even be entirely obviated by constructing each set of zones of the form suited to the known height of each tower and the required range of each light if such a correction were found to be of sufficient importance to warrant its application."[1]

But though the precaution of dipping the strongest of the light to the sea horizon was followed out by Mr. Alan Stevenson in high towers it was not always attended to, till the year 1860, when Mr. J. F. Campbell, the Secretary of the Royal Commission on Lighthouses, brought the subject prominently forward, and suggested the internal mode of adjustment. Since then the strongest beam has been invariably dipped to the horizon.

It must, however, be remembered that when the weather becomes even in the least degree thick or hazy, not to say foggy, the range of the light is greatly curtailed by atmospheric absorption and refraction; which last produces during fogs irregular diffusion of the light in every plane. So that at high towers where the beam is pointed to a very distant horizon, it is obvious that the strongest light is directed to a part of the sea, where it cannot be seen with certainty unless when the weather is exceptionally clear.

In an interesting paper on what he calls the "*horizontal visual penetrability of the atmosphere*," lately published in the *Journal* of the Scottish Meteorological Society,[2] Mr. A. Cruikshanks, M.A., makes the following remarks as to daylight observations:—"This shortening of the range of view arises from various atmospheric causes. Such obstructions to the horizontal view as low clouds or mists, or falling showers of rain, snow, or hail circumscribe it at once or abruptly from the observer at a distance of from a few yards to many miles off. The other great obstruction to the horizontal range of view is haze, which may or may not co-exist with the other obstructions, and is supposed to consist of minute particles of water, dust, and smoke floating in the air. The haze, unlike the abrupt obstructions to the view, apparently gradually increases with the distance from the observer till objects become invisible at the distance of three or four miles up to seventy, &c., miles."

Mr. Cruikshanks also gives the following table showing the mean results of twenty-one years' observations made at the middle of the day on terrestrial objects such as hills and mountains:—

Under 5 miles. Seen on days.	5 miles but under 10 miles. Seen on days.	10 miles but under 20 miles Seen on days.	20 miles but under 30 miles. Seen on days.	30 miles but under 40 miles. Seen on days.	40 miles but under 50 miles. Seen on days.	50 miles. Seen on days.
19	—	—	—	—	—	—
81	81	—	—	—	—	—
41	41	41	—	—	—	—
49	49	49	49	—	—	—
38	38	38	38	38	—	—
48	48	48	48	48	48	—
90	90	90	90	90	90	90
366	347	266	225	176	138	90

From this table it appears that during the twenty-one years there were 366 days per annum when an object under five miles distance could be seen, and only ninety days when an object fifty miles distant could be seen.

It has lately appeared to me that the strongest beam should be dipped lower than my brother proposed, and as is now everywhere adopted.

The best of the light should certainly be directed to the place where the safety of shipping most requires it. Now it may in most cases be laid down as axiomatic that the peril of any vessel is inversely proportional to her distance from the danger, whether that danger be a lee shore or an insulated rock. Confining ourselves to this one view of the subject, it would follow that the strongest of the light should, in hazy states of the atmosphere, be thrown as near to the shore, or the rock, as would admit of vessels keeping clear of the danger. But such a restriction as this would, if permanent, greatly impair the usefulness of the light by unduly curtailing its range in clear states of the atmosphere; and of course, *cæteris paribus*, the farther off a sailor is warned of his approach to the shore the better and safer. Besides, the loss due to atmospheric absorption increases in a geometric ratio, and as the rays diverge in cones from the apparatus, the power of the light is further decreased in the inverse ratio of the squares of the distances from the shore.

It is of course well known that the sun itself is extinguished by fog, and we cannot expect to compete with that luminary. But seeing there are endless variations in the density of fogs and in the transparency of the air when there is no fog properly so called, it always appeared to me that had we an easy way of doing so, we ought to increase *temporarily* the dip of the light, and thus during haze and fogs to direct the strongest beam to a point much nearer the shore than the sea horizon. At present we direct our strongest light not only in clear weather, when it *can* be seen, but also during fogs, when it cannot possibly be seen, to a *part of the sea where the danger to shipping is in most situations the smallest*, and this is done to the detriment of that region where, even when the weather is hazy, there is at least some chance of the light being visible, and to a part of the sea where the danger to shipping is unquestionably the greatest.

The simplest mode of depressing the light temporarily would be to raise the lamp itself in relation to the focal plane of the lens. But this is, for several reasons, very inexpedient. The proper adjustment of the apparatus to the focus, so as to secure

[1] "On the Application of Catadioptric Zones to Lighthouses," by Alan Stevenson, LL.B. (Edinburgh, 1840.)

[2] *Journal* of the Scottish Meteorological Society, new series, vol. v. p. 97.

its being situate in the section of maximum luminosity of the flame, is a somewhat delicate one, and ought, if possible, not to be disturbed oftener than is necessary for changing the lamp. Moreover, while the raising of the lamp would depress the light which passes through the refracting portion of the apparatus it would have precisely the opposite effect upon the portions which pass through the totally reflecting prisms placed above and below the refracting part, which would then throw the rays upwards to the sky, where they would be useless. But any desired change could be effected by surrounding the flame with prisms spheric on their inner faces, and concentric with the foci of the different parts of the apparatus, so as to depress the rays before they fall upon the main apparatus. Those prisms which subtend the lens would have their thicker ends lowest, and those subtending the reflectors would have their thicker ends uppermost.

The great disadvantage, unless in the case of electric lights, of employing the temporary apparatus which has just been described, arises from the loss of light by divergence, due to the relation subsisting between the radius of the flame and the radii of curvature of the apparatus itself.

But this loss may be prevented by another plan. Outside of the apparatus, and either close to it, or what would be more convenient, close to the glazing of the lantern, movable refractors made of panes of plate glass could be placed during fogs. In ordinary states of the weather, these fog screens, which would be hung by chains passing over pulleys fixed to the top of the lantern, would be close to the inside of the parapet wall of the light-room, and below the apparatus. If these refractors were fitted with counter-weights they could, in the course of a few minutes, be hauled up in front of the apparatus by the hand when the thick weather came on, and pulled down again when it became clear. The panes of plate glass which act as refractors, would be of prismoidal section vertically having their thicker ends placed downwards. The vertical angle of the prisms would in each case depend on the height of the light-room above the sea, and the distance off shore to which the strongest beam of light required to be dipped during fog. But after more fully considering the question, I have come to the conclusion that a great improvement could be effected even without resorting to temporary expedients. From a series of observations made with two kinds of photometer by Messrs. Stevenson, in 1865, on the penetrative power of light from a first order lens and cylindric refractor, it appears that for an angle of 0° 30′ in altitude above the plane of maximum intensity, and for 0° 30′ below that plane, the power of the light does not vary more than at greatest from 4 to 6 per cent., and that if the strongest part be sent to the horizon, about one-half of the whole is sent uselessly to the skies.

Power of Lens in the Vertical Plane.

° ′		Means of four sets of observations.
0 40	above the level of maximum	·90
0 30	,, ,, ,,	·94
0 20	,, ,, ,,	·97
0 10	,, ,, ,,	·98
0 0	maximum power	1·00
0 10	below the level of maximum	·99
0 20	,, ,, ,,	·97
0 30	,, ,, ,,	·96

Note.—These results, which are the means of four sets of observations, did not extend further in the vertical plane.

Result of Dipping Light as Proposed, Contrasted with Present System.

Present System. Above horizon. ° ′	Power.	Proposed System. Above horizon. ° ′	Power.
0 40	·90 lost on sky.	0 20	·90 lost on sky.
0 30	·94 ,, ,,	0 10	·94 ,, ,,
0 20	·97 ,, ,,	0 0	·97 on horizon.
0 10	·98 ,, ,,		
0 0	1·00 on horizon.		
Below horizon.		Below horizon.	
0 10	·99 on sea.	0 10	·98 on sea.
0 20	·97 ,,	0 20	1·00 ,,
0 30	·96 ,,	0 30	·99 ,,
		0 40	·97 ,,
		0 50	·96 ,,

Applying these observations, so far as they extend in the vertical plane, to the case of lighthouses elevated much above sea-level, we see that to dip the strongest beam to a point much nearer the shore than the sea horizon, while it would not appreciably affect the visibility there, would even, so far as the observations go, increase the power of the light nearer the shore. Those who have been close to a lighthouse on a hazy night must have noticed the luminous rays passing through the air far above the sea-level, and cases are adduced by Mr. Beazeley of shipwrecks having occurred when the light could not be seen by the sailors, although their vessels were stranded close to the tower. As the lens has the greatest divergence, and is the only agent for giving light near the shore, it only should be dipped so as to throw as few of the rays as possible uselessly on the skies, while the reflecting prisms, which have much less divergence, will remain as at present throwing their rays to the horizon. By this different distribution of the light from the lens and the prisms, although the strongest beams from the lens were dipped 0° 20′ below the horizon, which causes a loss there of 3 per cent. of lens power, yet the loss on the *whole* light coming from both lens and prisms, taken at Mr. Chance's valuation of 70 and 30 respectively, will be reduced to only about 2 per cent., while the sea near the shore will be more powerfully illuminated than at present. It may, however, be fairly questioned whether the strongest beam ought not to be dipped to 0° 30′, as this would still further increase the power near the shore, and would only depreciate the light at the horizon by about 5·8 per cent. It is well to remember that, should the flame, through neglect of the keeper, fall at any time below the standard height, such a defect will operate most injuriously on the light falling near the shore, and not so much on that sent to the horizon. Now there can be no question that in all ordinary cases a vessel with such an offing as twenty miles, which is the sea-range due to 300 feet of elevation, is in a far safer position than if she were within a mile or two of the shore, and hence the propriety of increasing the light near the shore so long as we do not to any appreciable extent reduce it at the horizon.

T. STEVENSON

MEDICAL ENDOWMENTS AT OXFORD

WE have been requested to publish the following details of existing endowments assigned by founders to the study of Medicine and of Human Anatomy and Physiology as bearing on Medicine :—

I. The Regius Professorship of Medicine, as at present constituted, is worth about 500*l.* a year. The items are : (1) from the Queen's exchequer, 35*l.*; (2) as Master of Ewelme Almshouse, 250*l.*; (3) as Aldrichian Professor of Medicine, 126*l.*; (4) examination and graduation fees, 70*l.* to 100*l.*

II. Lord Lichfield's Clinical Professorship, which is not united with the Regius Professorship, is worth 200*l.* a year.

Dr. H. W. Acland holds both the Regius and the Clinical Professorships : no instruction is given by Dr. Acland in either capacity.

III. The Linacre Professorship of Physiology and Anatomy has absorbed the old foundations for the encouragement of human anatomy, namely, the Tomlinsian Prælectorship and the Aldrichian Professorship. It is worth 800*l.* a year, the sum which Merton College pays in place of the original endowment entrusted to it by Thomas Linacre, founder of the College of Physicians, and once a lecturer on medicine in Oxford. The Linacre Professor is engaged in teaching Comparative Anatomy to candidates for the B.A. degree.

IV. A separate Demonstratorship of Anatomy, worth 200*l.* a year, also still exists, and was intended by the commissioners of 1852 to provide for the teaching of human anatomy, as designed by Tomlins and Aldrich. The gentleman who holds this post is Curator of the Museum of Comparative Anatomy and does not teach Human Anatomy.

V. The beautiful old Physic Garden founded by Earl Danby in 1622 is another heirloom of the Medical Faculty of Oxford. The chair of Botany was endowed by Dr. Sherard and the College of Physicians of London elect the professor. By special provision, the clergy are excluded from this professorship, and *preference is to be given to a medical graduate.* The chair is now worth, with later additions, about 400*l.* a year.

VI. Lastly, a very important trust fund is administered by the governing body of Christ Church, the bulk of which was left by Dr. Matthew Lee in 1755 to provide for anatomical teaching in relation to medicine *exclusively*. Dr. Lee's expression of his

intentions is very clear and precise. He assigns, in his will, 100*l*. a year as the salary of a reader in anatomy; 50*l*. for expenses of two bodies and dissection; 30*l*. to a reader in mathematics and physics, and the remainder to be given in annual prizes of 10*l*. to scholars from Westminster School. The trust is now worth 3,400*l*. annually. It is spoken of in the return made by Christ Church to the Commissioners of 1874 as "Dr. Lee's Benefaction for Senior Students in Natural Science." This is not quite accurate: firstly, because Dr. Lee designed the major portion of his benefaction for students in anatomy as bearing on medicine, and not for natural science generally; and, secondly, because Christ Church uses nearly half of Dr. Lee's trust-money to pay classical scholars from Westminster School; whilst the remainder is used to support a most efficient chemical laboratory, and to pay, in part, the salaries of the accomplished chemist, zoologist, and physicist, who are styled "Lee's Readers." No part of Dr. Lee's bequest is now assigned to medical studies, though it should be stated that the present application of Dr. Lee's fund has obtained Parliamentary sanction.

ON THE THERMAL PHENOMENA PRODUCED BY THE PASSAGE OF ELECTRICITY THROUGH RAREFIED GASES[1]

A PORTION of the experiments described in this paper were made before the publication of Wiedemann's experiments on the same subject. The authors state that if they had known of the work of the German physicist they would probably not have undertaken the investigation, but they have continued the experiments and think them worthy of description as the methods employed differ from those of Wiedemann.

The apparatus used consisted of ordinary Geissler tubes, the electrodes being in wide tubes connected by a narrow one. A large Ruhmkorff's coil with a secondary wire 100,000 metres in length was set in action by two large Bunsen cells, and the current was made and broken by a Foucault's interrupter. In order to measure the induced current a reflecting galvanometer was employed, being placed at such a distance from the coil that the effect of the electro-magnet on the needle was very small; this slight deviation was, however, applied as a correction in all the readings. It was first proved that the current induced on completing the primary circuit was incapable of passing through the Geissler tube, for the galvanometer needle was equally deflected whether placed in the secondary circuit or not, indicating that the movement of the needle was due solely to the direct action of the magnet of the coil; on the contrary, when the primary circuit was broken, a considerable deflection of the needle occurred when the galvanometer was in the secondary circuit, and a slight one, but in the opposite direction, when the needle was influenced by the magnetism of the coil alone.

Tubes containing chlorine, carbonic anhydride, and hydrogen, were employed in the experiments, the electrodes being soldered to the wires from the coil by means of Wood's fusible alloy. The narrow part of the tube was placed in a copper cylinder containing water, or preferably mercury, in which a delicate thermometer was immersed, the deflection of the galvanometer was read every thirty seconds, and the thermometer every minute. When the current indicated by the galvanometer was greatest the increase of temperature was most rapid, but the important fact shown by these experiments is that in every case the rise of temperature divided by the deflection is a constant. Unfortunately the different constants are not comparable, as the experiments were not made with the same tube containing the different gases at known pressures, but with different tubes of nearly the same sizes; but the pressures of the gases are not given.

Some experiments were undertaken to determine the heating effects in the neighbourhood of the electrodes. For this purpose the upper end of a Geissler tube containing chlorine was placed in the calorimeter, the latter surrounding the part of the tube containing the platinum wire. When the electrode was negative about eight times more heat was developed than when it was positive. As the quantity of heat produced when the electrode was positive was very small the relation between the deflection of the galvanometer and the rise of temperature was not so regular in the different experiments as in the other case; and when the electrode was negative it was observed that the quantity of heat increased a little more rapidly than the deflection. More accurate results were obtained by immersing the lower end of the tube in the calorimeter; under these circumstances the quantity of heat collected when the electrode was negative was 22·8 times as great as in the experiments in which it was positive, and while the deflection of the galvanometer varied from 100 to 640, the rise of temperature divided by the deflections increased from 100 to 120 only.

It being suspected that the different effects at the two electrodes might be due to a cause similar to the Peltier effect in solid conductors, an attempt was made to discover if the positive electrode is cooled during the passage of the electricity. There appeared to be a very slight diminution of temperature at the commencement of the experiment, but it was soon marked by the conduction of heat from other parts of the tube.

The calorimeter was next placed on the wide part of the Geissler tube, but not surrounding the electrode, which was 16·5 mm. from the calorimeter. In this case, also, a larger quantity of heat was developed near the negative electrode than near the positive, but the ratio was only 4·9. When a portion of the narrow part of the tube was placed in the calorimeter, that near the negative electrode was slightly more heated than the other portion. When a tube containing hydrogen was used similar results were obtained, but the difference between the quantities of heat at the two ends was very much less than in the case of chlorine.

The next series of experiments was made to determine the effect of different diameters of tube. For this purpose a U-tube containing air of the pressure of two mm. was used, one limb of the tube having a sectional area of 36·3 square mm., the other 12·6 square mm., both limbs being surrounded by calorimeters. The quantity of heat developed in the narrow tube was only very slightly greater than in the other, the ratio being about 1·1. By using another tube with the areas of 116·9 square mm. and 4·5 a similar result was found, but in this case the ratio was not greater than 1·2.

[1] By Dr. Naccari and M. Bellati (vol. iv. ser. v., degli *Atti del R. Instituto veneto de Science, Lettere ed Arti*).

NOTES FROM NEW ZEALAND

THE following notes have been sent us from New Zealand by Mr. T. H. Potts, of Ohinitahi:—

Maori Food Feast.—At the great meeting of Kingite natives convened by Tawhaio to meet Sir George Grey and the Hon. Native Minister, amongst other very interesting incidents was the food feast which was held at Hikurangi on May 8.

A procession formed of several hundreds of women, each carrying a neatly woven basket filled with food, proceeded through the village till it arrived opposite to Sir George Grey's tent; at a given signal the baskets were placed on the ground and stacked into a huge heap. The presentation of each article of food was accompanied with an appropriate chant or ngori, with dancing and facial contortions of an extraordinary character, many of the most ancient persons of different hapus taking part in the celebration.

Amongst the various articles of vegetable food in season was offered:—

Pohua.—The root of *Convolvulus sepium*, as flowery as a potato with a slightly bitter taste.

Sowthistle.—*Sonchus oleraceus.* The Hauhaus, when compelled to use cooked sowthistle, found to their surprise they did not lose condition on this spare diet.

Para.—The thick solid scale from the rootstock of the grand fern *Marattia fraxinea.* This edible was pinkish or pale purple when cut, solid, tough, almost tasteless, with a slightly bitter flavour.

Marnaku.—This esculent appeared in thick junks of about a foot in length; it is the mucilaginous pith of the great black tree-fern *Cyathea medullaris.* It was presented ready dressed, was soft, very sweet to the palate.

Roi.—The rhizome or root of the bracken *Pteris aquilina*, var. *esculenta.* It was offered in the uncooked state, in which it is usually kept ready for use.

Tawha.—The prepared berries of a common forest tree *Nesodaphne tawha.*

Hakeke.—The Jew's-ear fungus *Hirneola auricula-Judæ.* It is found in the forests of Pirongia; that which grows on the Karaka is most esteemed.

On Moa Remains, &c.—There has been so warm a controversy as to the probable date of the extinction of the dinornis, that

any reliable fact connected with the remains of the wonderful animals may be of value to the biological student.

On the low hills and flats north-east of the gorge of the Rakaia, from 1854 to 1858, there were quantities of gizzard-stones lying in small heaps on the surface of the ground; for years no one collected them for scientific purposes, but boys or bush-hands sometimes turned over the heaps, and picked out a "few pretty ones" that happened to take their fancy.

In April, 1857, with two friends, I went up the course of the Rakaia, followed the southern stream, then through the country west of Mount Hutt and Mount Somers, returned to the "plains" by the Ashburton or Haketere River; this was then all new country, not taken up. On the southern side of the Ashburton or Haketere River, on the flats above the gorge, a vast number of moa bones lay exposed *on the surface of the soil;* after I had taken up a run there, I used frequently to pick up specimens from amongst these bones and throw them into heaps, with the view of making a selection therefrom at some future time.

It may be worth mentioning that near that spot, at a now well-known place called "Paddle Hill," I found a paddle made of totana (*Podocarpus totana*), with a longer handle and much broader blade than any hoe that I have seen used by natives; it seemed too large for a paddler kneeling or squatting; it had probably been used to propel a moki or raft.

The Pahu.—The Hauhaus at the Hikurangi meeting were called to their place of worship by the beating of the pahu; it is a long, sonorous piece of wood, made (when possible) from an aromatic tree called porokaiwhiria (*Hedycaria dentata*). It is hung from a cross pole supported at either end by a forked stick. The sound may be heard to an extraordinary distance. It is produced from this rough kind of wooden drum being beaten on its edges by several persons furnished with short batons.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Cambridge Syndicate appointed to consider the higher mathematical studies and examinations of the university have issued a further report in which they state that they have been led by the observations of members of the Senate in the Arts School, and by the results of the voting on the scheme of examination recommended in their report of March 29, to believe that in the opinion of the Senate the examination in Part III. of the Mathematical Tripos should be independent of the preceding parts, and also that the Senate would be averse to any scheme in which it was not provided that all the subjects should be included in the examinations of each year. They have framed regulations in accordance with these principles in substitution for those rejected by the Senate on May 29, under which it is provided that from the results of the examination in Parts I. and II., the candidates shall be arranged in order of merit as Wranglers, Senior Optimes, and Junior Optimes. Only Wranglers are to be admitted to the examination in Part III., and from the result of the examination in that part only, the Moderators and Examiners shall publish in three divisions, each division arranged alphabetically, a list of those examined and approved. Power is given to the Moderators and Examiners to place in the first division any candidate who has shown eminent proficiency in the subjects of any one group in Schedule III. In each of the papers in bookwork in Part III. a limit is to be fixed to the number of questions to which any candidate shall be permitted to send in answers, such limit to be printed at the head of each paper. The subjects in Part III. are grouped in four divisions. Group A consists of differential equations, calculus of variations, higher algebra, higher parts of theory of equations, higher analytical geometry (plane and solid), finite differences, higher definite integrals, elliptic functions, theory of chances, including combination of observations. Group B, Laplace's and allied functions, attractions, higher dynamics, Newton's "Principia," book i. sec. ix.–xi., lunar and planetary theories, figure of the Earth, precession and nutation. Group C, hydrodynamics, including waves and tide, sound, physical optics, vibrations of strings and bars, elastic solids. Group D, expression of functions by series or integrals, involving sines and cosines, thermodynamics, conduction of heat, electricity, magnetism. From the discussion which took place on the subject on November 2, opinion seems to be pretty much divided at Cambridge as to the advisability of the proposed alterations.

A POST-MASTERSHIP at Merton College, Oxford, for Natural Science, has been awarded to Mr. Geo. Howson, a pupil of Giggleswick School.

SCIENTIFIC SERIALS

Memorie della Società degli Spettroscopisti Italiani, April, 1878, contains a table showing the solar spots and faculæ for each day of observation for the months of January, February, and March 1877. A note by Prof. Tacchini on the solar spots of the first three months of 1877, compared with those of the same months in 1878.—An account of the observations of solar prominences made at Palermo during the first three months of the present year.—Drawings of the chromosphere for the months of March, April, May, and June, 1871.

May.—This number contains full accounts of the transit of Mercury of May 6, 1878, as observed at Rome, with notes on the same by Respighi, St. Ferrari, Millosevich, and Tacchini. —Drawings of the chromosphere for June and July 1871.

June.—Tables of solar spots and faculæ for April and May, 1878.—Note on the spots and solar eruptions of April, May, and June, 1878, by Prof. Tacchini.—A long paper on a cause for the appearance of bright lines in the solar spectrum, by Mr. Meldola.

July.—A paper containing tables of the solar prominences observed at Palermo in April, May, and June, 1878, by Prof. Tacchini.—A long paper by Schiaparelli on the observations of the rotation and topography of the planet Mars made at Milan during the opposition of 1877.

August.—Spectroscopic observations of the chromosphere made at Palermo during the months of April, May, and June, 1878. Tables showing the number of protuberances and spots on the sun for each day of observation for the months of August, September, October, November, and December, as seen at Rome, with notes thereon.—Drawings of the chromosphere for the last part of July, 1871, made at Palermo.

Bulletin de l'Académie Royale de Belgique, No. 7, 1878.—It has been affirmed by more than one observer that, during auroræ boreales, the intensity of scintillation of stars has been considerably increased; a singular influence, if real. M. Montigny, testing such statements, observed twice such an increase (on the nights of April 5, 1870, and June 1, 1878). He notes the fact that this increase coincided with a lowering of temperature of the air in the locality. In the one case this fall occurred exactly at the moment of the aurora and observation of the scintillation; in the other case it preceded the aurora, but was more marked the night of it, and a few hours after observation of the increase in question. He considers the increase due to the cooling, which must have affected first the upper regions of the atmosphere through which the stars' rays pass; and this agrees with the fact that the stars furthest above the horizon showed the increase most.—MM. Spring and Durand study some obscure points in the composition of oxygenated compounds of nitrogen. Finding that the products of reaction, with water, of the body formed by action of chlorine on nitrite of silver, are exclusively nitric acid and chlorhydric acid, their surmise was verified that the chlorine is substituted for the silver of the nitrite, atom for atom, forming nitric chloride. Hence the structure of the group NO_2 of nitric acid is inferred to be the same as that of its correspondent nitrous acid, and the rational formulæ of these two substances must be written respectively $HO.N = O$ and $HO.O - N = O$. M. Melsens seeks to refute M. du Moncel's statements about the cost of his system of lightning-conductors, as applied to the Hotel de Ville, in Brussels, and criticises the instructions of the Paris Commission for erecting conductors on public buildings. —Some letters in a controversy between M. du Moncel and MM. Navez on the theory of the telephone, appear in this number.—M. Malaise announces the discovery of Brachiopoda of the genus Lingula in the Cambrian formation of Stavelot.

No. 8.—The digestion of albuminoids in some invertebrates forms the subject of a paper here from Dr. Fredericq. From a combination of his results with those got by Hoppe-Seyler and Plateau, it appears that the mechanism of digestion is the same throughout the animal kingdom, and the transformation of food in invertebrates is effected through substances that have the greatest similarity to the digestive ferments of vertebrates (solubility in water, precipitation by alcohol). Digestion by means of

a peptic ferment is very rare among invertebrates; a ferment similar to thrypsine, on the other hand, is met with among different classes of these animals.—M. Plateau communicates an account of experiments (with the graphic method and poisons of the heart), on the movements and innervation of the central organ of circulation in articulate animals. *Inter alia*, section of the cardiac nerve diminishes the number of pulsations (in vertebrates, it produces acceleration).—M. Renard describes the diabase of Challes, near Stavelot, in the Cambrian system.—Dr. Koninck continues his researches on Belgian minerals; and there are some papers on mathematical subjects.

THE *Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg* (vol. xxv. No. 2) contains the following papers of interest:—On the occurrence of the musculus peroneo tibialis in Quadrumana, by Dr. Wenzel Gruber.—On a control barometer, by H. Wild (with plate).—On the reduction of Kirchhoff's spectral observations to wave-lengths, by Dr. B. Hasselberg.—On the observed ingress of Mercury upon the solar disc, at the transit of 1878, May 6, by O. Struve, of Pulkowa (with plate), —Catalogue of forty-two new red stars, by E. Lindemann.—On the Russian species of the mollusc *Clausilia*, Drap., by Dr. Oskar Boettger.—On the theory of curves of the shortest parameter on curved surfaces, by F. Minding.—On the hexylenes resulting from tertiary hexylalcohols and their polymerisation, by L. Jawein.—On the action of tertiary iodide of butyl upon isobutylene in the presence of metallic oxides, by J. Lermontoff. —On tetramethylethyl and its derivates, and on the chemical composition of pinacone, by D. Pawlow.

The Journal of the Russian Chemical and Physical Societies of St. Petersburg (vol. x. No. 6) contains the following papers of interest:—On tetramethylethylene and the chemical structure of pinacone, by M. D. Pavloff.—On the glucose derived from lactose, by M. Foudakovsky.—On the action of bromide of aluminium in the formation of the bromides of aromatic hydrocarbons, by G. Gustavson.—On the dextrogyrate terpene obtained from Russian terpentine, by F. Flavitzky.—On the chemical structure of terpenes, by the same.—On dibenzoyldinitrodiphenol, by M. Goldstein.—On some new mineral springs in the Caucasus, by E. Wroblewsky.—On the influence of the surrounding medium upon electrodynamical actions, by J. Borgmann.—On the determination of the magnetic function of liquids, by the same.

Verhandlungen der naturforschenden Gesellschaft zu Freiburg i. B. (vol. vii. part 2).—From the part we note the following papers: On organic cyanides and their decomposition, by A. Claus.—Note on wine analysis, by the same.—On the equilibrium of a system of expanded molecules and the theory of elastic after effects, by E. Warburg.—Observations on the torsion oscillations of an iron wire heated to redness, by Dr. Messer.—On the sensitiveness of alum crystals towards minute variations in the concentration of their mother-liquor, by F. Klocke.

SOCIETIES AND ACADEMIES

CAMBRIDGE

Philosophical Society, October 21.—Dr. Pearson read a paper on a series of lunar distances taken by him during the years 1875-77, mostly at Cambridge and at a place not far distant, the longitude and latitude of each spot being accurately known. He said that they entirely bore out the conclusions at which he had arrived some time back from a much smaller number of observations, and which were communicated by him to the Society in a paper read by him, March 13, 1876 (see *Proceedings*, ii. pp. 414-418), viz., that the errors are such as cannot be called errors of observation of any kind, and may probably arise from the solution of the spherical problem on which the result depends not being, as at present given, strictly accurate. It was mentioned that there is much to justify this conclusion; for example, this method of obtaining longitudes is not much resorted to now in practice (from which it may be argued that it is actually found inaccurate). It is not formally adopted in Germany, though it still is retained in the *Nautical Almanac*, and in the corresponding publication, the *Connaissance des Temps*, issued at Paris. Capt. Toynbee, F.R.A.S., in a paper in the *Nautical Magazine* for February, 1850 (of which there is an abstract in the *Monthly Notices* of the R.A.S.), distinctly states that lunars taken east of the moon give always a result thirty or forty seconds different from those taken west, though his mean result he says was entirely satisfactory; and until the early part of this century all East Indian longitudes were in error nearly 3m. to the east, a result which very nearly agrees with the errors resulting from these observations, supposing them to have been deduced from the new moon of five to eight days old, probably the most convenient time at which to take them from the sun. The whole series, it was stated, consists of 250 separate distances, each distance being either a mean of three or two, or else only one observation, there being about an equal number of each class, though there is no reason to think that the last are less trustworthy than the others in any serious degree; the Greenwich mean time for each being established, with the exception of a very few, within certainly ten seconds. Only 200 of these, the number at present thoroughly verified, were discussed on the present occasion. Classing these in groups of about forty, it was found that the first group gave thirty-two results where the measured distance was in defect of the theoretical distance, and thirteen in which it was in excess. Assuming the rule given in p. 417 of the paper referred to to be correct, this result exactly agrees with what might be expected, it being almost always most convenient, especially for a beginner, to take lunars, at any rate from the sun, under such circumstances as will give this result, while the example of India, founded apparently on observations made at Madras, seems to imply this probable facility, and also that they were made on the new moon, these being more easily taken in our hemisphere than those made on the old one. In the four remaining groups the proportions are 26 to 18: 28 to 15: 25 to 17: 17 to 14: giving a total of 128 observations in defect, and 77 in excess. Rejecting three or four certainly questionable results, the greatest errors occurring are 2′ 59″ in defect, and 2′ 48″ in excess. The true mean has not yet been ascertained, but is certainly in each case not far from 1′—1′ 20″; which, on an average, will give the observer an error of about half a degree of longitude, or of twenty to thirty-five miles, advancing from our own latitude to the equator. There are probably not a dozen clear exceptions to the rule suggested in the communication of March, 1876, that if the luminaries are both on the same side of the meridian, the observed distance is always in defect of the true if the moon be nearest to it, and in excess if she is farther distant; while the same rule holds good, but with less certainty, when the two luminaries are on different sides. The only exceptions seem to arise where the one more distant from the meridian has a greater altitude than the other, or is of a considerably higher declination, and when the distances are very great, *i.e.* from 120° to 130°, in which case the measured distance seems generally to be slightly in excess of the true; but, as might naturally be expected, these last distances cannot often be taken in our own climate. It was explained that all the reductions had been made by Borda's formula, stated in the *Philosophical Transactions* for 1797 to have been the first strictly mathematical solution of the problem. But the results vary only by a few seconds of arc from those given by the system adopted in the large folio published in 1772 by Mr. Shepherd, Plumian Professor at Cambridge University under the superintendence of the Commissioners of Longitude, and while Dr. Maskelyne was Astronomer-Royal; or from other methods which it is believed are allied to this. Two examples were also exhibited of distances reduced according to the elaborate method suggested by Bessel in the *Astronomische Nachrichten* of 1832; Bessel's results, however, do not differ to any great extent from those obtained otherwise. It was suggested that the problem is really one of spherical trigonometry, and from the fact that the errors seem to depend on the position of the luminaries towards the meridian, whereas the old methods of solution depend on their altitudes, and also that the different ways suggested for eliminating the error due to the difference between the geocentric and geographical latitude of the place of observation give different results, a hope was expressed that if these two circumstances were thoroughly reconsidered in dealing with the question, means might be found of discovering a farther correction of the observed distance, which would give a really accurate result.

MANCHESTER

Literary and Philosophical Society, October 15.—J. P. Joule, F.R.S., &c., president, in the chair.—Relative brightness of the planets Venus and Mercury, by James Nasmyth, C.E., F.R.A.S., Corresponding Member of the Society. "On many occasions, when observing Mercury and Venus in full daylight,

I have always been impressed with the strikingly inferior brightness of Mercury as compared with Venus; and as such a condition is the very reverse of what might be expected by reason of Mercury being so much nearer to the sun than Venus, I awaited the rare event of a very close conjunction of these two planets that occurred on September 26 and 27 last. With the advantage of a perfectly clear sky I had the two planets before me for several hours, so to speak, side by side in the field of the telescope at the same time, thus affording me a most perfect opportunity for making a comparison of their relative brightness. It is difficult to convey in words an exact impression of the difference in the brightness of such objects, but I may attempt to do so by stating that Venus looked like clean silver, while Mercury looked like lead or zinc. Were I to indicate my impressions by way of number I would say that Venus was fully twice as bright as Mercury. So remarkable an inferiority in the brightness of Mercury, notwithstanding his much greater nearness to the sun, appears to me to indicate the existence of some very special and peculiar condition of his surface in respect to his capability of reflecting light—a condition that may be due to the nature of his envelope, if such exist, or of that of his surface, by which the fervid light of the sun's rays falling on him are in a great measure quenched or absorbed so as to leave but a small residue to be reflected from his surface. If this be so, it appears to me to be reasonable to suppose that the absorption of so much light must result in a vast increase in the heat of the surface of Mercury beyond what would have been the case had Mercury possessed the same surface conditions as Venus. Whether in the progress of spectroscopic investigation we shall ever be enabled to detect some evidence of metallic or other vapours or gases clinging to or closely enveloping the surface of Mercury that might in some respect account for so remarkable an absorption of the sun's light, we must be content to await the acquirement of such evidence if it ever be forthcoming. It appears to me, however, to be well to raise such a question, so that our astronomical spectroscopists may be on the outlook for some evidence of the cause of so very remarkable a defective condition in the light-reflecting power of Mercury to which I have thus endeavoured to direct attention."—On the water of Thirlmere, by Harry Grimshaw, F.C.S., and Clifford Grimshaw.

PARIS

Academy of Sciences, October 28.—M. Fizeau in the chair.—M. De la Gourmerie read a note on the works of the late M. Bienaymé.—The following papers were also read:—On the decomposition of hydracids by metals, by M. Berthelot. The heat of formation of gaseous chlorhydric acid from its elements is surpassed by that of all anhydrous chlorides, even chlorides of lead, copper, mercury, and silver; gold is the only exception among ordinary metals. The inference that all these metals, except gold, must decompose chlorhydric gas with liberation of hydrogen, is confirmed by experiment. Platina and palladium, also, their chlorides having low heats of formation and little stability, did not decompose chlorhydric gas up to 550°.—On Vice-Admiral Cloué's "Pilote de Terre Neuve," by M. Faye.—On the state in which carbonic acid exists in the blood and the tissues, by M. Bert. The escape of carbonic acid during the respiratory act requires a dissociation of the super-carbonised salts of the blood. These salts were saturated with carbonic acid neither in the arterial nor the venous blood, nor in the tissues. The life of the anatomical elements can only be maintained in presence of carbonic acid in the state of combination. When the alkalis are saturated, and this gas appears in excess in the state of simple solution it rapidly causes death.—Influence of the nervous system on the phenomena of absorption, by M. Moreau. He attached to the dorsal fins of fishes that had swimming bladders a small glass balloon, lighter than the water, and in a few hours the volume of the fish had diminished through absorption of a part of the air contained in the bladder. When a piece of metal was substituted for the balloon, the volume of the fish increased. There is thus a sensation of thrust upwards or downwards, and it is under influence of the former that absorption of air in the bladder takes place, probably through a reflex action. —On *decipium*, a new metal of samarskite, by M. Delafontaine. In the samarskite of North Carolina he finds yttria, erbine, terbine, philippine (yellow (PpO), equiv. about 90; characteristic absorption band about 449 in λ), decipine (white (DpO), equiv. about 122, band 416); thorine and oxides of didymium and cerium. The equivalents of the metals in some of these earths are shown to present interesting numerical relations. *Decipium* is so called from *decipiens*, deceiving. The didymium of cerite is probably a mixture of several bodies, by M. Delafontaine. This is based on spectral observations.—Reply to a recent communication by M. Hirn, on a gyroscopic apparatus, by M. Gruey.—Classification of double stars, by M. Flammarion. Of the 11,000 double or multiple stars discovered, he finds there are only 819 that give certain indications of a relative motion of the components. These groups are divided into 731 doubles, 73 triples, 12 quadruples, 2 quintuples, and 1 sextuple, in all 1,745 stars variously associated. Of these couples in motion 558 have been found with orbital systems, and 316 whose components have been connected merely by the chance of celestial perspectives and form optical groups. In the orbital systems there is a preponderance of retrograde motion from north to south by west (Several other facts are given.)—On the integration of the equation (1) $Ay'^2 + Byy' + Cy^2 + Dy' + Ey + F = O$, by M. Alexeeff.—On involution in curves of n degree, by M. Serret.—Remarks on two integrals obtained by Lamé in the analytical theory of heat, by M. Escary.—Reply to an observation of M. Boltzmann, by M. Lévy.—On the magnetisation of tubes of steel, by M. Gaugain. When a system formed of two parts having different coercive forces is subjected to action of a weak current, the part having the least coercive force is always that which takes the strongest magnetisation (whichever its position, tube, or core).—On a telephone call, by M. Perrodon. This consists in connecting a Ruhmkorff coil with the plate of the telephone, so as to get a loud continuous sound.—On the transformation of valerylene into terpilene, by M. Bouchardat.—Artificial reproduction of melanochroïte, by M. Meunier. This is by keeping fragments of galena in dilute aqueous solution of bichromate of potash.—On the elimination of salicylate of soda, and the action of this salt on the heart, by MM. Blanchier and Bochefontaine. It stimulates various secretions, notably the salivary. In man it is at once expelled by the kidneys (appearing in the urine in 20 mm.); in the dog it appears both in the urine and the saliva, also in the bile and pancreatic fluid. The hypersecretion of saliva is due to action of the salicylate on the grey substance of the central nervous system. In strong doses, the salt stops the heart in diastole.—On parthenogenesis in bees, by M. Sanson.

CONTENTS

THURSDAY, NOVEMBER 14, 1878

CLEOPATRA'S NEEDLE AND THE WIND PRESSURE

THE statements recently made in the *Times* respecting the stability of Cleopatra's needle and the maximum intensity of the pressure of the wind in this country have awakened much interest, if not anxiety, about the subject. The appearance of the lofty obelisk balanced on so small a base suggests to many the thought of an egg standing on its end, and presents every idea of instability. This idea is much amplified by a very erroneous estimation, we believe, by most persons of the real dimensions of the base; we have heard this estimated at various diameters down to two feet, but in reality it is in no direction less than five. The statement that the stability of the obelisk is sufficient to withstand a wind pressure of 80 or 90 lbs. per square foot having been made, the storm from Liverpool at once broke on it and upset people's minds, if not the monolith. Thus we learn, from the observations taken by Mr. Hartnup, the astronomer at the Liverpool Observatory, that on January 30, 1868, "it began to blow strongly about 9 A.M., and from that time gradually increased in violence until half-past 11 P.M. on the 31st, when there was one gust of wind which registered 51 lbs. on the square foot. From this time the gale rapidly increased till noon next day, blowing with a severity quite unprecedented in this country. The anemometer which has been erected at the Bidston Observatory is made to register up to 60 lbs. on the square foot, the idea being that no gale would reach that degree of violence. Between eleven o'clock and one o'clock, however, the registering pencil was driven far beyond this limit, and Mr. Hartnup calculated that at several periods the pressure could not have been less than from 70 lbs. to 80 lbs. on the square foot. The anemometer was erected in 1851, and the most severe gale registered up to this time was in December, 1863, when there were three gusts which registered 45 lbs. to the square foot." Further details respecting this remarkable hurricane will be found in the *Journal* of the Scottish Meteorological Society, from which we find that at Glasgow, from 1.15 P.M. to 1.30 P.M., twenty-one miles of wind passed the observatory, giving a velocity of eighty-four miles per hour, or corresponding to a pressure of $35\frac{1}{2}$ lbs. to the square foot, while the strongest gusts registered 42 lbs. on the square foot. At Edinburgh the gale was more severe than at the latter place; cabs and horses are said to have been blown over, but there is no record of the pressure or velocity as there was unfortunately no anemometer in working order. Many authorities state that the maximum pressure of the wind does not exceed 55 lbs. to the square foot in this country, and as this is the figure commonly assumed by engineers in the design of large structures, it is of the greatest importance that the trustworthiness of the Bidston anemometers should be ascertained. Pressure anemometers are obviously liable to errors from the varying modulus of elasticity of their springs and the momentum of their moving parts and supports, while Robinson's anemometers may give a maximum velocity due to small eddies, which is much in excess of the true value. Mr. John Dixon in his letter to the *Times* on the subject gives a good illustration of a pressure of 80 lbs. to the square foot by comparing it to the weight on the floor of a densely-crowded room. It has been ascertained by experiment that the weight of a crowd of persons can attain 80 to 120 lbs. per square foot, the latter figure being reached only when the experiment was made with labourers of above the average stature packed as closely as possible, and the former being commonly taken as the maximum load to which the platform of a bridge can be subjected by a dense mixed crowd. Thus Mr. Dixon remarks, "the windows of a building certainly have to bear an equal strain with the walls, and I suppose it would be immaterial to the glass whether it was placed vertically or horizontally. A densely packed crowd hardly weighs 80 lbs. per square foot of the space it stands upon. Reduce therefore the theory to common sense; would any one dream of standing on a floor formed of glazed window sashes?" On the whole we rather think not, even if, to make the case analogous, means were taken to distribute the pressure uniformly, and we are forced to the conclusion that either the Bidston Observatory is a very strongly constructed building with window-sashes and glass of unusual strength, or that the anemometers are untrustworthy.

Leaving now the question of the maximum pressure of the wind to be decided by meteorologists, there remains to be ascertained what that pressure would have to reach on the banks of the Thames to endanger the existence of the obelisk. Mr. Dixon's assurance has probably set the fears of many at rest; he says: "As to its stability there need be no fear—130 lbs. of wind-pressure would not upset it. The columns of the *Times* are not the place to ventilate calculations and figures." We can assure Mr. Dixon that these calculations would be of sufficient interest to the readers of NATURE to find a place in its columns, but in their absence we are obliged to fall back on our own. The widths of the top and bottom of larger face of the obelisk are respectively 64 inches and 95 inches, the height being 60 feet 6 inches exclusive of the pyramidal point, which would be 7 feet high if intact; assuming, then, an additional foot of height for the lower rounded end, the moment about the base of the pressure on the area of the larger face will be 12,931 foot pounds for a wind pressure of 1 lb. on the square foot. The weight of the stone is estimated at 196 tons, whence, on the assumption of perfect rigidity, the ultimate stability would be 196 tons × radius of base (2·5), and the corresponding wind pressure $= \frac{196 \times 2\cdot5 \times 2240}{12,931} = 84\cdot88$ lbs. per square foot. But the material of the obelisk not being perfectly rigid, it will be seen that this ultimate stability could not be reached. The effect of the wind-pressure is to cause a deviation of the line of action of the resultant pressure on the base from its centre with a diminution of the stress on the windward, and an increase of that on the lee-side of the base; if the decrease exceeds the normal pressure due to the weight the joint will tend to open, while if the increase is carried too far it may reach the crushing strength of the material.

Both these effects have to be considered. Now in the design of masonry work of a substantial character it is

the usual engineering practice to so distribute the stresses that no joint tends to open under the most unfavourable conditions, though this condition is doubtless frequently neglected in flimsy structures. In order that this condition should be fulfilled, the resultant of the pressure on the base must not deviate from the centre of gravity of the base by a quantity greater than x' given in the equation $x' = \frac{I}{XS}$, where I is the moment of inertia of the base about the neutral axis or line through its centre of gravity perpendicular to the direction of the deviation of the resultant, S = the area of the base, and X = the greatest distance of a point in the base from the neutral axis on the side of the greatest pressure. In the case of a circular base $x' = \frac{\text{diameter of base}}{8} = {}$·625 feet in the present instance. The wind pressure corresponding to this deviation $= \frac{196 \times {}^{\cdot}625 \times 2240}{12{,}931} = 21{}^{\cdot}22$ lbs. per square foot. When the wind-pressure exceeds this amount there is still the tensile strength of the cement with which the stone is bedded to resist the tendency of the joint to open on the windward side. While the introduction of a layer of cement under the stone doubtless adds to its steadiness under a wind-pressure of 30 or 50 lbs. to the square foot, it would add a very serious element of danger should the pressure ever approach that recorded at the Bidston Observatory, as the cement on the lee side would probably then be subjected to a crushing stress in excess of its strength, and by giving way would cause the column to heel over to some extent; in fact, if there was any probability of that wind-pressure being reached, it would have been safer to have omitted the cement and trusted for the ultimate stability to the far greater resistance to crushing of the granite. It would be impossible, without making assumptions unfounded on experiment, to estimate with any accuracy the value of the additional stability given by the cement in the case of moderate wind-pressures. We have, however, calculated the conditions of equilibrium, neglecting the tensile strength of the cement, as well as the bending of the stone.

On this assumption we find that a wind pressure of 50lbs, per square foot would cause the joint to open on the windward side as far into the base as the centre; the column would thus be standing only on the leeward half of its base, but the stability would not be endangered by this as the maximum pressure on the base at its outer edge would only amount to 40 tons per square foot, which is less than the crushing strength even of the cement. The line of the resultant pressure on the base would be at a distance of 1·472 feet from the centre, if the bending of the column is disregarded. To take into consideration the flexure of the column would involve too long calculations for our present purpose, even if the modulus of elasticity of granite had been determined with sufficient accuracy to make the results of any value, but this we believe has not yet been done. The conclusions we arrive at are as follows:—As long as the foundations remain secure, the obelisk may be frequently subjected to a wind pressure of 21 lbs. per square foot without the slightest tendency to accident; if subjected at long intervals to a pressure of 40 or 50 lbs. to the square foot, it would probably stand for an indefinitely long period until the fatigue of the cement under variations of stress or its natural decay, if that ever takes place, causes its rupture, but under a pressure of this intensity it must be borne in mind that considerable oscillation would take place, and that if the period of the gusts nearly agreed with the time of vibration of the stone it might be overturned; while if a pressure of 80 lbs. per square foot is reached it is very questionable if the survivors among the inhabitants of the neighbourhood will find it *in situ* when they have time to go and look for it.

DRAPER'S SCIENTIFIC MEMOIRS

Scientific Memoirs: being Experimental Contributions to a Knowledge of Radiant Energy. By John William Draper, M.D., LL.D. (London: Sampson, Low, and Co. New York: Harper Brothers, 1878.)

THE scientific world is to be congratulated on the accession to its literature of these memoirs constituting as they do a distinct historical sketch of the works of a physicist who is at once an ardent experimentalist and a careful theorist. As he remarks in his preface, many of his results of experimental investigation on scientific topics have been largely disseminated in European languages, and many of the conclusions they have presented have been admitted into the accepted body of scientific knowledge. The papers in which these results were published have, however, appeared from time to time in various American and English periodicals, but we now have them collected in a form in which they are accessible and convenient for reference.

The four opening memoirs seemingly occupy their position in the volume for the purpose of calling the attention of the reader to the fact that a large portion of the subject that Kirchhoff treated mathematically in a paper which appeared in *Poggendorff's Annalen* in 1860, and which at the time was considered the foundation of spectrum analysis, had already been experimentally proved and published by our author some thirteen years before. The theorist apparently ignored the work of the experimentalist, and the claim of the one to priority in regard to the enunciation of certain fundamental principles of spectrum analysis is now on the best of evidence disputed by the other. The titles of these first four memoirs and their dates of original publication will give an idea of the indictment framed against Kirchhoff which appears in a note appended to the last of them. They are—

I. Examination of the radiations of red-hot bodies. The production of light by heat, published in 1847.

II. Spectrum analysis of flames. Production of light by chemical action, published in 1848.

III. On invisible fixed lines in the sun's spectrum detected by photography, published in 1843.

IV. On the nature of flame, and on the condition of the sun's surface, published in 1858.

Controversy regarding priority of discovery is always distasteful, and the indictment against Kirchhoff is a heavy one, but the offence might have been charged also against those scientific writers who, careless of history, have been accomplices in doing Draper an injustice. But turning to the more agreeable side of the subject of these memoirs we find that Draper fixed the temperature at which solid

bodies emit light with heat to be 977°, and shows experimentally that as the temperature of an incandescent body rises it emits rays of an increasing refrangibility; also that the amplitude of any particular vibration increases with the temperature, and that to every particular wave-length there belongs a particular colour. But even more remarkable are the deductions he makes regarding light and heat, deductions which, though evident *now*, perhaps, in the present state of knowledge, had by no means *then* the appearance of undoubted truths. He boldly asserted that light and heat were effects of radiation and not forces existing in the radiations themselves.

It is, however, with photographic research that the name of Draper is most generally linked; and as his researches in this line commenced in 1837, two years before the announcement of Daguerre's and Fox Talbot's discoveries, his claim to be considered one of the pioneers in photography admits of no contravention. In his memoir on "Studies in the Diffraction Spectrum" we read: "Several years before the commencement of the discovery of photography by Daguerre and Talbot (1839), I had made use of the process for the purpose of ascertaining whether the so-called chemical rays exhibited interference, and in 1837 published the results in the *Journal* of the Franklin Institute, Philadelphia (July, 1837, p. 45). In this, as will be seen by consulting that publication, I was successful." In his memoir of 1843, he describes the mode in which he photographed the spectrum, from the blue to the ultra-violet, and from near C in the red region to a point some distance below the limit of visibility. The apparatus he employed would at the present time be considered, perhaps, somewhat rude, but, as is well known, the roughest appliances in the hands of a true philosopher are sufficient even for delicate experiment. Thus, in photographing his spectrum we find that he worked before the days of collimating lenses, and with a consequent feebleness of light which was a serious matter when the slow (as compared with that now extant) process of Daguerre was employed for registering the impressions of the radiations. Half an hour's exposure was not too long to give to obtain a developable image, whereas now as many seconds as he gave minutes, with the same size of spectrum and width of slit, would be more than ample. The method by which Draper registered the lines in the red and ultra-red regions is fully treated of in his fifth memoir. The plate received a preliminary exposure to white light, and was then exposed to spectrum; or feeble daylight was allowed access to the plate whilst being similarly exposed; the result, on development by mercury, being that the dark lines in these regions were registered as light lines on a dark background, instead of as dark lines on a white background. This action Draper, Claudet, and others ascribed to the antagonism of the blue and red rays which are found in white light, and heads his memoir "Interference of radiations" in consequence. Till last year this view of the antagonism of rays was accepted as existent, when it received a blow, and probably a final one, from the announcement of the experimental proof that this action was produced through the spectrum possessing the power of accelerating the oxidation of the compound of silver which had been altered by light, and which, when so changed became undevelopable. Whatever may be the explanation of this phenomenon, we have in Draper's photographs of the least refrangible region a gigantic feat, considering the date at which it was performed. Though recent methods may outstrip the more antiquated one as regards rapidity of execution, yet it is due to him to acknowledge that he has long priority in showing that chemical action was not confined to the least refrangible end of the spectrum. As regards the application of photography to portraiture, to our author seems to belong the honour of having taken the first portrait by the Daguerreotype process, and the arrangements adopted for the purpose read rather comically in these days of quasi-instantaneous pictures. In his memoir, "On Taking of Portraits by Photography," he says:—"On a bright day, and with a sensitive plate, portraits can be obtained in the course of five or seven minutes in the diffused daylight. . . . Difficult parts of the dress . . . require intervals (exposure) differing considerably, to be fairly copied, the white parts of a costume passing on to solarisation before the yellow or black parts have made any decisive representation. We have therefore to make use of temporary expedients. A person dressed in a black coat and open waistcoat of the same colour must put on a temporary front of a drab or flesh colour, or, by the time that his face and the dark shadows of woollen clothing are evolved, his shirt will be blue, or even black, with a white halo around it." We are sure that the author cannot have regretted the supercession of a process which entailed such "dodging" to render a portrait practicable, more particularly at the time when he sat for the photograph from which the admirable portrait forming the frontispiece was engraved.

To this same memoir we have also an append in which it is shown that Dr. Draper had priority in taking a photograph of the moon; and when it is considered that the exposure was twenty minutes, and the diameter of the image about one inch, it would not be surprising had it lacked in detail. By an extract from the minutes of the New York Lyceum of Natural History we learn that in this photograph we have "a distinct representation of the moon's surface."

To yet another discovery of Draper's we must refer, since, like some others of his, it has been re-discovered quite recently. He says, in his description of the Daguerreotype process, "On these principles" (he alludes to the different photographic effects produced by different rays of light) "it is plain that an achromatic object-glass is by no means essential for the production of fine photographs; for if the plate be withdrawn at a certain period when the rays that have a maximum energy have just completed their action, those that are more dispersed but of slower effect will not have had time to leave any stain. We work, in fact, with a temporary monochromatic light." With a cigar-box as camera and a spectacle-lens as an objective he tested his theory, and found that on this principle he could photograph an engraving, with all its finest details present. The similarity between Janssen's use of an uncorrected lens for solar work and this is apparent.

Mixed up with photography is actinometry, and here we find that Draper not only invented the chlor-hydrogen photometer, which depends on the combination of chlo-

rine and hydrogen when acted upon by radiations, but that he also used it practically, though not with such nicety of method as subsequently employed by Bunsen and Roscoe. He also invented the ferric oxalate photometer, dependent on the reduction of this ferric compound to the ferrous state and the liberation of carbonic acid. In both of the foregoing we have a measure of the *quantity* of the radiations which these mixed gases, or solution, select. On this particular subject of selective absorption, when chemical action takes place, Draper experimented most fully. He showed, for instance, that the sensitiveness of the surface of a Daguerreotype plate is at its maximum when of a yellow tint, owing to the absorption of the blue rays, and conclusively shows that when it is of a blue tint that these same rays are largely reflected. In fact, he announced, with all the authority of a successful experimentalist, that for the production of chemical action in a compound by any particular ray, the absorption of that ray by the compound was an absolute necessity. In late years we have had several rediscoveries of this important truth, and probably it will be rediscovered again and again, notwithstanding the publication of these memoirs.

We have not space to do more than to mention the memoirs on the "Distribution of Heat in the Spectrum," on "The Chemical Force in the Spectrum" (both titles of which, by the by, are inexact, as Draper himself was the first to prove), and on "The Supposed Magnetic Effects Produced by the Violet Rays," all of which are important contributions to science, as are also those on "The Cause of the Flow of Sap in Plants, and the Circulation of the Blood in Animals," and "On the Decomposition of Carbonic Acid Gas by Plants in the Prismatic Spectrum." All these have been treated in a masterly manner, and the results lucidly and tritely recorded. Reading these memoirs leads us to the conclusion that we have in Draper a successful experimenter, who has been perhaps too little appreciated in the world owing to his too great modesty in neglecting to call attention to the facts he has observed, and to claim for himself honour where the honour was due. Like other men of mark in science, the ardent pursuit of it was undertaken through what might be termed an accident. He tells us in his preface that happening to see a glass containing some camphor, portions of which had been caused to condense in very beautiful crystals, he was induced to read everything he could obtain respecting the chemical and mechanical influence of light, adhesion, and capillary attraction; the experiments he made in connection with these subjects being contained in the volume before us. His thoughts being thus directed to physiological studies, he published papers on these topics in the *American Journal of Medical Sciences*, which created such a favourable impression that he was appointed, in 1836, Professor of Chemistry and Physiology in Hampden Sidney College, Virginia. He afterwards was appointed to a similar chair at New York University, which, we believe, he at present holds.

It would be travelling out of our province to do more than call attention to Dr. Draper as the author of "A Treatise on Human Physiology," "The History of the Intellectual Development of Europe," "The History of the American Civil War," and of "The History of the Conflict of Religion and Science," works which have met with well-merited success, and which show the varied bent of his mind.

The history of the Rumford medal fund held in trust by the Royal Society, and the awards made by this body are too well known to need repetition; but it is not equally well known that a similar medal fund was founded in the United States by Rumford, and is held in trust by the American Academy of Arts and Sciences. The medals were to be awarded for "the most important discovery or improvement relating to light and heat that had been made during the preceding two years in any part of America." The awards of the American Rumford medals have been made few and far between, and till 1876 may be said to have been given for inventions rather than discoveries. At this date the medal was awarded to Dr. Draper (as the medal itself records) "for researches on radiant energy." Had he been an European there can be little doubt but that he would have received one of our English medals years ago, and that his name would have been in the same list with those of Leslie, Fox Talbot, Fresnel, and Faraday. As it is he has the honour of being the first recipient of the American Rumford medal which has ever been awarded for pure scientific research.

A CATECHISM OF BOTANY

A First Catechism of Botany. By John Gibbs, of the Essex and Chelmsford Museum. (Chelmsford: Edmund Durrant and Co. London: Simpkin, Marshall, and Co.)

THIS little book is in its way quite a curiosity. It is a survival of a method of instruction which was very popular in its day, but which it is to be hoped—notwithstanding that Magnall's "Questions" is still said to be a good property—even in country towns like Chelmsford, is on the road to extinction. Catechisms originated in the necessity of giving some uniformity and precision to oral religious instruction. Their great merit is of course that they remove all responsibility from the teacher, and merely require that their formulæ should be taught with patience and perseverance. They render unnecessary, indeed even undesirable, any knowledge of the subject on the part of the teacher, and hence it is easy to see the reason of their popularity amongst persons engaged in education, and who, possessed of no scientific training, are yet anxious to get credit for teaching scientific subjects. Mr. Gibbs has evidently felt some uneasiness on this head, and points out accordingly in his preface that:—

"The answer to every question may be verified by examination of the plant itself in all its parts to which reference is made. Only in such a way can this catechism be made useful, and by such criticism its value will be ascertained."

But the insidious influence of the purely dogmatic method makes itself but too evident in the next sentence, which is surely the strangest ground of recommendation ever urged for a scientific book:—

"In its original form it was admitted to the International Exhibition of 1871, which contained nothing but what the Committee of Selection approved as excellent."

The approval of the Committee must have been easily given if it extended to the following curious inquiry :—

"83. *In the common horse-tail of our ditches every internode of the stem seems to consist of two hollow tubes, one inside the other. How do these differ from the wood and bark of plants more highly organised?*"

Another extract will show the catechetical method in its most aggravated form. Imagine the children in some small country school bitterly endeavouring to commit to heart the following :—

"261. *Can you describe the flower of the fuchsia?*

"It is that of a Calycifloral Exogen, with a coloured calyx valvate in æstivation, and consisting of four sepals. The corolla is twisted, and consists of four petals. The stamens are eight, in two rows, with long filaments. The pistil is syncarpous, with an inferior ovary, a long style, and a stigma of four lobes."

It is fair, however, to say that some of the information imparted by Mr. Gibbs is less indigestible. If the whole catechism were as sensible as the following, it might be actually useful :—

"221. *What name do botanists give the cabbage?*

"*Brassica oleracea.*

"222. *What do they call the cauliflower?*

"It is a variety of the same species.

"223. *Is not the difference between them enough to make them appear distinct species?*

"It would be if they were not known to be derived from a common origin.

"224. *Then how came they to differ so much?*

"They differ in consequence of cultivation having increased the luxuriance of their growth; some plants producing an abundance of large and succulent leaves, and others an extraordinary number of flower buds. Such plants have been selected by gardeners for many generations, till they have resulted in the production of distinct varieties.

"225. *How are these varieties perpetuated?*

"The varieties of *Brassica oleracea* are perpetuated by seed which is taken from plants kept carefully apart from other varieties of the same species, for if the pollen of one variety happen to fall on the stigma of another, the seed produced by that flower would give origin to plants intermediate or uncertain in their character. Sometimes new varieties appear in this way."

But the catechetical method in its unrelieved dryness is too much even for the author. He preserves his gravity with undeviating firmness till we come to the last question and its answer, which deserve reproduction in the pages of NATURE as something absolutely unique in botanical literature.

"322. *Describe a Daisy.*

"Of this little plant of the Composite order,
Bellis perennis is surely the name ;
A perennial herb in the garden's gay border,
To ornament which from the meadows it came.

"Its roots of a good many fibres are growing
From under the sides of a prostrate rhizome ;
Which branches above, but is never found going
At any great length from the centre to roam.

"Spathulate leaves in a rosulate cluster,
Every ramification surround ;
And in the middle about which they muster,
A simple peduncle is commonly found.

"Are these the green leaves of a bud? Let us waken
With knowledge and skill to examine the facts ;
And then shall not be for a calyx mistaken
A real involucre consisting of bracts.

"For each of the ligulate florets composing
The circular ray is a separate bloom ;
And each little cup in the centre reposing,
For every part of a flower has room.

"It seems that the cup of the calyx adhering
Unto an inferior ovary so,
Accounts for that innermost organ appearing
To be in the place where we find it below.

"The corolla above it of tubular figure,
Coherent epigynous petals compose ;
As whoso describes it with technical rigour
By five little teeth on the edge of it knows.

"The stamens are called syngenesious truly,
Because of the fact that their anthers cohere ;
The style passes through them, and on it will duly
A couple of stigmatic branches appear.

"When all this is done and the blooming is over ;
When fruits monospermous are ripen'd and gone ;
They leave the receptacle nothing to cover
Its form which we find in the shape of a cone.

"If now I have done my agreeable duty,
I venture to hope I shall have better luck
Than the flower itself, which, because of its beauty,
Some ruthless examiner haply may pluck."

T. D.

OUR BOOK SHELF

A Manual of Anthropometry. By Charles Roberts, F.R.C.S. (London: J. and A. Churchill, 1878.)

THE full title of this moderately-sized volume gives an accurate account of its contents. It is "a guide to the physical examination and measurement of the human body, containing a systematic table of measurements, an anthropometrical chart or register, and instructions for making measurements on a uniform plan."

The author's appetite for figures is marvellous, and would be commendable, were it not too indiscriminate, his tables having the air of being immense hotch-pot collections, both in their titles and headings and in the run of their results, which is more irregular than is natural to the statistics of generically similar facts. The author does not seem to recognise the importance of what is really the main question to the statist, namely, how to obtain trustworthy results with a minimum of effort. He seems to squander his efforts, the results he obtains being very disproportionate to the labour bestowed in getting them. Thus in a chart, of which the leading idea is very good, for recording measurements at frequent intervals of different parts of the body of the same growing person, he has places for the entry of the measurement of more than sixty parts, a number altogether too great to be dealt with satisfactorily in statistical combination with similar measurements of other persons. The theoretical part of the book is altogether loose and unsatisfactory; it is a mixture of imperfectly understood Quetelet and water; Quetelet himself being somewhat lax in theory and much too watery already. On the other hand, the volume has many merits, and it is a pleasant duty to notice them as much as it is a disagreeable one to point out the defects in a work that aspires to set a standard to which future statisticians should conform. Thus the arrangement of the sample statistical tables is very convenient in its main features, and to that extent well worthy of imitation, though not as good as might be in all its details. For instance it omits mention of any measure of variability, such as the Probable Error. The book includes a long and useful list of anthropometric works copied with important additions from the American work of Dr. J. H. Baxter. The author is evidently most zealous, and having himself measured and weighed people by the hundred, he writes with the aplomb due to

much experience. His zeal is contagious, and consequently much of what he has written will favourably attract the attention of the statist.

It is a pity that the custom does not exist of weighing and measuring all the members of a family at frequent intervals between childhood to manhood, seeing how critical a test of sanitary condition is afforded by the progress of growth. Each illness leaves its mark, therefore a chart of height and weight with accompanying remarks, would give in a compendious form a very valuable life-history of the individual.

A Text-book of Arithmetic for Use in Higher Class Schools. By Thomas Muir, M.A., F.R.S.E. (Daldy, Isbister and Co., 1878.)

"NUMBER," infers "Recorde," in his "Whetstone of Witte," "is the onelie thing (almost) that seperateth man from beastes. Hee therefore that shall contempne numbre, he declareth himselfe as brutishe as a beaste, and unworthy to be counted in the fellowshippe of men. But I truste there is no man so foule ouerseene, though manie right smallye do it regarde."—(De Morgan, "English Mathematical and Astronomical Writers.") We have done with the miserable mercantile compendiums founded on Cocker, which De Morgan condemned, and have had, since his own "Arithmetic" appeared, many works of high value. Mr. Muir's work is worthy of taking place with these. His aims are high—mathematical accuracy, rational treatment, the presentment of essentials, with the accessories in due subordination, the production of a work suited both for mental training and as a preparation for the practical business of life. There is perhaps matter given more suitable for the use of teachers than of pupils—that is, for a school book we think much might be more concisely put. Persons taking up the subject at a more advanced age may find this fulness of explanation very valuable. The exercises are good and varied, and there is a chapter containing notices of books for future reading.

In connection with these notices and Mr. Muir's chapter on the roots of numbers (a part of the work which appears to us to require excision and a new treatment), we may add the following title :—"Éléments de Calcul Approximatif." Par Charles Ruchonnet (de Lausanne). Seconde Édition. (Paris, 1874)—a work which we have already noticed in NATURE.

The work is a thoroughly reliable one, accurately and neatly printed.

The Elements of Dynamics (Mechanics), with numerous Examples and Examination Questions. By J. Blaikie, M.A. (Edinburgh: J. Thin, 1878.)

MR. BLAIKIE says "special pains have been taken to establish the necessary propositions by proofs involving no higher mathematics than the geometry of the first two books of Euclid, and algebra as far as simple equations. At the same time the nomenclature, definitions, and general treatment are in harmony with advanced modern works on the subject." The author starts with kinematics and kinetics and derives statics as a particular case. A chapter is devoted to machines and another to hydrostatics. The objects laid down in the above statement have been well kept before the writer's eye, and the result is a good introductory book for young students. A word of decided commendation is due to the selection of general examples; there are besides some six university examination papers, specimens from the old universities, and from Edinburgh, London and Glasgow. The list of errata we have made is a very slight one, and we say this after working out all the examples.

This accuracy and carefulness of selection may be attributed to the fact of the work having been examined by such men as Professors Tait and Balfour Stewart and Mr. N. M. Ferrers. That our favourable opinion of this work is not singular may be inferred from the fact that a second edition is already in preparation.

Handbook to Map of the Geographical Distribution of Animals. By Andrew Wilson, Ph.D., &c. (Edinburgh and London: W. and A. K. Johnston, 1878.)

THIS is a very brief statement of the extent and limits of the six zoological regions and sub-regions, as given in Mr. Wallace's "Geographical Distribution of Animals," with an enumeration of the chief groups of mammals and birds characteristic of each region. The only novelty is that of placing the Ethiopian region fourth in order, thus separating it from the Palæarctic and Oriental, to both of which it is closely allied, and making it follow the Australian, with which it has no affinity. It is difficult to see the reason of this innovation, which will certainly not be considered an improvement. The map is in two large sheets, with the regions and sub-regions copied from Mr. Wallace's map, and similarly distinguished by colours. It is, however, a mere outline, and entirely without names—a great deficiency in any case, and especially when intended for junior students, to whom alone such a meagre sketch of the subject would be acceptable.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Gigantic Land Tortoises

MY attention has been called to the recent discussions in NATURE (vol. xviii. p. 220 *et seq.*) in reference to the geographical distribution of the gigantic land-tortoises of Malta, the Galapagos, and other oceanic islands (see also the annual address of the president of the Geological Society). On my return to America in 1864 from a four-years' residence in the guano key of Sombrero, West Indies, I put a small collection of fossil reptilian remains, found on that key, in the hands of the late Prof. Jeffries Wyman, of Boston, who had kindly consented to examine them. His report on the subject, received in August, 1865, was added as an appendix to a paper of mine on the geology of the key, but the latter was partially injured by a fire in the study of the late Prof. Joseph Henry, of the Smithsonian Institution, and its publication consequently postponed. I have since found time to restore and publish only a portion of my own paper,[1] but the report of Prof. Wyman has remained untouched among the charred MSS. of my observations on the guano-deposit of Sombrero. I inclose herewith a copy of Prof. Wyman's report, and also his subsequent autograph letter, in returning me the specimens. (In the former I have supplied, in italics, the probable words which are missing along the singed edges of some of the leaves.) I have thought these details worthy of statement in justice to this distinguished comparative anatomist —who recognised thirteen years ago the occurrence of this interesting fauna in a locality of the Atlantic, and one evidently unknown to the recent investigators—and in explanation of the long delay in the publication of his views. The specimens in question remain still in my possession, with the exception of one in the museum of this school.

I may further state that fossil specimens of these turtles, mostly fragmentary, have been found in many of the northern excavations upon Sombrero, since they were first worked, in 1856, and perhaps are still, as its exploitation has been continued by an English company for many years past. They occur altogether in the guano-veins which intersect the limestone beds to an unknown depth beneath the sea-level, and which are certainly but insertions from an ancient surface-bed of rock-guano, overlying but long since entirely denuded along with the crown of the key. In 1860–61, just before my arrival, enormous quantities of a guano-breccia were taken out from the largest of

[1] "Ann. Lyc. of Nat. Hist. of N.Y.," 1868, p. 251.

the northern quarries, which was largely made up of these fragments, probably to the amount of hundreds of tons! It will be seen from my paper (*loc. cit.*) that the Sombrero Key is merely the eroded remnant of an atoll, which presented, by its peculiar isolation, the most favourable refuge for this Chelonian family. Their existence in this region seems to furnish an important link in reference to the question of the ancient geographical connection between the Mascarene and the Galapagos Islands. Their occurrence has never been reported on the other guano keys of the Caribbean Sea—Navassa, Swan, Monk's, Redonda, &c; but I would suggest the propriety of an examination of the cargoes of guano from Navassa, &c., as well as from all other localities, which may still be brought to England. The conditions of the existence of this fauna will be discussed with more detail in the remainder of my Sombrero paper, when sufficiently restored and re-written for publication.

ALEXIS A. JULIEN

MY DEAR SIR,—The collection of fossils from the Sombrero Key, which you kindly placed in my hands for examination, comprises the remains of birds, turtles, and Saurian reptiles. Those of the first, and from the more superficial deposit, appear to belong to existing species of sea-birds now found along the coast, and have undergone no change. The following comprises a list of such specimens as are sufficiently well preserved to be identified. There are many other specimens, but they are so much broken that I have not been able to determine them. Specimens numbered from 1 to 17 inclusive are bones of turtles, and those from 1 to 11 inclusive are all from the same, viz., the middle, deposit.[1]

1. Left humerus of a large turtle; the ends are gone and the shaft alone is preserved; it is 6 inches in length, and at the smallest part has a diameter of 2·07 inches, from before backwards, and of 1·57 inches from side to side. Admitting that the usual proportions existed, the whole length would have been about 12 inches.

2. A fragment comprising a large part of a humerus; nearly the whole of the articular facet is preserved, its longest diameter measuring 2·38 inches, thus indicating a large species like that of 1.

3. Middle portion of a femur, 1·25 inch in diameter. This would indicate an entire bone of from 8 to 9 inches in length.

4. Lower third of a left humerus; the inner tuberosity is broken off, and does not therefore exhibit the emargination found in most turtles. This is somewhat smaller than the same part from a Galapagos turtle (*T. elephantopus*), the length of whose carapace was 27 inches.

5. A femur from which the upper portion is broken off. This is of the same size as the corresponding part in the Snapper (*E. serpentina*), the carapace of which was 15 inches long. The transverse diameter of the condyles was 1·35 inch.

6. Middle portion of the shaft of a humerus, 0·55 inch in diameter.

7. A similar fragment, 0·75 inch in diameter.

8. A fragment of one of the marginal bones, 1 inch in thickness. This could have only belonged to a turtle of the size of the Galapagos species.

9. A fragment of the right ilium, including the middle portion, the longest diameter of which last is 1·55 inch, and the two facets for the articulation of the ischium and pubes.

10. The shaft of a femur 0·75 inch in diameter.

11. Upper half of the ascending portion of the scapula; this has a diameter at the articular end of 0·90 inch.

All of the above specimens are from the same deposit in which the matrix is soft and crumbling.

12. Lower two-thirds of a humerus partly imbedded in a very dense matrix, which contrasts very strongly with that found in connection with the preceding specimen. The fragment is 3·50 inches long, 2·35 inches across condyles; a part of the inner one is broken off, but there are some signs of the lateral groove and notch at the end. The narrowest part of the shaft measures 0·92 inch in diameter.

13. Lower end of a radius from the left side.

14. Fragment of a carapace having a raised articular surface for the articulation of the pelvis, as in some of the land turtles.

15, 16, 17. Other fragments of a carapace.

17*a*. An irregular cast of a part of the interior of the carapace and plastron, the walls of the latter (?) being broken, but portions of them still adhering. Three of the median bony dermal plates, the largest 2·25 inches in diameter. These plates have neither ridge nor tuberosity on the median line, though there are slight projections over the head of the ribs resembling those of very old specimens of *E. serpentina*. The plastron is indistinctly seen, but enough remains to show that its union with the carapace was by a broad surface, and not by a narrow one, as in the marine and some of the fresh-water species.

18. A caudal vertebra of a saurian.

19. Anterior half of a lower jaw of a saurian which resembles in size and the arrangement of the teeth that of the *Iguana cornuta*, as figured by Cuvier in the *Ossemens fossiles*. The points of the teeth are compressed and show some signs of having been serrated, though now worn nearly smooth.

20. Fragment of a femur, which closely resembles in size and shape that of the *Iguana tuberculata*.

21. Another fragment of a femur closely resembling the last, but about one-fourth smaller.

The remains of turtles form by far *the largest part* of the collection of which the above is a list. From the fact which you communicated to me, that at *the present time* not a single species of turtle inhabits the *Sombrero Key*, the question at once arises whether these remains belong to species like those now inhabiting the sea, or to such as live either in fresh-water or on the land. After a careful comparison I do not find that any of them can be considered as of marine origin. All the long bones, consisting chiefly of arm and thigh bones, differ in a marked degree from corresponding ones of the sea-turtles in having the axis of the bones strongly curved instead of being nearly straight, in having the shaft at its middle nearly round instead of flattened, and in having the distal ends proportionally much broader. A comparison of the fossil fragments with the corresponding parts of fresh-water and land species is much more difficult, since these two kinds, in their anatomical features, so gradually shade into and so closely resemble each other that there is really no well-marked line of distinction.

It is certain, nevertheless, that the remains above noticed belong either to the fresh-water or land species, and the discovery of them where such no longer exist alive indicates a great revolution in the *previous history* of the island, and is therefore a *marked fact*. In addition we have the interesting remains of one of the species, which is certainly extinct and of gigantic size, equalling the largest specimens which are found living in any part of the world, and thus surpassing any now found in North or South America. The nearest instances of turtles of similar size are in the Galapagos Islands, where is found *T. elephantopus*. Specimens 7 and 10 indicate species as large as those now inhabiting the Americas.

Although among turtles it is almost impossible to establis species from fragments of bones, and these not the most characteristic ones, yet I have no doubt that the remains here described show the existence at least of three species, one, the longest of which represented by specimen 1, was undoubtedly an inhabitant of the land.

JEFFRIES WYMAN

Cambridge, August 14, 1865

[1] The Red Sandrock.—A. A. J.

The Figure of the Planet Mars

IN the report of the proceedings of the Academy of Sciences at Paris for October 22 (NATURE, vol. xviii. p. 712), with reference to a communication from me relative to the flattening of the planet Mars, it is stated that I confirm M. Amigues' conclusions from independent calculations. Allow me to say that the communication referred to, has clearly established by reference to dates of publication, that the calculations I had been the first to make were confirmed by the subsequent results of M. Amigues. A formula presented by me in February, 1870, in which the mean density, surface density, and velocity of rotation of Mars are expressed in connection with its ellipticity, was reproduced by apparently identical methods by M. Amigues, in the *Comptes Rendus* for June, 1874. The conclusions drawn from this formula by M. Amigues were, that in order to account for the high amount of ellipticity assigned to Mars by many astronomers its mean density must be less than its surface density. My conclusion was, on the contrary, that the high ellipticity alluded to was improbable and that the values given by Bessel, Oudemanns, Johnson, and other astronomers, whereby Mars would have an ellipticity nearly the same as that of the earth, should be adopted until the subject was cleared up by fresh observations.

H. HENNESSY

Royal College of Science for Ireland, November 2

The Colour-Sense

WITH reference to Dr. Pole's valuable papers on Homer's colour-blindness, it may interest your readers to learn that I have now nearly completed a work on "The Origin and Development of the Colour-Sense," which will be shortly published by Messrs. Trübner and Co. In it I have endeavoured to show (*inter alia*) that the use of colour-terms in the Homeric poems is strictly analogous to that of other races, existing or extinct, at the corresponding stage of culture; and that both depend, not upon dichromic vision, but upon a defect of language closely connected with the small number of dyes or artificial pigments known to the various tribes. To establish this result I have sent a number of circular letters to missionaries, Government officials, and other persons having relations with native uncivilised races in all parts of the world; and their answers to my queries, framed so as to distinguish carefully between perception and language, in every case bear out the theory which I had formed. As my results will so soon be published elsewhere, I shall not burden your columns with them at present, but may add that my researches lead me to place the origin of the colour-sense far lower down in the animal scale, as evidenced both by the distinctive hues of flowers and fruits, and by the varied integuments of insects, birds, &c., so far as these are the result of sexual selection, or of mimicry and other protective devices.

GRANT ALLEN

Magnus's "Hydrostatics" and the "London Science Series"

I KNOW it is unusual for an author to offer any reply to the favourable or unfavourable criticism of his reviewer; but I shall be glad if, by way of exception to this wise rule, you will allow me to make a few remarks on the notice of my little book which appeared in NATURE, vol. xviii. p. 693, as they refer to a subject of wider interest than the contents of the work itself. It unfortunately often happens that an author is able to detect that the reviewer has taken no further trouble than to make a few quotations from the preface of the book under review. For my own part I have no complaint on this ground. On the contrary, if the reviewer had even glanced at the preface he would have seen that the book has not been written for the use of very young boys, but that it "is intended for the use of those pupils in the upper Forms of schools who have already acquired some elementary knowledge of the principles of mechanics"—for those, in fact, to whom, after some adverse criticism, he is good enough to say "the book will undoubtedly prove useful."

My object in writing, however, is less to disprove anything that may have been said with regard to my own book than to take away the point of the criticism which has been directed against other volumes of the "London Science Series." As one of the Editors of this Series, I am anxious to correct an erroneous but somewhat prevalent impression that these books are intended to be "science primers." Nothing could be farther from the intentions of the Editors of this Series than the attempt to rival the excellent and original science primers published by Messrs. Macmillan. Judged by such a standard they must necessarily appear difficult and elaborate. But the standard is incorrect. The books of the present series are, as they purpose to be, essentially class-books, and many of them have been expressly written to meet the wants of the pupils of the higher forms of schools.

Although my reviewer "cannot imagine" that I "can be acquainted with science teaching in schools or its requirements," I may lay claim to so much experience as a teacher and school-examiner as shows me that a class-book should be rather above than below the average attainments of the form, and should be so written as to encourage the more advanced pupils to pursue their studies to a higher point.

If Science is to take the place of Classics in any of our schools it should be so taught as to afford an equivalent amount of mental discipline. If this is not the case the modern school will always rank below the grammar school, and there will be some ground for the alleged inferiority of the modern, with respect to the classical side of a public school. To teach science properly several hours a week ought certainly to be given to it, and I should be disposed to criticise somewhat severely the time-table of any school in which the boys "probably have one hour, or at most two, to devote to the subject in a week." Indications of showy and superficial knowledge on the part of boys who take up Science instead of Classics are not wanting, and this showiness may be partly due to the want of thoroughness of some of the text-books commonly in use.

PHILIP MAGNUS

Savile Club, London, November 3

[We have sent Mr. Magnus' letter to our Reviewer, who replies as follows.—ED.]

MR. MAGNUS complains that if his reviewer "had even glanced at the preface he would have seen that the book has not been written for very young boys, but that it is intended for the use of those pupils in the upper Forms of schools who have already acquired some elementary knowledge of the principles of mechanics." To this I may reply, firstly, that I did much more than glance at the preface; secondly, that I nowhere assert that the book is intended for "very young boys;" and thirdly, that I assert my belief that "for advanced boys in Upper Fifth and Sixth Forms the book will undoubtedly prove useful." By "young boys" I meant boys in the Upper Fourths, Removes, and Lower Fifths, whose average ages range between fourteen and sixteen. It is in these Forms that the principal science teaching in a Public school takes place.

I nowhere have asserted that the book is intended to be a "science primer," and my remark that it is intended for "school purposes" is taken from the commencement of the Editor's preface.

I am the more constrained to repeat my belief that the author cannot "be acquainted with science teaching in schools or its requirements" when I read his remarks on the character of a school class-book, and when I notice that he speaks of "science taking the place of classics."

I believe I am right in stating that in no school in England does science take the place of classics. I should sincerely deplore such a result. Any attempt to enforce it could only result in utter failure from an educational point of view. Science can never "afford an equivalent amount of mental discipline" to classics. Mr. Magnus may not be aware of the fact that science is taking the place of Latin verses in our public schools, and that no proposition has ever, as far as I know, been made to the effect that classics should be abandoned.

Finally, Mr. Magnus tells us that he "should be disposed to criticise somewhat severely the time-table of any school in which the boys probably have one hour, or at most two, to devote to the subject in a week." Possibly he would; but the fact is no less true that in almost all our principal schools two hours a week is the maximum time which is given, in the ordinary course of school work, to any one subject of science, and class books must be framed in accordance with such usage.

THE REVIEWER

The Discovery of a Crannog in Ayrshire

IN his letter reporting this interesting discovery (NATURE, p. 695), Dr. Munro remarks that amongst the constituents of the crannog was "brushwood, amongst which beech, birch, and hazel were readily recognised."

Now as *beech* is certainly not at present a *native* of Scotland, and as, to the best of my knowledge, it never has been claimed as an indigenous tree at any period, the finding of beech branches in the Tarbolton crannog is not the least interesting part of the discovery, and I venture to hope that Dr. Munro will be able to give us proofs that the brushwood he mentions is, beyond any doubt, beech.

Of course the other trees—oak, birch, and hazel—are truly indigenous. The *absence* of Scots-fir is also not without interest.

F. BUCHANAN WHITE

The Power of Stupefying Spiders Possessed by Wasps

MR. CECIL'S letters on "The Power of Stupefying Spiders possessed by Wasps" give details of a fact perfectly well known to entomologists, certainly to all those who have studied the Aculeata; but it is well known to the latter that no true wasp, according to the popular understanding of that name, ever supplies its larvæ with insects stupefied in the manner described. The insects alluded to in Mr. Cecil's letters probably belong to the section of aculeate insects usually known as sand-wasps by naturalists, a very misleading name, since a large number are wood-borers. It would render the subject of stupefying much more understandable to the general reader if this was more clearly elucidated; the general term wasp gives no clue in this instance to the insect observed. We have in this country fossorial insects belonging to the genera Pompilus, Priocnemus,

Agenia, Aporus, Miscophus, Ammophila, Crabro, Cerceris, Philanthus, and some others, all of which stupefy the caterpillars, spiders, or bees, which they store up for the nourishment of their brood, and it would be desirable to have it pointed out to what genera the insects really belong. The species seen by Mr. Cecil, in a collection at Athens, which is described as "a thin-bodied variety," is, I think, a species of the genus Ammophila, or of Pompilus; that observed by Mr. Armit, of Queensland, is probably a species of Pelopæus. Some further definition of the insects I consider highly desirable, as the general term wasp must, I believe, lead to a very false conclusion.

British Museum, November 2 FREDK. SMITH

The Expected Meteor Shower

THE meteor shower of Biela's comet, referred to in your "Astronomical Notes" as likely to occur on the 27th inst., should also be looked for on several evenings preceding that date. Last year there were more of these slow-moving *Andromedes* on the 25th than on the 27th of November. On the former night, from 5h. 30m. to 7h., I saw sixteen shooting stars, seven of which belonged to this stream, for they showed a good radiant at R.A. 24° Dec. 45° N. Yet on the 27th only two others were noted out of a total of 10 recorded during a watch from 9h. to 10h. 30m. An occasional look out is therefore advisable on several nights about the 27th, when, if meteors are seen in more than ordinary profusion, it will be important to record their numbers and paths. W. F. DENNING

Ashleydown, Bristol, November 7

Geological Climate and Geological Time

IN considering the climatic changes which have evidently taken place on various parts of the earth's surface, it seems to me that what may have been a very important factor has been rather strangely left out of calculation by physicists, never having been noticed hitherto, as far as I am aware. It is that of the heat which must at one period or other have been transmitted from the moon. There can be scarcely a doubt that this must at one time have influenced the earth's climate to a very powerful degree, producing the effects of a second or additional sun. In the absence of any perceptible marks of atmospheric or aqueous erosive action on the moon it is at present impossible to arrive at any idea of its relative age or at what period its heat may have been most abundantly radiated; but if the much hotter climate which once prevailed in northern latitudes could be referred to this cause it might give us some clue to the difficulty. Something also might be done in comparing the various changes of climate which have taken place in certain parts of the earth's surface, as indicated by geological evidence, with the actual course of the moon. The subject is at least worth entertaining, and may be recommended to the consideration of physicists. WILLIAM DAVIES

10, Guildford Street, Russell Square, November 4

A "New Galvanometer"

THE galvanometer (with its coil moving about a horizontal axis) described by Dr. Obach in NATURE, vol. xviii. p. 707, is not new. Prof. Pickering has fully described it, under the name of *Cosine* galvanometer, in his "Elements of Physical Manipulation," Part 2 (1876), p. 260. When this instrument was first used I do not know. R. E. BAYNES

Christchurch, Oxford, November 9

COMMERCIAL CRISES AND SUN-SPOTS[1]

"Thou Sun, of this great world both eye and soul."

IT is curious to notice the variety of the explanations offered by commercial writers concerning the cause of the present state of trade. Foreign competition, beer-drinking, over-production, trades-unionism, war, peace, want of gold, superabundance of silver, Lord Beaconsfield, Sir Stafford Northcote, their extravagant expenditure, the Government policy, the wretched Glasgow Bank directors, Mr. Edison and the electric light, are a few of the happy and consistent suggestions continually made to explain the present disastrous collapse of industry and credit.

It occurs to but few people to remember that what is happening now is but a mild repetition of what has previously happened time after time. October, 1878, is comparable with May, 1866, with November, 1857, with October, 1847, and, going yet further back, with a somewhat similar condition of things, in 1837, in 1825-26, and even in 1815-16. The incidental circumstances of these commercial collapses have indeed been infinitely diversified. At one time the cause seemed to be the misconduct of the great firm of Overends; in 1857 there was the mutiny in India, the peace with Russia, and a commercial collapse in the United States; in 1847 occurred the Irish famine and a failure of European harvests generally, following upon the great railway mania; the crisis of 1837 succeeded an immense expansion of home trade, the establishment of joint stock banks, and the building of multitudes of factories and other permanent works; 1825 was preceded by extravagant foreign speculations and foreign loans; 1815 was the year of the general peace. All kinds of distinct reasons can thus be given why trade should be now inflated and again depressed and collapsed. But, so long as these causes are various and disconnected, nothing emerges to explain the remarkable appearance of regularity and periodicity which characterises these events.

The periodicity of the earlier portion of the series is so remarkable that, even without the corroboration since received, it convinced scientific inquirers that there was some deep cause in action. Dr. Hyde Clarke, for instance, wrote, more than thirty years ago, a paper entitled "Physical Economy—a preliminary inquiry into the physical laws governing the periods of famines and panics." This paper was published in the *Railway Register* for 1847, and is well worth reading. In the commencement he remarks: "We have just gone through a time of busy industry, and are come upon sorrow and ill-fortune; but the same things have befallen us often within the knowledge of those now living. Of 1837, of 1827, of 1817, of 1806, of 1796, there are men among us who can remember the same things as we now see in 1847. A period of bustle, or of gambling, cut short in a trice and turned into a period of suffering and loss, is a phenomenon so often recorded, that what is most to be noticed is that it should excite any wonder." Dr. Hyde Clarke then proceeds to argue in a highly scientific spirit that events so regularly recurring cannot be attributed to accidental causes; there must, he thinks, be some physical groundwork, and he proposed to search this out by means of a science to be called Physical Economy. In the third page of his paper he tells us that he had previously written a paper on the laws of periodical or cyclical action, printed in Herapath's *Railway Magazine* for 1838. "At this time," he says, "it was my impression that the period of speculation was a period of ten years, but I was led also to look for a period of thirteen or fourteen years. . . . In the course of these inquiries I looked at the astronomical periods and the meteorological theories without finding anything at all available for my purposes." A little below Dr. Hyde Clarke continues:—"Still thinking that the interval was an interval of about ten years, I was, during the present famine, led to look for a larger period, which would contain the smaller periods, and as the present famine and distress seemed particularly severe, my attention was directed to the famine so strongly felt during the French Revolution. This gave a period of about fifty-four years, with five intervals of about ten or eleven years each, which I took thus:—

"1793 1804 1815 1826 1837 1847."

Dr. Hyde Clarke was by no means the only statist who

[1] This article, although treating the same subject, and partially containing the same facts as a paper by the same writer, read at the recent meeting of the British Association, is a distinct composition. The paper in question will probably be published elsewhere.

adopted a theory of periodicity thirty or forty years ago. In February, 1848, Mr. J. T. Danson read a paper to the Statistical Society of London, attempting to trace a connection between periodic changes in the condition of the people and the variations occurring in the same period in the prices of the most necessary articles of food. Mr. James Wilson had published, in 1840, a separate work or large pamphlet upon "Fluctuations of Currency, Commerce, and Manufactures," in which he speaks of the frequent recurrence of periods of excitement and depression. In later years Mr. Wm. Langton, the esteemed banker of Manchester, independently remarked the existence of the decennial cycle, saying: "These disturbances are the accompaniment of another wave, which appears to have a decennial period, and in the generation of which moral causes have no doubt an important share." The paper in which this remark occurs is contained in the *Transactions* of the Manchester Statistical Society for 1857, and is one of the most luminous inquiries concerning commercial fluctuations anywhere to be found.[1] In still later years Mr. John Mills of the Manchester Statistical Society has almost made this subject his own, insisting, however, mainly upon the mental origin of what he has aptly called the Credit Cycle.

The peculiar interest of Dr. Hyde Clarke's speculations consists in the fact that he not only remarked the cycle of ten or eleven years, but sought to explain it as due to physical causes, although he had not succeeded in discovering any similar astronomical or meteorological variation with which to connect it. Writing as he did in 1838 and 1847, this failure is not to be wondered at. His supposed period of fifty-four years is perhaps deserving of further investigation, but it is with his period of ten or eleven years that we are now concerned.

My own inquiries into this interesting subject naturally fall much posterior to those of Dr. Clarke; but, about the year 1862, I prepared two elaborate statistical diagrams, one of which exhibited in a single sheet all the accounts of the Bank of England since 1844, while the other embraced all the monthly statements I could procure of the price of corn, state of the funds, rate of discount, and number of bankruptcies in England from the year 1731 onwards. Subsequent study of these diagrams produced upon my mind a deep conviction that the events of 1815, 1825, 1836-39, 1847, and 1857, exhibited a true but mysterious periodicity. There was no appearance, indeed, of like periodicity in the earlier parts of my second diagram. In the first fifteen years of this century statistical numbers were thrown into confusion by the great wars, the suspension of specie payments, and the frequent extremely high prices of corn. It must be allowed, moreover, that the statistical diagram, so far as concerns the eighteenth century, presents no appreciable trace of decennial periodicity. The recent continual discussions concerning the solar or sun-spot period much increased the interest of this matter, and in 1875 I made a laborious reduction of the data contained in Prof. Thorold Rogers' admirable "History of Agriculture and Prices in England from the Year 1259." I then believed that I had discovered the solar period in the prices of corn and various agricultural commodities, and I accordingly read a paper to that effect at the British Association at Bristol. Subsequent inquiry, however, seemed to show that periods of three, five, seven, nine, or even thirteen years would agree with Prof. Rogers' data just as well as a period of eleven years; in disgust at this result I withdrew the paper from further publication. I should like, however, to be now allowed to quote the following passage from the MS. of the paper in question:—

"Before concluding I will throw out a surmise, which, though it is a mere surmise, seems worth making. It is now pretty generally allowed that the fluctuations of the money market, though often apparently due to exceptional and accidental events, such as wars, great commercial failures, unfounded panics, and so forth, yet do exhibit a remarkable tendency to recur at intervals approximating to ten or eleven years. Thus the principal commmercial crises have happened in the years 1825, 1836-9, 1847, 1857, 1866, and I was almost adding 1879, so convinced do I feel that there will, within the next few years, be another great crisis. Now if there should be in or about the year 1879, a great collapse comparable with those of the years mentioned, there will have been five such occurrences in fifty-four years, giving almost exactly eleven years (10·8) as the average interval, which sufficiently approximates to 11·1, the supposed exact length of the sun-spot period, to warrant speculations as to their possible connection."

I was led to assign the then coming (that is, the now present) crisis to the year 1879, because 11·1 years added twice over to 1857, the date of the last perfectly normal crisis, or to 1847, the date of the previous one, brings the calculator to 1879. If I could have employed instead Mr. J. A. Broun's since published estimate of the sun-spot period, to be presently mentioned, namely, 10·45 years, I should have come exactly to the present year 1878. My mistake of one year was due to the meteorologists' mistake of eight months, which, as crises usually happen in October and November, was sufficient to throw the estimate of the event into the next twelvemonths.

While writing my 1875 paper for the British Association, I was much embarrassed by the fact that the commercial fluctuations could with difficulty be reconciled with a period of 11·1 years. If, indeed, we start from 1825, and add 11·1 years time after time, we get 1836·1, 1847·2, 1858·3, 1869·4, 1880·5, which show a gradually increasing discrepancy from 1837, 1847, 1857, 1866 (and now 1878), the true dates of the crises. To explain this discrepancy I went so far as to form the rather fanciful hypothesis that the commercial world might be a body so mentally constituted, as Mr. John Mills must hold, as to be capable of vibrating in a period of ten years, so that it would every now and then be thrown into oscillation by physical causes having a period of eleven years. The subsequent publication, however, of Mr. J. A. Broun's inquiries, tending to show that the solar period is 10·45 years, not 11·1,[1] placed the matter in a very different light, and removed the difficulties. Thus, if we take Mr. John Mills' "Synopsis of Commercial Panics in the Present Century," and, rejecting 1866 as an instance of a premature panic, count from 1815 to 1857, we find that four credit cycles occupy forty-two years, giving an average duration of 10·5 years, which is a remarkably close approximation to Mr. Broun's solar period. Thus encouraged, it at last occurred to me to look back into the previous century, where facts of a strongly confirmatory character at once presented themselves. Not only was there a great panic in 1793, as Dr. Hyde Clarke remarked, but there were very distinct events of a similar nature in the years 1783, 1772–3, and 1763. About these dates there can be no question, for they may all be found clearly stated on pp. 627 and 628 of the first volume of Mr. Macleod's unfinished "Dictionary of Political Economy." Mr. Macleod gives a concise, but, I believe, correct account of these events, and as he seems to entertain no theory of periodicity, his evidence is perfectly unbiased. Yet, in the space of a few lines, he unconsciously states this periodicity, saying:—"Ten years after the preceding crisis of 1763 another of a very severe nature took place in 1772 and the beginning of 1773. It extended over all the trading nations of Europe." A few lines below he goes on to state that in May, 1783, a rapid drain of bullion to the Continent set in, which greatly alarmed the Bank directors and embarrassed the merchants. The

[1] It is reprinted in the *Transactions* of the same Society for 1875-76.

[1] NATURE, vol. xvi. p. 63.

paragraph in which this occurs is headed "The Crisis of 1783," and on turning the page we at once come on another paragraph headed "The Crisis of 1793." Here then we have, in a few lines of a good authority concerning the history of finance, a statement of four crises occurring at almost exactly decennial intervals. It is wonderful that no writer has, so far as I know, previously pointed out the strictly periodic nature of these events; and I may add that I have several times lectured to my college classes about these crises without remarking their periodicity. It is true that we cannot, by any management of the figures, bring them into co-ordination with later crises so long as we adhere to the former estimate of the solar period. If, starting from 1857, we count back nine intervals of 11·1 years each, we get to 1757 instead of 1763; we are landed in the middle of a cycle instead of in the beginning or end; and there can be no possible doubt about the crises of 1763 and 1857. But, if we are once allowed to substitute the new estimate of Broun, which is the same as the old one of Lamont, the difficulty disappears; for the average interval is 10·44 . . . years!

This beautiful coincidence led me to look still further backwards, and to form the apparently wild notion that the great crisis generally known as that of the South Sea Bubble might not be an isolated and casual event, but only an early and remarkable manifestation of the commercial cycle. The South Sea Bubble is generally set down to the year 1720, and the speculations in the shares of that company did attain their climax and commence their collapse in that year. But it is perfectly well known to the historians of commerce that the general collapse of trade which profoundly affected all the more advanced European nations, especially the Dutch, French, and English, occurred in 1721. Now, if we assume that there have been since 1721, up to 1857, thirteen commercial cycles, the average interval comes out 10·46 years; or if we consider that we are in this very month (November, 1878) passing through a normal crisis, then the interval of 157 years from 1721 to 1878 gives an average cycle of 10·466 years.

It would be impossible, however, to enlist the South-Sea Bubble in our series unless there were some links to connect it with subsequent events. I have, therefore, spent much labour during the past summer in a most tedious and discouraging search among the pamphlets, magazines, and newspapers of the period, with a view to discover other decennial crises. I am free to confess that in this search I have been thoroughly biased in favour of a theory, and that the evidence which I have so far found would have no weight if standing by itself. It is impossible in this place to state properly the facts which I possess; I can only briefly mention what I hope to establish by future more thorough inquiry.

It is remarkable to notice that the South Sea Company, which came to grief in 1720-21, was founded in 1711, just ten years before, and that on the very page (312) of Mr. Fox Bourne's "Romance of Trade,"[1] which mentions this fact, the year 1701 also occurs in connection with speculation and *stock-jobbing*, as the promotion of companies was then called. The occurrence of a crisis in the years 1710-11-12, is indeed almost established by the lists of bubble insurance companies formed in those years, as collected by Mr. Cornelius Walford, and obligingly shown to me by him.[2]

Again, it is quite plain that about ten years after stock-jobbing had been crushed by the crisis of 1721, it reared its head again. A significant passage in the *Gentleman's Magazine* of 1732 (vol. ii. p. 561) remarks that "Stock-jobbing is grown almost epidemical. Fraud, corruption, and iniquity in great companies as much require speedy and effectual remedies now as in 1720. The scarcity of money and stagnation of trade in all the distant parts of England, is a proof that too much of our current coin is got into the hands of a few persons." This "getting the current coins into the hands of a few people" was the favourite theory at that time to explain any slackness of trade, just as now over-production is the theme of every short-sighted politician. But the legislature of that day thought they could remedy these things in a drastic manner, so they passed in 1734 "An Act to prevent the infamous practice of stock-jobbing." Mr. Walford, who has inquired into the commercial history of this time far more minutely than any other writer, remarks[1] that "gambling in stocks and funds had broken out with considerable fervour again during the few years preceding 1734. It was the first symptom of recovery from the events of 1720."

I may add that there was in 1732 a great collapse of a society called the "Charitable Corporation for Relief of the Industrious Poor." A great many people were ruined by the unexpected deficit discovered in the funds of this kind of bank, and Parliament and the public were asked to assist the sufferers, just as they might now be asked to aid the shareholders of the City of Glasgow Bank. Thus does history repeat itself!

Whether it was that the act of 1734 really did diminish the infamous practice of stock-jobbing, or, whether the sun-spots manifested less variation than usual, it is clear that between 1732 and 1763 it is very difficult to discover anything approaching a mania or crisis. My learned and obliging correspondents at Amsterdam and Leiden, Drs. S. and W. Vissering, disclaim any knowledge of such events in the trade of Holland at that time, and my own diagram, showing the monthly bankruptcies throughout the interval, displays a flatness of a thoroughly discouraging character. Nevertheless, inquiry leads me to believe that although there really was nothing to call a crisis, mania, or panic, yet there were remarkable variations in the activity of trade and the prices of some staple commodities, such as wool and tin, sufficient to connect the earlier with the later periods. It is a matter of much regret that I have hitherto been quite unable to discover a connected series of price-lists of commodities of the early part of last century. The accounts of prices of goods at Greenwich Hospital, to be found in several statistical works, are not only incomplete, but probably misleading. Any reader of this article who can point out to me series of prices of metals or other commodities, not merely agricultural, before 1782, will confer a very great obligation upon me by doing so.

Deferring, however, for the present, any minuter inquiry, I permit myself to assume that there were about the years 1742 and 1752 fluctuations of trade which connect the undoubted decennial series of 1711, 1721, and 1732, with that commencing again in the most unquestionable manner in 1763. Thus the whole series of decennial crises may be stated as follows: (1701?), 1711, 1721, 1731-32, (1742? 1752?), 1763, 1772-73, 1783, 1793, (1804-5?), 1815, 1825, 1836-39 (1837 in the United States), 1847, 1857, 1866, 1878. A series of this sort is not, like a chain, as weak as its weakest part; on the contrary, the strong parts add strength to the weak parts. In spite, therefore, of the doubtful existence of some of the crises, as marked in the list, I can entertain no doubt whatever that the principal commercial crises do fall into a series having the average period of about 10·466 years. Moreover, the almost perfect coincidence of this period with Broun's estimate of the sun-spot period (10·45) is by itself strong evidence that the phenomena are causally connected. The exact nature of the connection cannot at present be established. As we have seen, Hyde Clarke, Wilson, and Danson all argued, some thirty or forty years ago, that commercial fluctuations must be

[1] This book contains an interesting account of some of these early manias and panics.

[2] These lists are partly published in Mr. Walford's "Insurance Cyclopædia," article Gambling.

[1] "Insurance Cyclopædia," art. Gambling.

governed by physical causes. But here we are embarrassed by the fact that no inquirer has been able to discover a clear periodic variation in the price of corn. This is what Sir William Herschel attempted to do, at the beginning of this century, in his truly prophetic inquiry about the economic effects of the sun-spots; but his facts are evidently too few to justify any sure inference. Carrington also compared the sun-spot curve with that of the price of corn, without detecting any coincidence; and my own repeated inquiries have been equally without result as to this point. The fact is, I believe, that cereal crops, as grown and gathered in Europe, depend for their success upon very complicated conditions, so that the solar influence is disguised. But it does not follow that other crops in other latitudes may not manifest the decennial period. Dr. Schuster[1] has pointed out a coincidence between good vintages and minima of sun-spots which can hardly be due to accident, and the whole controversy about the connection of Indian famines with the sun-spot period is of course familiar to all readers of NATURE. Now if we may assume Dr. Hunter's famine theory to be true there is little difficulty in explaining the remarkable series of periodic crises which I have pointed out.

The trade of Western Europe has always been strongly affected by communication with the Indies. Several of the crises are distinctly traceable to this cause, especially those at the beginning of the eighteenth century. That was a time of wild enterprise in the tropical regions, as the very names of the South Sea Company, the Mississippi scheme, the Darien project, &c., show. The Dutch, English, and French East India Companies were then potent bodies, the constant subject of legislation and controversy. Thus it is my present belief that to trade with India, China, and probably other parts of the tropical and semi-tropical regions, we must attribute the principal fluctuations in European commerce. Surely there is nothing absurd in such a theory when we remember that the present crisis is at least partly due to the involvement of the City of Glasgow Bank in the India trade, through the medium of some of their chief debtors. Thus the crisis of 1878 is clearly connected with the recent famines in India and China, and these famines are confidently attributed to solar disturbance.

To establish this view of the matter in a satisfactory manner, it would be desirable to show that there has been a decennial variation of trade with India during the 170 years under review. The complications and disturbances produced in the statistics of such a trade by various events are so considerable that I have not yet attempted to disentangle them properly. Yet the accounts of the merchandise (not including bullion) exported by the English East India Company between the years 1708–9 and 1733–34 display such a wonderful tendency to decennial variation, that I cannot refrain from quoting them. As stated by Milburn in vol. i. p. xlviii. of his "Oriental Commerce," they are as given in the following table, except that I have struck off three places of figures useless for our purposes:—

Values of Merchandise Exported to India

Years.	£1000	Years.	£1000
1708–9	162	1721–22	148
1709–10	161	1722–23	135
1710–11	201	1723–24	118
1711–12	162	1724–25	97
1712–13	109	1725–26	80
1713–14	85	1726–27	77
1714–15	79	1727–28	101
1715–16	61	1728–29	102
1716–17	69	1729–30	135
1717–18	88	1730–31	137
1718–19	107	1731–32	150
1719–20	134	1732–33	105
1720–21	122	1733–34	140

[1] NATURE, vol. xvi., p. 45.

In the above table there are three well-marked maxima in 1710–11, 1721–22, and 1731–32 at intervals closely approximating to that of the sun-spot curve. I believe that there are some traces of the same decennial variation in subsequent portions of the same tables. The fact that this variation is difficult to trace may possibly explain the absence of any serious crises in 1742 and 1752.

Probably, however, we ought not to attribute the decennial fluctuation wholly to Indian trade. It is quite possible that tropical Africa, America, the West Indies, and even the Levant are affected by the same meteorological influences which occasion the famines in India. Thus it is the nations which trade most largely to those parts of the world, *and which give long credits to their customers*, which suffer most from these crises. Holland was most easily affected a century ago; England is most deeply affected now; France usually participates, together with some of the German trading towns. But I am not aware that these decennial crises extend in equal severity to such countries as Austria, Hungary, Switzerland, Italy, and Russia, which have comparatively little foreign trade. Even when they are affected, it may be indirectly through sympathy with the great commercial nations.

There is nothing in this theory inconsistent with the fact that crises and panics arise from other than meteorological causes. There was a great political crisis in 1798, a great commercial collapse in 1810–11 (which will not fall into the decennial series); there was a Stock Exchange panic in 1859; and the great American collapse of 1873–75. There have also been several minor disturbances in the money market, such as those of February, 1861, May and September, 1864, August, 1870, November, 1873; but they are probably due to exceptional and disconnected reasons. Moreover, they have seldom, if ever, the intensity, profundity, and wide extension of the true decennial crises.

If it were permitted to draw any immediate conclusion from these speculations, I should point to the necessity of at once undertaking direct observations upon the varying power and character of the sun's rays. There are hundreds of meteorological observatories registering, at every hour of the day and night, the most minute facts about the atmosphere; but that very influence, upon which all atmospheric changes ultimately depend, *the solar radiation*, is not, I believe, measured in any one of them, at least in the proper manner.[1] Pouillet showed long ago (1838) how the absolute heating power of the sun's rays might be accurately determined by his Pyrheliometer. This instrument, and the results which he drew from its use, are fully described in his "Éléments de Physique Expérimentale et de Météorologie" (livre 8me, chap. i., section 285). But I have never heard that his experiments have been repeated, except so far as this may have been done by Sir John Herschel, with his so-called Actinometer, as described by him in the Admiralty Manual of Scientific Inquiry. I fancy that physicists still depend upon Pouillet's observations in 1837 and 1838 for one of the most important constants of the solar system, if constant it can be called. While astronomers agitate themselves and spend infinite labour about the two-hundredth planetoid, or some imperceptible satellite, the very fountain of heat and light and life is left unmeasured. Pouillet indeed assumed that the heating power of the sun's rays is a constant quantity, which accounts for his not continuing the solar observations. But, if there is any truth in all these sun-spot speculations, there must be a periodic variation in the sun's rays, of which the sun-spots are a mere sign, and perhaps an unsatisfactory one. It is possible that the real variations are more regular than the sun-spot indications, and thus perhaps may be explained the curious fact that the decennial crises recur more regu-

[1] Of course there have been abundance of black-bulb thermometer observations made in various parts of the world, but I doubt whether they are of much value.

larly on the whole than the maxima and minima of sun-spots.

But why do we beat about the bush when all that is needed is half-a-dozen of Pouillet's pyrheliometers with skilled observers, who will seize every clear day to determine directly the heating power of the sun? Why do we not go direct to the Great Luminary himself, and ask him plainly whether he varies or not? If he answers No! then some of us must reconsider our theories, and perhaps endure a little ridicule. But if, as is much more probable, he should answer Yes! then the time will come when the most important news in the *Times* will be the usual cablegram of the solar power. Solar observatories ought to be established on the table-lands of Quito or Cuzco, in Cashmere, in Piazzi Smyth's observatory on the Peak of Teneriffe, in Central Australia, or wherever else the sun can be observed most free from atmospheric opacity. An empire on which the sun never sets, and whose commerce pervades every port and creek of the sunny south, cannot wisely neglect to keep a watch on the great fountain of energy. From that sun, which is truly "of this great world both eye and soul," we derive our strength and our weakness, our success and our failure, our elation in commercial mania, and our despondency and ruin in commercial collapse.

W. STANLEY JEVONS

THE WERDERMANN ELECTRIC LIGHT

WE are able this week to give some further details concerning Mr. Werdermann's method of dividing the electric light.

The real difficulty was found in devising a form of *light* which *could* be divided into several, and still give enough illuminating power for practical use; and it is in this particular that Mr. Werdermann has apparently succeeded. It may be interesting here to state Mr. Werdermann's reasons for adopting this particular form of lighting.

When in an electric lamp, electrodes having the same sectional area are used, the changes at the points between which the voltaic arc passes, take place in a manner which is well known, viz., a crater or hollow is formed in the positive electrode which emits the light, the crater itself being heated by the current to white heat, and the surrounding part to redness. The negative electrode which assumes the form of a cone, is only heated to redness, and emits scarcely any light.

It was found that an increase in the sectional area of the positive electrode diminishes the light emitted by that electrode, and if the increase is continued gradually, the light on that electrode finally disappears entirely, whereas the heating effect upon the negative electrode in connection therewith increases, until finally light is emitted by the same. Again, by increasing the sectional area of the negative electrode, the heating effect upon the same decreases proportionally to the increase of its area, until the area having been sufficiently increased the heat almost entirely disappears, and consequently the consumption or wearing away of that electrode is scarcely appreciable.

The light given out by the positive electrode in connection therewith, on the contrary, increases in proportion to the difference existing between the sectional area of the two electrodes, and instead of a crater being formed in the positive carbon, the latter assumes the form of a cone as formerly was the case with the negative carbon. The greater the difference between the areas of the two carbons the shorter is the length of the voltaic arc which can be obtained between them, and when the area of the positive is gradually diminished and that of the negative increased, the light is produced by the carbons apparently in contact, and a small deposit of graphite is seen on the negative electrode. The section of this deposit is about $\frac{1}{4}$ that of the positive carbon itself, and it is about $\frac{1}{8}$ of an inch high.

Mr. Werdermann was led to make these experiments by the idea that perhaps by altering the sectional area of the carbons a similar effect might be produced to that which is obtained in electrolysis when a plate is used as one electrode and a small wire at the other, and from the

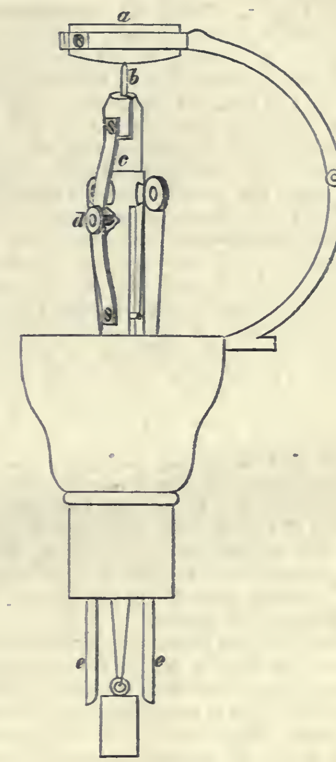

FIG. 1.

results obtained he devised his present system of electric lighting.

His lamp is constructed in the following manner:—

He places the negative carbon which is in the form of a disc 2 inches in diameter, and about 1 inch thick, uppermost. This carbon is clasped all round by a copper band which is prolonged to the terminal to which one of the leading cables is attached. The lower or positive electrode is a small pencil of carbon 3 millimetres in

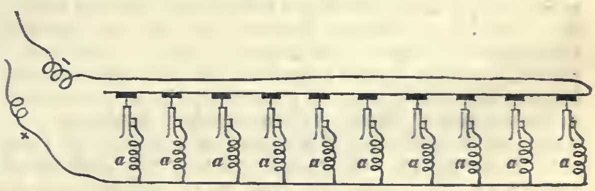

FIG. 2.

diameter, and can be made of any suitable length. This slides up vertically in a tube placed directly underneath the disc. This tube guides the pencil and also forms a contact for it, the top of the tube being solid copper in two pieces, one being rigid and the other pressing against the carbon by means of a regulating spring. The carbon pencil protrudes above the tube about $\frac{3}{4}$ of an inch, and touches the negative disc, and this length when the current passes is made incandescent.

The small carbon is pointed at its upper extremity and retains this point while burning. A small electric arc is formed round the points of junction, and to this is due the greater part of the light and not to incandescence alone. The carbons are kept in contact by chains attached to the lower end of the pencil passing over pulleys and down again to a weight of about 1½ lbs., which is sufficient to keep the pencil pressing gently against the disc.

The sketch, Fig. 1, shows the arrangement of the lamp; *a* is the negative carbon, connected by the semicircular piece of metal *f* to the conductor *e* on the right-hand side which forms part of the lamp-post. The metal *f* is hinged so that the top carbon may be moved back when a globe is put on. *b* is the pencil or positive carbon sliding in the tube *c*, this tube being connected to the conductor *e* on left-hand side. The pressure of the contact upon the small carbon is regulated by the spring *d*. The tube is shown in perspective for greater clearness. The arrangement and details of the lamp being thus shown, we will now describe the experiments which have recently been exhibited at the works of the British Telegraph Manufactory in the Euston Road. The display was mostly of an experimental character, the lamps being somewhat different in construction to those which will be made use of in actual practice, but the principle remains the same. The chief object of the inventor was to demonstrate that a number of lights can be steadily maintained in one circuit. The first experiment tried was that of putting two large lamps such as will be used for street-lighting in circuit with a Gramme electro-plating machine. It may be here remarked that this is probably the first time that such a machine was ever used for the purpose of producing an electric light. The two lamps were said to give a light equal to 360 candles each, but they gave to all appearance a considerably higher illuminating power.

A pure white light was given out, perfectly steady, and showing none of the blue or purple rays observed so frequently in the ordinary form of electric arc. The wonderful steadiness of the light is one of its chief features. After burning for some considerable time the current was switched on to a row of ten smaller lamps arranged on a shelf. The light from each lamp was apparently of the same strength and the effect was very brilliant, but the total illuminating power was not nearly so great as in the case of the two larger ones. But it seemed to show that a form of light had been devised that could be split up into a considerable number of smaller ones, each of which could be made use of in a practical way. The ten lamps were estimated to have a lighting power of forty candles each, but this is probably somewhat above the mark. But the results obtained, both as regards the wonderful regularity of the lamps and the practical demonstration of dividing the light, seem to have been satisfactory; and the more remarkable from the fact of the weak electro-motive force of the machine, which is only equal to that of four Daniell's cells. More lamps could have been lighted even from this machine had they been at Mr. Werdermann's command, but of course with a diminution of light. When suitable machines have been constructed Mr. Werderman is confident of being able to put 50 or 100 lights in circuit, but he does not believe in the indefinite division of the current for lighting purposes.

The lights were all connected parallel, as shown in diagram, Fig. 2. The thick wires + and − connect the lamps with the machine, the first lamp on the + cable being last on the − wire. The spirals *a* are extra resistances put in the circuit of each lamp, the object being to render the divided current less sensitive to any slight variation in the resistance of the lamps themselves, due to unequal pressure of contact, &c. The resistance of each lamp, including the wire *a*, is about 0·39 ohms. The resistance of the ten in parallel circuit about 0·037 ohms. The carbon pencil consumes at the rate of from 1½ to 2 inches per hour in the small lamps; the large ones taking 4½ millimetre carbons, consume about 2½ or 3 inches in the same time. The pencils are made in Paris, costing about 1 franc per yard, which length will last for twelve hours. The discs are of ordinary carbon.

However many lights may be in use, one, two, three, or any number can be put out without affecting the others, the regulation of the current being provided for by a switch attached to each lamp. But if necessary, the current which originally went through those that are extinguished can be added to those kept alight, of course increasing their illuminating power. The lamps are set in action simultaneously, can be as easily put out, and again re-lighted.

Returning again to the intensity of the light, it was stated that the large lamps were equal to 360 candles. Now the effect of this light upon the eyes is apparently not injurious, and it is Mr. Werdermann's intention to use only globes of ordinary glass, as in the present form of gas-lamps; by this means the loss of light will be very slight indeed as compared with other systems, where the loss is from 20 to 30 per cent., incurred by using opal or ground glass globes.

Owing to the very small electromotive force of the machine the insulation of the cables can easily be provided for, and Mr. Werdermann hopes, with sufficiently powerful machines, to be able to carry the current to a considerable distance without any appreciable loss.

In conclusion, it may be worth while giving a few details in regard to the Gramme machine used. It is an electro-plating machine of the old pattern, having four upright electro-magnets and two bobbins, one for feeding the electro-magnets, the other for taking off the light-producing current. The bobbins are wound with thick copper bands. The electromotive force, as before stated, is only equal to four Daniell's cells, and the resistance of the taking-off bobbin is about 0·008 ohms. The quantity of current produced is of course large.

It may be mentioned that the large lamps were connected parallel, but having no extra resistances, as in the case of the 10; their resistance is also a trifle less. The resistances given are when the lamps are not alight; when burning it would be somewhat less. The power required to drive the machine described is about two horse-power.

A curious fact about the light is that the top carbon is not consumed, or at any rate so slowly, that it is not noticeable; therefore, to all intent and purpose, the lower carbon only is wasted.

T. E. GATEHOUSE

DUPLEXING THE ATLANTIC CABLE

THE simultaneous transmission of two telegraphic messages in opposite directions upon one wire, now known by the name of duplex telegraphy, dates back from the year 1853. In that year Dr. Gintl, the director of state telegraphs in Austria, described a method by which this feat could be accomplished, and in July of the same year the method suggested by Gintl was tried between Prague and Vienna. An improvement on this method was suggested by a German electrician, Frischen, by Messrs. Siemens and Halske, of Berlin, and other workers at this subject. Nevertheless, owing to practical difficulties, the experiments were little more than interesting additions to our knowledge. So little hope, indeed, was there of the practical realisation of this important matter that, in a standard work on telegraphy, published in 1867, after describing the early methods of duplex telegraphy, the author remarks:—"Systems of telegraphing in opposite directions and of telegraphing in the same

direction more than one message at a time must be looked upon as little more than feats in 'intellectual gymnastics,' very beautiful in their way, but quite useless in a practical point of view." Such assertions should teach all scientific writers the lesson of "hoping all things not impossible, believing all things not improbable," an attitude of mind which, Sir John Herschel remarks, should always characterise the natural philosopher, and which, in the present day, is certainly the safest one. Within six years of the publication of the foregoing statement duplex telegraphy was not only largely employed in actual telegraphy, but its use on certain busy lines became absolutely indispensable. The change from theoretical to practical success is due to an American, Mr. J. B. Stearns, who in 1872 succeeded in overcoming the main obstacle in duplex telegraphy, namely, what is known as the static discharge from the line. This Stearns accomplished by using a "condenser"; and further he developed a system of "duplexing" the line similar to the principle of the Wheatstone bridge.

FIG. 1.

FIG. 2.

More or less successful attempts were afterwards made to duplex submarine cables, and in the early part of 1877 Mr. J. Muirhead succeeded in duplexing the cables of the Eastern Telegraph Company by his artificial condensers. But we believe that his success was only partial. Subsequently Mr. Muirhead has been at work duplexing the Direct United States Cable with some prospect of success, and this week Stearns, who may be called the father of duplex telegraphy, has actually achieved the great feat of perfectly duplexing the Anglo-American Cable. In a message received by Mr. W. H. Preece this week, Mr. Stearns says, "I managed to get some

FIG. 3.

specimens for you this morning, though we had no time to make the balance *especially* perfect for the purpose. No. 1 shows signals received single; No. 2, ditto, duplex. No one can tell the difference. No. 3 is our balance while keying, but not receiving. No. 4 shows the balance perfect at first, but destroyed and restored again by the adjuster. It shows with what facility the ordinary adjustments can be made after the balance is once obtained. The whole time occupied by slip No. 4 was about twenty seconds."

To understand these drawings our readers must know that all the messages now sent across the Atlantic are

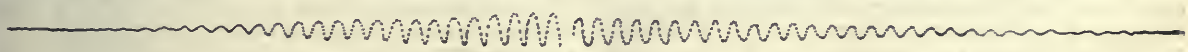

FIG. 4.

automatically registered by means of Sir W. Thomson's delicate and beautiful siphon recorder, which spirts out little jets of ink in a fine stream on a moving ribbon of paper. When no current passes the ink-marks form a straight line, but a current causes this line to deviate to the right or left, according to the direction of current. Hence the ordinary right and left strokes of a needle instrument or the long and short dashes of a Morse are here indicated by marks above and below the middle line. Thus the balance is shown by the almost perfectly straight line in Fig. 3 and the messages in Figs. 1 and 2.

The essence of duplex telegraphy is to obtain an electrical balance round on the line such that the sending instrument is not affected by currents circulating round it coming from the sending end, but only by currents received from the opposite end, and *vice versâ*. Hence, if the balance be once obtained, double transmission is possible. This balance Stearns has succeeded in obtaining by the use of his system as applied to land lines, and without the aid of the additional arrangements of artificial condensers used by Dr. Muirhead.

THE ROYAL SOCIETY MEDALLISTS

THE following are the awards of medals by the Council of the Royal Society for the present year. The medals will be given away at the Society's anniversary meeting on the 30th inst.:—The Copley Medal to M. Jean Baptiste Boussingault for his long-continued and important researches and discoveries in agricultural chemistry; a Royal Medal to Mr. John Allan Broun, F.R.S., for his investigations during thirty-five years in magnetism and meteorology, and for his improvements in methods of observation; a Royal Medal to Dr. Albert Günther, F.R.S., for his numerous and valuable contributions to the zoology and anatomy of fishes and reptiles; the Rumford Medal to M. Alfred Cornu, for his various optical researches, and especially for his recent re-determination of the velocity of propagation of light; the Davy Medal to MM. Louis Paul Cailletet and Raoul Pictet, for their researches, conducted independently but contemporaneously, on the condensation of the so-called permanent gases.

Jean Baptiste Boussingault was born in Paris in 1802. He was educated at the Mining School of St. Étienne, after leaving which he became connected with an English company formed to recover and work some mines in South America. This project, however, turning out unsatisfactory, after a considerable time spent in scientific travel in that continent, he returned to France and commenced those researches with which his name is more closely allied, the most important of which lie in the domain of agricultural chemistry. Probably his investigations of greatest value are those in which he has determined the quantities of carbon, nitrogen, and hydrogen found in plants, and his comparison of these with the amounts of the same constituents supplied to the plant by manures, &c. During these investigations he has shown, by a series of most conclusive experiments, the inaccuracy of the theory "that plants in their growth

derive nitrogen from the air," but, on the contrary, has pointed out that all the nitrogen assimilated by them may be accounted for in the different compounds of that body which are supplied to the plant in other ways. Boussingault's experiments also on the nutritive properties of the nourishment supplied to herbivorous animals are of great interest. In these he has traced the distribution of the various constituents of the food by the vital process, and has determined the different quantities of the various constituents which undergo assimilation. Besides investigations in the directions just indicated, he has introduced various improvements in methods of analysis, and has published many valuable articles, most of which are collected in his "Mémoires de Chimie Agricole et de Physiologie;" he has also written a work entitled "Traité d'Economie Rural."

With the work of Mr. J. Allan Broun most of our readers must be familiar. On the commencement of magnetic observations he indicated the errors or insufficiencies of the methods for determining coefficients and correcting the observations issued by the committee of the Royal Society for the instruction and direction of superintendents of observatories; he devised new methods for these ends which have made the observations in all the magnetic observatories available for strict scientific conclusions. He has made investigations in magnetism and meteorology during thirty-five years; among the new results obtained many of them are of the highest value, and have taken their place as standard scientific data. He established an observatory twice on a mountain-peak 6,000 feet above the sea, with the complement of instruments employed in first-class observatories (on the second occasion with a double series of magnetical instruments)—this, in a wild country, done amidst great difficulties in erecting instruments and obtaining trained observers, requiring continued and persevering action. This and many other duties were done at his own expense, and though, in general, ultimately repaid, they yet included considerable pecuniary loss. He also spent his own means in obtaining new instruments, and in every matter likely to forward science. He has laboured for years without remuneration in scientific work of a peculiarly tedious kind.

Albert C. L. G. Günther is the Keeper of the Zoological Department of the British Museum, a position to which he succeeded on the death of Dr. J. E. Gray. Very early in his life he devoted himself to the study of the natural sciences, and, if we are not mistaken, his earliest essay as an author was a very complete memoir of the fishes of the Neckar. About 1854 he accepted an appointment in the British Museum under Dr. J. E. Gray, who soon learned to value and appreciate the services of his assistant. Dr. Günther commenced the investigation and arrangement of the batrachian reptiles in the National Collection, with a zeal and energy that knew no limits, and which soon rendered this portion of the Zoological Department without a rival among the Museums of Europe. Not wearied by such a task he set about a far greater one, the arrangement and description of the immense class of the fishes, and no zoologist has ever raised a greater or more enduring monument to his memory than Dr. Günther has done in his great and truly scientific catalogue of all the known fish. The care of a large and daily-increasing collection, with all the worry incident on the want of room to properly store it—the toil and labour involved in the publication of the extensive work just alluded to, might well have excused Dr. Günther from attempting other work, but still we find him apparently never weary, and memoirs of a value like those on Hatteria, on Ceratodus, and on the Giant Tortoises, not to mention a long list of others, were being constantly published by him. He is also the author of an important work published at the expense of the Ray Society, "On the Reptiles of British India," and joint author with Col. Playfair of a work on the "Fishes of Zanzibar." All will agree that his name is a very worthy one to be added to the grand roll call of the Royal Society's medallists.

The name of M. Alfred Cornu must be familiar to the readers of NATURE in connection with his remarkably ingenious and successful method of determining the velocity of light. A detailed account of M. Cornu's method will be found in vol. xi. p. 274, and succeeding volumes of NATURE. It was also expounded by him, it may be remembered, at the Royal Institution, on May 7, 1875. The important bearings of M. Cornu's experiments in various directions, we need not point out; its value in attaining an accurate estimate of the sun's parallax is evident. As is evidenced by the *Comptes Rendus*, M. Cornu's work in his own department is constant and varied; his research into the spectrum of the star that appeared in Cygnus two years ago was a fine example of the result of spectroscopic research; an account of it will be found in NATURE, vol. xv. Although probably the youngest of the new medallists, M. Cornu's long and incessant work makes him almost a veteran in scientific research.

MM. L. Cailletet and Raoul Pictet have lately been so closely engaged in the same kind of experimental work, namely, the liquefaction of gaseous bodies, that their names have naturally become associated in connection with the important results which have followed their independent researches. The methods, however, employed by the two chemists in obtaining those results which have lately added so much to their reputation are to a certain extent different. Cailletet's experiments, which were conducted on the gases air, hydrogen, marsh gas, nitric oxide, and carbonic oxide, depend for the cold necessary to produce the liquefaction of the gas, on the expansion of the gas when suddenly compressed only at moderate degrees of cold.

Pictet's experiments, on the other hand, are the result of his endeavours to discover improved methods for producing and maintaining for a considerable time very low degrees of temperature. Combining these improvements with the production of the bodies to be liquefied under great pressure, he has succeeded in liquefying oxygen and hydrogen and in solidifying the latter. He has also determined the specific gravities of the gases when in that condition, assigning to them the weights ·9883 and ·9787. Besides his most recent researches on the condensation of gases, M. Pictet has carried out other investigations on those phenomena, the consideration of which lies between the provinces of physics and chemistry. Among such investigations may be mentioned his observations on the application of the mechanical theory of heat to the study of volatile liquids and to some simple relations between the latent heats, atomic weights, and tensions of vapours. M. Pictet has also been successful in applying his scientific investigations to practical use, in the perfecting of apparatus for the rapid production of large quantities of ice.

We are glad to know that the health of M. Pictet is not so seriously impaired by overwork as was rumoured a few days ago. On the best authority it is stated that his recovery is by no means beyond hope, and that he is not suffering from incipient softening of the brain. There is no contradiction, however, to the sad statement that the sight of one of M. Pictet's eyes is gone, and that he will probably lose the power of the other.

AFGHANISTAN

WHATEVER opinions our readers may hold as to the present action of the Government with respect to Afghanistan, it may not be considered inappropriate to summarise briefly what we know about a country, which at no remote date may become a part of the British

Empire. A good deal has been written on the country, and some valuable records of the observations of travellers who have been permitted to penetrate it have been published. One of the best sketches of Afghanistan in its various aspects will be found in the magnificent "Noveau Dict. de Géog. Universelle," by Vivien de St. Martin, now being issued by Hachette and Co. On this the present article is mainly based. For further details we would refer our readers to this work, and the long bibliography appended to the article, as well as to a paper in the *Geographical Magazine* for October, and a pretty exhaustive account of the botany of the country in the *Gardener's Chronicle* of November 2, and the works referred to in these two journals.

As a whole, Afghanistan may be regarded as a mountainous table-land. It is the eastern half of the table-land of Iran, of which Persia occupies the western half. On the eastern side, towards Sind, the table-land sinks into what may be considered as parallel chains, forming so many staged terraces; one of these chains, more elevated than the others, and from which shoot several remarkable peaks, is known by the name of the Sulieman Mountains—*Soleimân-Kôh.* The mean height of this transverse chain is about 9,000 or 10,000 feet. The Sulieman Mountains are about sixty miles distant from the Sind border, though the crest of the plateau is not reached for about another 150 miles. On the south side the plateau descends by similar, but perhaps less pronounced gradations, towards the sea; up to the present time the southern slope has been but incompletely explored. To the west the Afghan plateau undergoes a very considerable depression, at the bottom of which lies the marshy lake, Hamoun, into which the Helmand river finds its way; but it again rises in the vast plains which lead towards Farsistan. On the north the plateau falls rapidly towards the plains through which flows the Oxus, and in the north-east it is connected with the highlands of Central Asia and the Himalayas by the enormous mass of the Hindu-Koosh. The highest summits of the latter chain, which conceals the deep basin of the Cabul river, are lost in the regions of eternal snow; many of its peaks exceed 20,000 feet in height. The Khavak Pass, one of the principal passes of the Hindu-Koosh, is at a height of more than 12,000 feet. On the south the basin of the Cabul river is shut in by the Sefid-Kôh chain, which runs, like the Hindu-Koosh, from west to east, and the highest summits of which, to the south-south-east of Jellalabad, reach a height of more than 13,000 feet. The source of the Cabul river is about 8,000 feet above the sea, and of the Helmand, more than 9,000 feet. All this extreme zone of Afghanistan is only a mass of mountains cut by a multitude of rugged valleys; this is the coldest part of the country. A mass which dominates it, above the sources of the Cabul and the Helmand, the Kôh-i-Baba or "Father of Mountains," rising to a height of 15,000 feet, may be regarded as the knot which connects the Hindu-Koosh with the Mountains of Ghour. It is in this cold region, which commences at the Mountains of Ghour and is continued eastward by the long valley of the Cabul river, that the primitive race of the Afghan people have been formed, a race uncultured, rude, and vigorous like their native valleys.

With the exception of the two basins of the Cabul and the Helmand, Afghanistan is characterised by a scarcity of running water. Elphinstone had noticed this prevailing feature, and all subsequent observations have confirmed it. Only four or five rivers of any consequence flow down the long eastern slope of the plateau, and some of these are lost in the sands. The Helmand, fed by many affluents, after a course of about 600 miles, discharges into a large shallow lake, nearly all covered with reeds. This lake, known variously as Hamoun, Meshileh-Seistan, or Meshileh-Rustem, is black and nauseous, changes greatly in its configuration, sometimes even dividing up into two distinct basins.

Four-fifths of the surface of Afghanistan is, on the whole, a country of rocks and mountains, interspersed here and there with picturesque valleys, fertile and well watered, but presenting in several directions only elevated plains, cold, arid, and covered with meagre pasturage, which, however, supports numerous flocks. Situated under the parallels of Egypt and Syria, but with a surface not less varied than that of Switzerland, and mountains much more elevated than the Alps, Afghanistan combines in its climate and its products the extremes of the torrid and temperate zones. As the meagreness of its water-supply is characteristic of many parts of the country, dryness is the dominant feature of its climate, as also extremes of temperature, according to the season.

Of the geology of Afghanistan scarcely anything is known by direct observation. Its mineral products, however, appear to be abundant enough. The sands of the Cabul river are known to furnish gold. The Hindu-Koosh mountains contain silver, copper, cinnabar, lead, antimony, zinc, sulphur, &c. Iron especially abounds there, and copper is found in many other parts of the country; coal also has been proved to exist in several localities. Rock salt has given its name to an entire chain of mountains which stretch into the north of the Punjâb. In the north of Damân, in the neighbourhood of Kohat, are mines of sulphur and wells of naphtha.

The character of the vegetation in all the higher parts of Afghanistan is essentially European; all kinds of fruits abound, and many of them are found in a wild state. From the article on the botany of Afghanistan in the *Gardeners' Chronicle* of November 2, we find the following statement as to the general characteristics of the vegetation of the country:—

"Afghanistan abounds in spiny plants; herbs are to a great extent replaced by dwarf prickly shrubs having a relatively small amount of leaf-surface; and bulbous plants are also numerous. Fragrant flowers prevail, and resiniferous and oleiferous plants are numerous. Stocks, who travelled in Beloochistan and the southern borders of Afghanistan, says the upper region especially is remarkable for the fragrance of its plants, as Artemisia, Perowskia, Salvia, Teucrium, and other Labiatæ, from which cause the flesh of the sheep and goats acquires a fine and almost aromatic flavour. There is no nakedness of the soil, for hill and plain are alike covered with depressed shrubs, although their scorched aspect, after the sun acquires power in June, is anything but agreeable to the eye. The prevailing tint of the indigenous vegetation and of the landscape of the uncultivated plains is olive-green, though the well-cultivated valleys, as Quettah, are charmingly green in the spring time. The aspect of the vegetation of the lower region is far different, the plants being few and scattered over the bare brown and stony soil. Even in spring no annuals appear to diversify the scenery, and the under-shrubs are remarkably similar in external appearance. Woody, stunted, thorny, not above 1 foot high, with round cushion-like outlines, bleached stems, and a few leaves—they look like skeletons of plants, the grey ghosts of a vegetation which has perished of thirst. The glaucous aspect of all, and the universality of spines, are noteworthy features."

Wild animals are not over-abundant. The high valleys of the Hindu-Koosh shelter lions and leopards, which have neither the size nor ferocity of those of India. Wolves are met with, and in the eastern jungles tigers and hyænas. There are two species of bear, a black and a brown, but elephants are not found in the wild state. Jackals and foxes are numerous, monkeys in the north-east, and wild sheep and goats in the mountains of the north. The Angora cat is said to have originally come from Cabul.

There is probably no country of equal extent which

presents so great a mixture of races, though the two main divisions are Afghans proper and Tajiks. The former name is applied to all those tribes that speak Pushtu or Pukhtu, the other to those of the people whose native language is Persian. To the latter are also given the names Parsivân and Deggän or Dekkân. The Afghans are mostly pastoral, while the Tajiks are sedentary and cultivate the soil. To these two great classes must be added the Hindkis, of Hindu origin, and speaking Hindustani, very numerous in the eastern districts; also the Hezareh, a people with Mongol features but speaking Persian, found mainly between Ghazna and Herat. The Kisil-bash or "Red Heads," although not very numerous, are an important factor in the population. They are not Persian by origin, as is commonly stated, but belong to one of the Turcoman tribes of Persia. There has been much discussion as to the origin of the Afghans, no doubt the true aborigines of the country. Some have sought to find in them the descendants of the captive ten tribes of Israel; but this is a mere fanciful Mussulman tradition. Their language, at least, proves that they belong to the great Indo-European family, and that in this family they are specially allied to the Iranian group, but with a linguistic development peculiar to themselves, showing also the influence of Sanscrit. Moreover, it can be conclusively established, both by historical and ethnological evidence, that from the most ancient times the Afghans have inhabited the basin of the Cabul river, which is still the principal seat of the race. The true national name of the Afghan people, the name recognised by themselves, is that of Pashtoûn, Pakhtoûn, or Poukhtanêh. In physical type the Afghans, like all the other peoples of Western Asia, approach the European conformation; the difference being in physiognomy, not in type. Like most other primitive peoples, the social organisation of the Afghans is based on the tribe or clan, and in many respects, it has been said, resembles the old clan system of the Scottish Highlands.

OUR ASTRONOMICAL COLUMN

THE SOLAR ECLIPSE OF 1879, JULY 19.—Though the second of the solar eclipses of next year will not be actually total at any point upon the earth's surface, the difference between the geocentric diameters of sun and moon is sufficiently small to allow of the effect of the augmentation upon the latter, bringing up the phase to one nearly approaching totality in those parts which have the sun close upon the meridian. In the longitude of Aden, or rather, upon the opposite coast of Africa, about Zeyla, the moon's augmented semi-diameter will be only four seconds less than that of the sun, and though the eclipse thus remains annular, it will be seen that the annulus is very narrow in this part of its path—including its passage across Abyssinia. At Aden, there will be a very large eclipse, beginning at 10h. 23m. A.M., local mean time, and ending at 2h. 1m. P.M.; at greatest phase about 0h. 12m., the magnitude will be 0·97 of the sun's diameter. The difference between the illumination of the sky while any portion, no matter how small, of the direct light of the sun remains, and the instant it is entirely extinguished in a total eclipse, is so great as we know from our experience of total eclipses, that there may probably be a doubt as to the possibility of utilising the eclipse in question, in a further endeavour to observe the intra-mercurial planet or planets discovered by Prof. Watson. The next total eclipse of the sun will take place on January 11, 1880, and although, notwithstanding the long track of the shadow across the Pacific Ocean, it may be possible to secure observations, the interval available for so doing cannot be more than half that at the command of observers during the eclipse which traversed the United States last July.

COMETS OF SHORT PERIOD.—An ephemeris of Brorsen's comet, which, according to the calculations of Prof. Schulze, will arrive at perihelion on March 30, 1879, will be found in *Astron. Nach.*, No. 2,220; as already stated in this column, it is likely that the comet will be first observed at the southern observatories. About six weeks later Tempel's comet, 1867 II, which was re-observed in 1873, after undergoing great perturbation from the planet Jupiter, will be due at perihelion; the elements deduced from the observations of 1873 alone, by Dr. Sandberg, would assign, without taking account of perturbation, April 26 for the perihelion passage, but according to an orbit just published by M. Raoul Gautier, of Geneva which he says may be considered the most probable one till the observations to be expected next year, afford additional means of determining the mean motion, the comet would not be in perihelion till May 8, in which case its apparent track in the heavens will differ little from that which it pursued in 1873, when it arrived at its least distance from the sun on May 9. It is pretty sure to be always a faint object except for the larger telescopes, and considering the uncertainty which still appears to exist regarding the mean motion at its last appearance, a close search may be necessary for its rediscovery. M. Gautier is calculating the effect of Jupiter's attraction during the actual revolution, with the intention of publishing an ephemeris in due time; this effect, however, must be small, as the comet has not been nearer to the planet than about 1·5 during the interval. The ensuing return of Faye's comet in the latter part of the year 1880 will take place under much more favourable circumstances for observation than has been the case at any of its appearances since 1843, when it was first detected by the French astronomer; indeed, in 1851, 1858, 1865–66, and 1873 it was always a faint object, but the admirable calculations of Dr. Axel Möller have enabled us to follow its course with extreme precision, with a precision perhaps greater than has yet attended similar investigation in the case of any other comet during so long a period. Though the date of perihelion passage is not yet exactly ascertained, the comet will probably approach almost as near to the earth as in the autumn of 1843, and will be observable for many months.

THE SATURNIAN SATELLITE MIMAS.—According to elements which represent approximately the Washington observations, 1874-77, the following will be times of greatest eastern elongation of Mimas:—

	h.		h.
November 15 at	12·3	November 18 at	8·1
,, 16 ,,	10·9	,, 19 ,,	6·7
,, 17 ,,	9·5		

At these times, on the assumption of circular motion, the distance of the satellite from the end of the ring would be about 8′.

THE MINOR PLANET ISMENE.—According to a calculation of the elements of the small planet *Ismene*, No. 190, by Herr Leman, from observations between September 30 and October 31, it very closely approaches *Hilda*, in its exceptionally great distance from the sun, and consequent length of the period of revolution. We have for comparison:—

	Ismene.	*Hilda.*
Mean distance	3·893	3·950
Aphelion distance	4·466	4·595
Sidereal period in days ...	2·805	2·855
,, ,, years ...	7·68	7·85

Another group is formed by *Cybele*, *Freia*, *Sylvia*, *Camilla*, and *Hermione*, wherein the periods vary from 2297 days to 2377, the mean distance for the group, 3·451. Eventually the planets which approach so near to the orbit of Jupiter, as *Ismene* and *Hilda*, will furnish independent determinations of his mass, though the narrow limits of probable error, within which its value

has been determined by Bessel, Krueger, and Axel Möller, may detract from the importance of further investigation in this direction. Thus the sun's mass exceeds the mass of the planet,

1047·88 times according to Bessel, from elongations of fourth satellite.

1047·54 times according to Krueger, from perturbations of *Themis.*

1047·79 times according to Möller, from perturbations of *Faye's Comet.*

GEOGRAPHICAL NOTES

THE following form the series of scientific lectures to be delivered before the Royal Geographical Society during the present session, in pursuance of the scheme organised by the council two years ago:—"Geographical Evolution," by Prof. Geikie; "The Flora of the European Alps and its Connection with that of other Regions of the Earth," by Mr. John Ball, F.R.S.; and "The Modifications of the External Aspects of Organic Nature produced by Man's Interference," by Prof. Rolleston. The first two will be delivered some time before Easter, next year, and the last probably on the second Monday in May.

IN the absence of Lord Dufferin, the session of the Geographical Society was opened on Monday night by an address from Sir Rutherford Alcock, who reviewed recent exploring work.

AT the opening meeting of the Royal Geographical Society a paper by Signor L. M. d'Albertis, the well-known Italian naturalist, was read, descriptive of his three journeys up the Fly River, and his explorations of other parts of New Guinea. Perhaps the most interesting part of this very interesting paper was that which related to his earlier work when in company with Dr. O. Beccari in 1872. On that occasion they landed first on Sorong Island, between Salwatti and the mainland of New Guinea, in about S. lat. 0° 25′, and after making collections of plants and animals there, they moved on to Andai, near Dorei. During their stay there Signor d'Albertis explored the country to the foot of the high chain of mountains named Lapi Arfak. No one hitherto had been able to penetrate to the Arfak highlands, the home of the bird of paradise, and it is doubtful if the attempt had ever been made, owing to the fear entertained by the coast natives of the mountain tribes. Signor d'Albertis, however, succeeded in accomplishing this feat, for he lived for a month in a Papuan house at a height of 3,600 feet above the sea, and in the course of his daily shooting expeditions reached an altitude of 5,000 feet; so that with pardonable pride he claims to have been the pioneer of the Arfak mountains. Judging by the altitude he attained, he considers that the estimated height of the range—9,000 feet—is no exaggeration. From the point which he reached, 5,000 feet above the sea, the range runs uninterruptedly in a southerly direction, and joins that which constitutes the chief part of the backbone of New Guinea. As far as he could judge, separate streams issue from these ranges, giving origin to many small rivers which disembogue in the two bays known by the name of Geelwink. The mountains, even at the highest point he attained, are clothed with magnificent arboreal vegetation, but he was much astonished to find amongst the trees a species of oak and a conifer, the latter of which was afterwards recognised by Dr. Beccari as an *Araucaria.* Another point is worthy of notice; within a few minutes of the equator, in 134° E. long., all the climates of the world, except the Arctic, are represented, the tropical at the base and the temperate on the upper slopes and summits, both of which offer a rich variety of trees and plants. The same description applies to the neighbouring mountains where exist the most beautiful species of birds of paradise known to the world. In his second expedition Signor D'Albertis spent some time at Yule Island, on the southern coast, near Port Moresby, and he expresses a very decided opinion that this place will be of great importance as a centre of trade in the future.

A LETTER from Mr. Andrew Goldie is published in the *Sydney Morning Herald,* in which he gives some account of a cruise along the south-east coast of New Guinea. Mr. Goldie found the currents and calms a great source of danger. On the way down the coast Mr. Goldie discovered a group of islands (Redlich Group) not marked on the chart, and two splendid harbours, the finest by far that he has seen in New Guinea. He names them Glasgow and Millport harbours, and he has taken soundings and drawn plans, which he intends to forward to the proper quarter. The party visited Cloudy Bay and ascended the Robinson River, taking soundings there and all through the bay, and correcting many errors on the Admiralty chart. They discovered a new river on the west side of Cloudy Bay, which has been named the Blunden. During this trip Mr. Goldie has evidently not overlooked one of the main objects of his being sent to New Guinea, for he has collected 100 fresh skins of birds, different from those in the neighbourhood of Port Moresby, and he has also obtained a large and very valuable collection of curiosities.

THE late Admiral Sir George Back, who was for some time one of the vice-presidents of the Royal Geographical Society, has bequeathed to that body the sum of 600*l.* to be invested in Consolidated Bank Annuities, the conditions attached to the bequest being that the interest shall be paid or applied annually "to or for the benefit of such scientific geographers or discoverers, or person or persons who may then be engaged in discovery or exploration, and in such manner and form as the president and council shall determine." It is further provided that if in any year no person shall be deemed of sufficient merit to receive the prize, the interest shall accumulate and in some succeeding year be awarded to one or more persons who may be considered most deserving, in such proportions as the president and council of the Society may determine. Sir George has also bequeathed to the Royal Geographical Society a very characteristic portrait of himself painted many years ago by Brockeden.

MR. KEITH JOHNSTON, the commander of the expedition despatched by the Committee of the African Exploration Fund to explore the country between the road now being constructed from Dar-es-Salaam, on the east coast of Africa, and the north end of Lake Nyassa, leaves for Zanzibar to-day, in company with Mr. Joseph Thomson, as geologist and naturalist, and great hopes are entertained that, in addition to achieving good geographical results, they will be able to furnish much information respecting the hydrology and geology of the unknown region they are about to explore. Should the financial position of the fund admit of it, Mr. Johnston will extend his explorations northward to Lake Tanganyika, and return to the coast by a different route.

THE arrival at Provincetown, Massachusetts, on October 26, of the Arctic exploration schooner *Florence,* Capt. George Tyson, relieved the anxiety felt for her safety, after her reported departure on September 26, from St. John, New Brunswick. The *Florence* has met with hard usage throughout her whole voyage, and officers and men have suffered considerably. According to the log, the coldest weather experienced was 53° below zero.

M. LIAIS, director of the Rio de Janeiro Observatory, has written to the Paris Geographical Society, intimating that, owing to the liberality of the Emperor of Brazil, he had been enabled to begin the great work of determining by electric telegraph the longitude of Rio in comparison with Greenwich. When the operation shall have been

completed, the geographical position of every city in Southern America will be known with exactitude.

THE Annual Meeting of the Dutch Geographical Society took place at Delft on October 27, when interesting communications were made by the president regarding the exploring expedition sent to Sumatra by the Society. The explorers report having passed through a number of districts which had never been visited before by Europeans, but through the resistance offered by one of the native chieftains, the expedition has now unfortunately been discontinued, and most of its members are on their way back to Holland.

IT is stated that the Russian Minister of Communications will shortly send a special expedition to the Amu Darya district, to describe the new waterway formed by the overflow of that river.

THE *New York Herald* publishes a complete list of positions on the Amazon and Madeira rivers which have been determined by the United States Survey Expedition in the corvette *Enterprise*, Commander Selfridge. They are ninety-two in number. The survey has demonstrated that it is possible for vessels drawing sixteen feet of water to pass during nine months of the year, and by careful navigation during the whole year, up to St. Antonio, on the Madeira. The river is always practicable for vessels drawing only eight feet. The Upper Madeira is not safely navigable except between December and July. Every evening the officers specially charged with the duty landed and ascertained the latitude and longitude of the halting-place, with reference also to its bearings with respect to certain conspicuous stars north and south, east and west. Six careful sets of observations, at intervals, were made to determine the rate of the chronometer. The charts compiled are to be reduced and published at Washington.

NOTES

WE take the following from the *Times*:—At the meeting of the Council of the Royal Society on Thursday last, the following were nominated as council and officers for the year ensuing to be proposed for election at the anniversary meeting of the Society, which will be held on St. Andrew's Day, the 30th instant:—President, William Spottiswoode, M.A., LL.D.; Treasurer, John Evans, F.G.S., V.P.S.A.; Secretaries, Prof. George Gabriel Stokes, M.A., D.C.L., LL.D., and Prof. Thomas Henry Huxley, LL.D.; Foreign Secretary, Prof. Alexander William Williamson, Ph.D. Other Members of the Council—Frederick A. Abel, C.B., V.P.C.S., William Bowman, F.R.C.S., William Carruthers, F.L.S., Major-Gen. Henry Clerk, R.A., William Crookes, V.P.C.S., Sir William Robert Grove, M.A., Augustus G. Vernon Harcourt, F.C.S., Sir Joseph Dalton Hooker, C.B., K.C.S.I., D.C.L., Vice-Admiral Sir Astley Cooper Key, K.C.B., Lieut.-Gen. Sir Henry Lefroy, C.B., Lord Lindsay, P.R.A.S., Sir John Lubbock, V.P.L.S, Lord Rayleigh, M.A., Charles William Siemens, D.C.L., John Simon, C.B., D.C.L., Prof. Allen Thomson, M.D., F.R.S.E. It will be remarked that Sir Joseph Hooker has carried out his intention of retiring from the presidency.

PROF. WÜRTZ delivered his Faraday Lecture on Tuesday evening at the Royal Institution, and was entertained at dinner last night at Willis's Rooms. We hope next week to give a full account of the proceedings on both occasions.

PROF. GYLDÉN, Director of the Stockholm Observatory, has received the Cöthenius Medal of the German Leopold-Caroline Society of Science, for his important researches in astronomy.

WE understand that, at a meeting of the Professors of Queen's College, Cork, it was resolved to erect a memorial to the late Prof. Harkness, in the form of a stained glass window, in the Examination Hall of the College. It is understood that the friends of the late professor in that city and elsewhere will be invited to co-operate in raising the funds necessary for this purpose.

DR. O. FINSCH, the well-known Bremen naturalist, is about to start on a scientific tour to the Polynesian Seas; the expenses of the tour will be defrayed by the Berlin Humboldt Institution, and Dr. Finsch travels at the special request of the Berlin Academy of Sciences.

OUR Paris correspondent writes that the Werdermann electric light has attracted much attention there, and will very shortly be tried at the office of the *Temps*. The Jablochkoff light is still in operation at the Avenue de l'Opéra, but will be stopped at the end of this month, unless a new arrangement as to cost can be come to. Indeed, our correspondent informs us, unless the present price of this light is considerably modified, it is not likely to keep its place.

AMONG the latest news about the progress of electric lighting is an account of an interview with Mr. Edison, given in the *New York Sun*. His Electric Light Company proposes to light the public buildings and private residences of New York with electric lights. The electricity would be made by twenty or more engines, stationed in different parts of the city. Each station would have an engine and several electric generating agencies. He thinks that the engines will be powerful enough to furnish light to all houses within a circle of half a mile. He passes the wires right through the gas-pipes, and brings them into the houses. "All that will be necessary will be to remove the gas burners, and substitute electric burners. The light can be regulated by a screw the same as gas. He does not pretend that it will give a much better light than gas, but it will be whiter and steadier than any known light; nor does he know now that it will be cheaper than gas. To the question as to whether he could measure the amount of electricity used, Mr. Edison said he had made no attempt to discover a meter. "I know that it can be measured, but it may take some to find out how. I propose that a man pay so much for so many burners whether he uses them or not. If I find that this works an injustice why I shall try to get up a meter, but I fear it will be very hard to do it." Mr. Edison says, according to the *Sun*, that "electric generating machines could be placed upon steamboats and locomotives, and the boats and cars lighted by the action of the engines, but the instant that the machinery stopped the lights would go out." Country towns, with the use of the electric generating machines, could be lighted by water power. Any power could be used provided it was strong enough to turn the shaft of the machine with the necessary rapidity. In an article on the subject of Electric Lighting in yesterday's *Times* an account is given of an exhibition of a new electric light by the Electro-Dynamic Light Company of New York on the 29th ult. It is described as a very simple affair, consisting of a small pencil of carbon a little larger than an ordinary pin, connected by wires with an electric machine, and inclosed in a hermetically sealed glass globe, which is filled with pure nitrogen gas. The pencil of carbon is heated by the electric current to a temperature of from 30,000° to 50,000° Fahrenheit. In an atmosphere with which it cannot chemically combine the carbon is practically indestructible, and the light is therefore produced without any consumption of material. In the experiments made five lights were placed in different parts of the darkened room, and all were connected by wires with a small electric machine. In an adjoining room a simple key was placed in one of the three ordinary keyholes in one of the walls and turned a little. Two of the burners attached to a hanging chandelier in the centre of the room immediately glowed faintly, and as the key was turned still further around the glow increased until a brilliant and perfectly steady white light was obtained,

equal to the light of twelve ordinary gas jets. The key was then turned to another of the keyholes, and another of the lamps was lighted up. In the same way the fourth and fifth burners were ignited, and there resulted an exceedingly brilliant white light, yet so soft and steady that it did not pain the eyes. The lights were easily turned to any desired degree of brilliancy —from that of a mere spark to a light of six times the intensity of the common gas jet, that being the *maximum* power of the lights in use. The company asserts its ability to easily fit up lights equal to thirty gas-burners. By a very simple "switch" in the wall the current of electricity is divided and subdivided to supply any number of burners desired, the electricity reaching the switch from the generator through a single wire. The light is turned on or off or regulated to any degree merely by turning a key which operates upon the switch. The plan is similar to that proposed by Edison. The difficulty of a meter has been overcome by the invention of a meter which will record the number of burners used in any given house and the number of hours each burner is lighted.

THE Institution of Civil Engineers have, as usual, issued a circular inviting communications on a number of subjects connected with their department; for such as meet with their approval they award several valuable prizes. The circular may be obtained by applying to 25, Great George Street, Westminster.

PROF. S. P. THOMPSON lectured to an audience of 2,600 persons in the Colston Hall, Bristol, on November 8, on the Electric Light. The Jablochkoff candle and Wallace lamp were amongst the systems shown in operation. The lecture is to be repeated on the 13th inst. to the working-men of Bristol.

WE are glad to notice a little work which is being done at Winchester, and which deserves commendation as a step in the right direction by a corporation. Mr. P. S. Abraham, M.A., B.Sc., who has for two months been engaged in naming, arranging, and cataloguing the different objects in the Winchester City Museum, has sent in a report to the Committee, from which we glean the following:—The zoological portion of the museum contains some 1,700 objects, which have been labelled and placed in their proper scientific positions by Mr. Abraham. This number is made up of 70 mammalian animals, 430 birds, 50 reptiles and frogs, 80 fishes, 50 specimens of corals, sponges, &c., and about 1,000 molluscs. There are, in addition, a few specimens of articulated animals, and a small cabinet of insects. There is also a valuable collection of lichens—many from Hampshire—and comprising 227 varieties. In the mineralogical room Mr. Abraham has classified, labelled, and arranged two large cases of minerals, which now contain above 1,200 specimens, one case of rocks, with 300 specimens, and two cases of fossils containing nearly 1,300 specimens. There are, besides, large and valuable collections in various other departments, though Mr. Abraham states they are very insecurely cased. We hope the Corporation will adopt his recommendations. He observes that the museum contains an excellent nucleus of specimens. The collection could be easily improved and enlarged by a judicious curator, by the exchange of duplicate specimens, of which there are many, and by filling up gaps by purchase of the wanting representative forms. In this manner, and without very great expense, the museum might be made well worthy of the city of Winchester. The Committee conveyed to Mr. Abraham their "unanimous expression of approval at the able manner in which he has arranged, labelled, and catalogued the collection in the City Museum."

VIOLENT volcanic eruptions are reported from several of the Aleutian Isles in the North Pacific. The news was brought to Honolulu by whalers returning from the Arctic Seas through Behring's Straits. The high volcanoes upon Amukta and Tshegula sent forth gigantic columns of smoke and copious streams of lava, and the same was the case with the mountain upon Umnak, which reaches a height of 2,800 metres. On the island of Unalashka an earthquake accompanied by a tidal wave totally destroyed the village of Makushin on August 29.

A LARGE sea- and fresh-water aquarium is now in course of construction at Leipzig. It will consist of about twenty tanks, of which nine are to contain marine animals. The capacity of the tanks will vary from 350 to 800 litres.

REMARKABLE discoveries of Roman structures have recently been made at Bonn, on the Rhine. It has been found that the Castrum the Romans had established there by far exceeded in extent and importance the celebrated Saalburg near Homburg. Unfortunately the directors of the Provincial Museum of Bonn, by whose orders the excavations were begun, were not able to acquire the ground upon which the discovery was made, so that archæologists must remain satisfied with the mere fact of the discovery and the measurements taken.

NEAR the Norwegian town of Hamar, at a farm called Storhammer, some 170 silver coins have been found in the ground, none of which are dated later than the year 1530. They are all of Danish, Swedish, or Norwegian origin. At the same time two silver spoons, a gold ring, and several small silver hooks, were found. All the objects have been purchased by the Christiania Museum.

IN 1880 there will be an international exhibition of sea and river fishing-tackle at Berlin. The programme of the exhibition, which will contain no less than nine subdivisions, will shortly be sent to all interested in fisheries, both in Germany and abroad.

FROM the twenty-second annual report of the Sheffield Free Libraries and Museum we are glad to see that in the reference department a large increase has taken place in the demand for books in the class of arts and science. Considerable additions have been made to the museum.

THE *Natural History Journal*, "conducted by the Societies in Friends' Schools," for October 15 contains several interesting papers on various scientific subjects. A new feature is the illustrations, which have been made by an ingenious, simple, and inexpensive process, described in the first paper. One of these, "Societies in Friends' Schools," is the Lisburn School Association, which sends us a very favourable "Fourth Annual Report." We have also received a creditable Annual Report (the eleventh) from the Metropolitan Scientific Association, which meets at the Ward Schools, Aldersgate Street, on the fourth Tuesday in each month.

WE have received a well-arranged list of lectures to be given in connection with the Hull Literary and Philosophical Society, in which scientific subjects are given a large place.

THE *Gardener's Chronicle* publishes the following abstract from a letter recently received from Dr. Beccari, from Sumatra:—"I have very little time to spare, only to tell you of a botanical discovery which I think is of some interest. It is a gigantic Aroid, which can only be compared with the Godwinia discovered by Seemann in Nicaragua. I have no books with me, and I am not able to ascertain the genus to which it belongs, especially as I have seen it only in fruit. I believe it to be a Conophallus, and if so, I propose to name it *Conophallus titanum*. The tuber of a plant that I dug up is 1·40 m. in circumference. Two men could hardly carry it; they fell down and the tuber was broken. I will secure some more, and I hope to be able to forward them to Florence in good state. Meanwhile I send you some seeds. From this tuber, as in the genus Amorphophallus, only one leaf is produced, which in form and segments does not much differ from those of the above-named genus. But what different dimensions! The stalk at the base was 90 ctm. in girth, it was slightly attenuated

at the apex, and reached the height of 3·50 m.; its surface was smooth, of a green colour, with numerous small, nearly orbicular dots, of a white colour. The three branches into which it was divided at the top were each as large as a man's thigh, and were divided several times, forming altogether a frond not less than 3·10 m. long. The whole leaf covered an area of 15 m. in circumference. The spadix of a plant that I found in fruit had the dimensions of the stalk already described; the fruit-bearing portion was cylindrical, 75 ctm. in girth, 50 ctm. long, and was densely covered with olive-shaped fruits 35–40 mm. long and 35 mm. in diameter, of a bright red colour, each containing two seeds." This letter was addressed to the Marquis B. Corsi-Salviati, who received at the same time a number of seeds of this gigantic novelty. Many of them have germinated, so that the species is secured to European collections.

THE Giffard great captive balloon was disinflated last week without accident. The following statistics in connection with the enterprise may be interesting. The Tuileries grounds were opened to the public during 100 days, but the balloon was unable to work owing to the state of the atmosphere during 30 days. The number of ascents was 1,023, the number of passengers 34,000. The number of pioneer balloons sent up 25. During this period the sum of 840,000 francs was collected. The expenses of building the balloons, of machinery, and working, reached about 500,000 francs, so that the enterprise was a financial success.

THE *Daily News* Quebec correspondent telegraphs as follows:—"After long study and many experiments Prof. Bell has made an important discovery in connection with the telephone. It is well known that the telephone has been a comparative failure in England on account of the fatal induction generated by the contiguity of other wires. Prof. Bell has discovered simple and efficacious means whereby not only is induction prevented, but the clearness and force of the telephonic vocalisation greatly increased. Prof. Bell tells me that practical demonstration of the importance of the discovery will be given in London as soon as the necessary preliminaries are complete." We give this statement as published in the *Daily News*, though it must be received with some caution.

A NATIVE Japanese paper states that besides the two docks already existing at Yokoska, a third is to be built which will accommodate the largest ships on the Pacific. It is expected that it will be completed within three years, and the probable cost will be about 55,000*l*.

FROM Stuttgart (E. Schweizerbart'sche Verlagsbuchhandlung) is announced a second collected edition of Mr. Darwin's works in twelve volumes, with 326 woodcuts, seven photographs, twelve charts and tables, and a portrait of the author on copper-plate.

THE publishing house of Hartleben, in Vienna, to mark the seventy-fifth year of its existence, have issued a neatly-printed catalogue of all the works issued by them from 1803 to the present year. A very large proportion of these belong to the various departments of science. The catalogue contains a portrait of the founder of the house, and a sketch of its history.

FROM Mr. Murton's annual report on the Botanical and Zoological Gardens at Singapore we learn that, both botanically and zoologically, the Gardens are in a flourishing state. One of the most important sections of the garden, namely, that devoted to the cultivation of economic plants, continues to receive a large share of attention, patches of Liberian, Cape Coast, and Arabian coffee have been planted, as well as cocoa, China and Assam tea, and ipecacuanha. *Castilloa elastica* and *Manihot Glaziovii*, both valuable rubber, or caoutchouc yielding plants, natives of South America, as well as the Sarsaparilla, Alligator pear (*Persea gratissima*), New Zealand Flax (*Phormium tenax*), Cubeb (*Piper Cubeba*), Camphor (*Camphora officinarum*), Allspice (*Eugenia Pimenta*), and many others, have been introduced. The Liberian coffee plants, sent to Larut in 1875, are reported to be making good growth with large healthy foliage, forming a great contrast to the Arabian coffee growing beside it. The Salt Bush, which is referred to two species of *Rhagodia*, namely, *R. hastata* and *R. parabolica*, has been introduced into Singapore. Both species are described as possessing wholesome and nutritious qualities, and are much relished by stock. The cultivation of the plants is said to be very easy, and in consequence of the rapidity with which, when protected from stock, they grow into large and handsome shrubs, together with their capability of resisting both heat and drought, are strongly recommended for cultivation. Besides the use as fodder, in a fresh state, the plants might also be advantageously given to sheep and cattle after being cut and dried, or in conjunction with other foods. In Singapore the plants seem better able to resist heat and drought than prolonged wet weather, but it is anticipated that, when they become well established, they will survive the effects of wet weather better. Mr. Murton states that the object that will be kept steadily in view in the working of the new economic garden is the introduction of new plants of economic value, and thoroughly testing their capabilities of production in Singapore before recommending them for general cultivation, while it will also afford an opportunity to intending planters in the Malay Peninsula of seeing the various plants adapted for their cultivation, and the amount of success, or otherwise, attending each operation. In the Zoological department little change has been affected to call for note.

THE additions to the Zoological Society's Gardens during the past week include two Arabian Baboons (*Cynocephalus hamadryas*), from Arabia, presented by Mr. C. Wood; two Squirrel-like Phalangers (*Belideus sciureus*) from Australia, presented by Mr. E. S. Waller; two Hooded Crows (*Corvus cornix*), European, presented by Capt. F. H. Salvin; a Burchell's Zebra (*Equus burchelli*) from South Africa, a Yellow-Shouldered Amazon (*Chrysotes ochroptera*), a Blue and Yellow Macaw (*Ara macaco*) from South America, a Dalmatian Dog (*Canis familiaris*), a Passerine Owl (*Glaucidium passerinum*); two Variegated Sheldrakes (*Tadorna variegata*) from New Zealand, six Summer Ducks (*Aix sponsa*) from North America, two Scarlet Tanagers (*Ramphocœlus brasiliensis*) from Brazil, two Grenadier Weaver Birds (*Euplectes onyx*) from West Africa, two Java Sparrows (*Padda oryzivora*) from Java, two Domestic Fowls (*Gallus domesticus*) from Japan, deposited; a Nisnas Monkey (*Cercopithecus pyrrhonotus*) from Nubia, a Red-Fronted Lemur (*Lemur rufifrons*) from Madagascar, a Mantchurian Deer (*Cervus mantchuricus*) from Japan, an American Tantalus (*Tantalus loculator*) from South America, purchased.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

AN influential meeting was held last week in Liverpool to consider the report of a committee appointed at a previous meeting to draw up a scheme for the establishment of a college for higher education in Liverpool. The committee have arrived at the following conclusion: that the most appropriate name will be "University College, Liverpool," unless the name of a founder or large benefactor be adopted. The smallest staff consistent with the objects should be composed of at least seven professors and two lecturers, allotted as follows:—Professorships—Mathematics and experimental physics; classical literature and history; engineering, practical mechanics, and steam; logic, mental and moral philosophy, and political economy; modern literature and history; chemistry, natural history (including botany, zoology, and geology). Lectureships—Jurisprudence and law; physiology. The stipend of each professor should be reckoned at 300*l*. per annum (exclusive of a share of

the fees), which is about the average of the stipends in the colleges recently established; and that of each lecturer at 150*l.* per annum. A further sum will be required for class expenses and for the general expenses of the college. A college consisting of the staff recommended would therefore require a permanent income of at least 3,000*l.* per annnm, necessitating a capital of 75,000*l.* This estimate does not include the cost of erecting any building for the purposes of the college, or the rent which might have to be paid for the necessary accommodation pending such erection. The committee suggest the desirability of deferring for the present the question of the government of the college, and they recommend that the management be placed in the meantime in the hands of a committee to be appointed by the adjourned town's meeting. The report was adopted and a committee appointed to carry out its objects.

A MEETING was held on Monday, in the City, for the purpose of formally constituting the "City and Guilds of London Institute for the Advancement of Technical Education." The meeting was, in fact, the first held by the Board of Governors which the provisional committee of the Guilds had recommended should be constituted as the supreme governing body of the new institute. This body consists of representatives from the subscribing Livery Companies, nominees from the Court of Common Council, the Lord Mayor, and other City officials, with a president and twelve vice-presidents. The amount of available income already promised is over 12,000*l.*, but it is anticipated that as soon as any actual progress is made in the work, contributions will be given by the companies who have not yet joined in the scheme. The proposals which the committee have before them include the establishment in London of a central technical school, the establishment and assistance of evening classes, trade schools, &c., and the development of a system of technical examinations such as that now carried on by the Society of Arts. All these proposals were made in the provisional committee's reporr, and it was proposed to carry them all into execution as soon as sufficient funds were obtained. It was stated at the meeting on Monday that the Commissioners of the 1851 Exhibition were proposing to erect a building at South Kensington in which would be included a technical school, and it was, therefore, understood that either some arrangement would be come to with them or the execution of the proposals connected with the London school would be deferred till it was definitely known what direction the action of the Commissioners would be likely to take. It may be assumed, therefore, that the proposals of the executive committee will embody the other recommendations of the provisional committee, and will include a detailed scheme for carrying them out.

THE New South Wales correspondent of the *Colonies* states that, in consideration of the necessity which is now felt] for extending the curriculum of Sydney University and augmenting its teaching powers, the Colonial Government have consented to ask Parliament for an additional annual grant of 5,000*l.* This will enable the Senate to make the following additions to the present course of study:—Mental philosophy, law, history, and English literature; (2) all the education necessary for the medical profession; (3) a complete course of natural philosophy, coupled with mechanics and engineering; (4) the addition of organic chemistry and metallurgy to the chemical school; and (5) biology, including animal and vegetable physiology. The Senate will also be in a position to establish a faculty of science, and to confer the degrees of Bachelor and Doctor of Science, and also degrees in medicine, on those who have received their education in Sydney.

WE have received a "Calendar" of Anderson's College, Glasgow, containing much information as to the founder and the curriculum of that useful institution. It shows that a very complete and thorough education may be obtained there at a very moderate cost.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, November 5.—Mr. A. Grote, vice-president in the chair.—A communication received from Mr. J. H. Gurney, F.Z.S., contained a memorandum from the late Mr. E. C. Buxton, stating that *Asturinula monogrammica*, observed on the Eastern Coast of Africa, had a song which was heard morning and evening.—An extract was read from a letter addressed to the Secretary by Dr. A. B. Meyer, C.M.Z.S., respecting a supposed new bird of paradise, obtained on the West Coast of New Guinea.—An extract was read from a letter addressed to the Marquis of Tweeddale by Mr. A. H. Everett, stating that the anoa of Celebes (*Anoa depressicorms*), or an allied species, was found in the Island of Mindoro, Philippines.—Prof. Newton, F.R.S., exhibited and made remarks on a supposed hybrid between the red grouse and ptarmigan, lately shot in Sutherland by Capt. Houston.—A communication was read from Mr. R. Bowdler Sharpe, F.Z.S., containing a description of a new species of *Indicator*, with remarks on other species of the genus.—A second paper by Mr. Sharpe contained a note on *Pœoptera lugubris*.—A communication was read from Mr. G. B. Sowerby, Jun., wherein he gave the descriptions of ten new species of shells from various localities.—Mr. A. G. Butler, F.Z.S., read a paper in which he gave the description of a remarkable new spider, obtained in Madagascar by the Rev. W. D. Cowan, for which the name of *Cœrostris avernalis* was proposed.—A communication was read from Lt.-Col. R. H. Beddome, C.M.Z.S., containlng the description of six supposed new species of snakes of the genus *Silybura*, family Uropeltidæ, from the Peninsula of India.—A communication was read from Mr. Edgar A. Smith, F.Z.S., containing the description of a collection of marine shells, made by Capt. L. W. Wilmer, in the Andaman Islands.—Mr. F. Moore, F.Z.S., communicated a list of the lepidopterous insects collected by Mr. Ossian Limborg in Upper Tenasserim, with descriptions of new species.—Mr. George French Angas, C.M.Z.S., gave the descriptions of six species of bivalve shells in the collection of Mr. Sylvanus Hanley, F.L.S., and of a *Helix* from the Solomon Islands. Mr. Angas also read descriptions of ten species of Marine Shells from the Province of South Australia. Mr. Angas likewise read a list of additional species of marine mollusca to be included in the fauna of the Province of South Australia, with notes on their habitats and local distribution, in continuation of former papers on this subject.—Dr. G. E. Dobson read a note on *Myxopoda aurita*, a new form of chiroptera from Madagascar, remarkable for possessing suctorial disks, as in *Thryroptera*. Mr. Dobson also gave descriptions of some new or rare species of bats based on specimens in the Museum of Natural History of Paris. To the new species the following names were given:—*Pteropus germaini* from New Caledonia, *Cephalotes minor* from New Guinea, *Emballonura raffrayana* from Gibolo, and *Schizostoma brachyote* from Cayenne.

CAMBRIDGE

Philosophical Society, October 28.—Annual General Meeting, Prof. Liveing, president, in the chair.—The following were elected Officers and new Members of Council for the ensuing year.—President, Prof. Liveing; Vice-Presidents, Prof. Stokes, Prof. Newton, and Prof. Clerk Maxwell. Treasurer, Dr. J. B. Pearson. Secretaries, Mr. J. W. Clark, Mr. Coutts Trotter, and Mr. J. W. L. Glaisher. New Members of Council, Prof. Humphrey, Prof. Cayley, Mr. W. M. Hicks.—Prof Cayley made a communication to the Society upon the transformation of co-ordinates. He investigated the formulæ for the transformation between two sets of oblique co-ordinates in three dimensions, which, when presented in the notation of matrices, assumed a very elegant form. The paper also contained developments relating to certain expressions that were involved in the transformation.—Mr. J. W. L. Glaisher made a communication to the Society on Henry Goodwyn's "Tabular Series of Decimal Quotients" and "Table of Circles" (London, 1823). The first contains the value, to eight decimal places, of every vulgar fraction, whose numerator and denominator, when the fraction is expressed in its lowest terms, do not exceed 1000. This table, in which the fractions are arranged in order of magnitude, was intended to extend to $\frac{1}{2}$, but only the first part, which ends at $\frac{99}{991}$, was published. The "Table of Circles" contains all the complete periods corresponding to the denominators, prime to 10, up to 1024. The object of the tables was the conversion of vulgar fractions into decimals, the complete quotients being shown. In the first table the fractions are arranged in order of magnitude, and Mr. Goodwyn was thus led to a remarkable theorem, viz., that if all the fractions in their lowest terms having their numerators and denominators both not exceeding a given quantity n be arranged in order of magnitude, then each fraction is equal to the fraction formed by adding together the two numerators and the two denominators

of the fractions on each side of it. Thus if $n=5$, the fractions are $\frac{1}{5}$, $\frac{1}{4}$, $\frac{1}{3}$, $\frac{2}{5}$, $\frac{1}{2}$, $\frac{3}{5}$, $\frac{2}{3}$, $\frac{3}{4}$ $\frac{4}{5}$, and, for example, $\frac{1}{3}=\frac{1+2}{4+5}$; also the difference between any two consecutive fractions is equal to unity divided by the product of their denominators. These properties, discovered by Mr. Goodwyn, were afterwards proved by Cauchy. Mr. Glaisher pointed out the great convenience of the arrangement of the periods in Goodwyn's tables, and exhibited a table showing the number of periods corresponding to every denominator up to 1000, and the number of figures in each period. This table was obtained by actual counting from Goodwyn, and in every instance the product of the number of figures in each period and the number of periods was found to be equal to the number of numbers less than the denominator and prime to it, as should be the case. After alluding to other similar tables, and to tables by Gauss, Reuschle, Desmarest, Shanks, &c., reference was made to the fact discovered by Desmarest that the number of figures in the period of the reciprocal of 487^2 is the same as the number of figures in the period of the reciprocal of 487, or in other words, $10^{486}\equiv 1$ (mod 487^2). In vol. iii. of *Crelle* Abel proposed the query, "Can $x^{\mu-1}\equiv 1$ (mod μ^2), if μ be a prime and x less than μ?" Jacobi replied and showed that $3^{10}\equiv 1$ (mod 11^2), $14^{28}\equiv 1$ (mod 29^2) and $18^{36}\equiv 1$ (mod 37^2). The case found by Desmarest is the only one known in which the conditions of Abel's question are satisfied for $x=10$; in fact we have $10^2\equiv 1$ (mod 3^2) and $10^{486}\equiv 1$ (mod 487^2), and there is no other known case in which $10^{p-1}\equiv 1$ (mod p^2), p being a prime, although there is no reason to suppose that such cases do not exist, and that there is not some value of p for which $10^{p-1}\equiv 1$ (mod p^3). Desmarest has verified that for values of p less than 1000 the congruence $10^{p-1}\equiv 1$ (mod p^2) is only satisfied for $p=3$ and $p=487$. Mr. Glaisher also exhibited the first fourteen printed pages of the factor table for the fourth million, an account of the construction of which was communicated to the Society on February 11, 1878.

PARIS

Academy of Sciences, November 4.—M. Fizeau in the chair.—The following papers were read:—Researches on the stability of the ground and of the vertical of Paris Observatory, by M. Mouchez. M. Wolf is occupied with the former question. M. Gaillot has been studying the latitude given at different epochs. The variations of a few tenths of a second in this, at different times of the year, are thought due to the influence of temperature either on the instruments, or (rather) on the astronomical refractions, whose coefficient has not yet been adequately determined; or they may be due to a systematic error of the declination of stars distributed regularly over the twenty-four hours of right ascension, these hypotheses being more admissible than that of a variation of the vertical.—On the reciprocal displacements between oxygen, sulphur, and halogen elements, combined with hydrogen, by M. Berthelot.—Reciprocal displacements between weak acids, by the same. Two weak acids opposed to each other divide the base, the division being regulated by the state of partial decomposition of the two salts dissolved, which depends both on the proportion of water and on that of the corresponding acid.—On the reaction between mercury and hydrochloric gas, by M. Berthelot. 13·5 gr. of mercury and 48 cub. ctm. of pure hydrochloric gas put in a very resistant sealed glass tube and heated to the highest possible temperature for an hour, yielded a little over 1 cub. ctm. of hydrogen, indicating decomposition of about one-twentieth of the hydrochloric gas.—Preliminary note on the compound nature of the chemical elements, by Mr. Lockyer. Besides calcium, several substances considered as elements are compounds.—On the native iron of Greenland and the basalt containing it, by Prof. Lawrence Smith. He gives an analysis of a memoir on the subject. He is convinced the iron is of terrestrial origin, and in many cases so intimately united with basalt that the felspathic and other crystals of the latter penetrate the iron particles. The iron is probably a secondary product formed by decomposing action of beds of lignite and other organic matters which the immense basaltic dykes have penetrated.—On a universal law relative to the dilatation of bodies, by M. Levy, a reply to objections.—On the maturation of the grain of ergot. The substance which plays the part of sugar in this grain, the author finds identical with *synanthrose*, the saccharine matter found in *Synanthereæ*, and more especially in Jerusalem artichokes. It is the only saccharine matter present, and it diminishes rapidly in proportion as maturation advances (but does not wholly disappear), being replaced by starch, formed doubtless at its expense. Wheat, oats, barley, and maize, do not contain synanthrose, but cane-sugar. Thus one may readily detect in flour the fraudulent addition of ergot flour.—On the dangers of the use of methylic alcohol in industry, by M. Poincaré. Animals kept eight to sixteen months in air, ever renewed, but charged with vapours of methylic alcohol, undergo hypertrophy and fatty degeneration of the liver, a like alteration of the muscular fibres of the heart, epithelial cells, uriniferous tubes, and the lung cells, also congestion of the nervous centres, &c.—M. Gelis stated that, owing to large demand, he proposed manufacturing 200,000 kilogrammes of sulphocarbonate of potassium (for phylloxera) for the coming year, and he desired the Academy to obtain from the railway companies reduced prices of transport.—Mr. Warton presented a marine compass with nickel needles.—On the direction of the vertical of Paris Observatory, by M. Gaillot. See first paper.—On a simple property, characterising the mode of distribution of weight of a solid, placed on an elastic horizontal ground, between different parts of its base, when the latter is a horizontal ellipse, by M. Boussinesq.—On certain ordinate series with reference to powers of a variable, by M. Appell.—On the rectification of a class of curves of the fourth order, by M. Darboux.—On an iodised derivative of camphor, by M. Aller. The formula is $C_{10}H_{15}IO$.—On the region of the solar spectrum indispensable to vegetable life, by M. Bert. The part thus necessary to life is that between the lines B and C; but it is not sufficient; for behind red glass plants may live, indeed, long, but they get elongated to excess and slender, with narrow and little-coloured foliar limbs; the blue and violet rays rectify this.—On relations presented by phenomena of motion proper to reproductive organs of some phanerogams with cross and direct fertilisation, by M. Hæckel. Motion provoked in both male and female organs seems to serve physiologically for cross fertilisation, while spontaneous motion assumes direct fertilisation in plants which are not sensibly profited by crossing. The former oftener characterises the more highly-organised plants, the latter seems proper to the less highly-organised.—Reproduction of felspars by fusion and prolonged maintenance at a temperature near that of fusion, by MM. Fouqué and Levy. The experiments here described were on oligoclase, labrador, and albite.—On two specimens of natural crystals of sulphate of magnesia (epsomite) of remarkable dimensions, by M. De Rouville.

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THURSDAY, NOVEMBER 21, 1878

MATHEMATICS AT CAMBRIDGE.

THE Cambridge Examination for Mathematical Honours has for a long time enjoyed a high reputation, especially among Cambridge men, who have been accustomed to point to it as the model of what an examination should be. The credit thus claimed has been in past times more or less deserved, but to what extent it is so now is a question on which there may be variety of opinion. Like every other institution, the practical usefulness of which depends upon the ease with which it can adjust itself to external conditions, the Tripos examination must undergo changes to meet corresponding changes from the outside; and there may come a time when the external conditions operate so powerfully that mere modifications are insufficient, and when the changes made must be both radical and extensive. The old Tripos system has recently been put to a severe strain, and it is admitted on all hands that the result has proved unsatisfactory. We propose to inquire into the causes which have brought this about, and to discuss the measures by which it is hoped the evil will be met.

It may be laid down as a fundamental axiom that a university honours examination should be in harmony with the studies of the candidates, and that it should be reasonable in its demands upon them. What the very best students may be expected to answer after faithful work during their undergraduate course should clearly be taken as a guide in fixing a superior limit to the number and difficulty of the questions. Nor should this estimate be pitched too high, because there are other subjects of interest and study besides mathematics, in which it is desirable that even the best of young mathematicians should engage, and an education based on mathematics alone must necessarily be defective.

If we accept these propositions we must admit that the estimate of what is reasonable towards the candidates was very different fifty years ago to what it is now. One cannot help looking back with regretful eyes on the Tripos questions of that time, so remarkable for their simplicity and elegance, as well as for the happy appreciation of the degree of difficulty such questions should possess. The questions of more recent times, although they are often to be admired from an æsthetic point of view, are in many instances far beyond the reach of any but the very best men, and may be described as being somewhat too difficult and elaborate.

There are obvious reasons why, as time goes on, the questions should become more difficult; still it would be interesting to trace the changes over some considerable period, so as to be able to explain how the Tripos examination has reached its present form. The changes must have been of a gradual character, because the traditions and customs of the examination have been faithfully transmitted from one set of examiners to the next. One can see, however, that if any particular person were to examine often, as has occasionally happened, or if several persons of like tastes were to examine together, we should find the questions displaying a particular bias. When this should occur, the studies of the candidates would receive the same bias, and particular branches of mathematics would thus be pushed for a time into undue importance. So much was this the case about the year 1864 that Sir G. Airy, when delivering the Rede lecture in the Senate House, went out of his way to denounce the excessive attention given in the University to certain branches of pure mathematics.

We can thus imagine the Tripos examination based upon the traditions of its predecessors, yet continuing to grow both in extent and difficulty, and with some of its features perhaps somewhat exaggerated. In 1873 a great change had to be made. The University, feeling it was not creditable to it that so little encouragement should be given to the higher branches, and especially to the great modern subjects of mathematical physics, determined that those subjects should be introduced into the Tripos examination. The additions thus made, besides considerable extensions in the subjects already existing, included, amongst others, Elliptic Functions, Electricity, Magnetism, and Heat.

It was thought that in thus extending the examination the students would have a choice of subjects, and that their course of study would thereby be rendered more interesting than it was before. It was certainly never intended that the burdens under which the Tripos candidate was already staggering, should be increased upon him. But how were these changes met on the part of the students and on the part of the teachers? The immediate result seemed to be that the best candidates attempted to know something of all the subjects. The examiners in 1874 were careful to watch for indications as to whether the candidates had devoted themselves to special groups, but they reported that there was no evidence to that effect. The University thereupon passed a Grace enacting that the number of questions in the higher subjects should be increased, the object being to supply a sufficient variety so that a candidate who had confined his studies to a limited group might reasonably expect plenty to occupy him in the examination. This provision has continued operative down to the present time, but it does not appear to have produced the salutary effect intended. There is much reason to fear that the best candidates still push their way through most of the subjects, whilst the next best struggle as far in the same direction as they can.

There is then a well-defined evil to be remedied. For no one can deem it a good education where the student is carried, necessarily with rapidity, over a variety of subjects, many of which he must therefore very imperfectly comprehend. What changes in the examination are proposed to the University as a remedy we will presently describe. Meanwhile let us glance at the position of the teachers in their relation to the new state of affairs.

It must be admitted that the great changes made in 1873 found the college lecturers unprepared. There were only one or two of them who ventured to expound the new subjects to college classes. The students were therefore compelled to depend upon the private tutors, and thenceforth the selection of groups became difficult, if not impracticable. The fact we have just mentioned was, in truth, a misfortune in more ways than one. For the new subjects, and, indeed, the higher subjects generally

admit of being really well taught only by men specially devoted to them, which clearly the private tutors as a class could not very well be. The higher subjects must accordingly for some time be taught in an uncertain way, and meanwhile we are deprived of the evidence, which would have been useful in the present emergency, as to how the Cambridge system would work if these subjects were completely in the hands of the lecturers, as they should be.

As between college lecturers and private tutors we have some reason to hope that one effect of the introduction of the new subjects will be that the former will rise in importance and the latter will, at least relatively, decline. We wish to write nothing but good of the private tutors personally, but if that result should really take place we should regard it as a decided boon. There may be some cases in which private tuition may have merits of its own, but for the ordinary student to have a private tutor perpetually at his elbow when he meets with a difficulty is to give him the worst possible education. In like manner we do not admire the system of more than paternal supervision practised by many of the college tutors over their pupils, the natural effect of which is that the pupil is not allowed to act or to think for himself. He is perpetually asking and getting advice about very trifling matters, and receives a great amount of what is called individual attention on a variety of subjects. But though he may thus gain a little knowledge, if he ultimately learns habits of self-reliance he learns them from other sources.

If the system of private tuition could be done away with, and if more vigour could be instilled into the collegiate system of lecturing, so that complete and adequate courses of lectures could be given, there would be a healthier tone and spirit in the studies of the University, and we are convinced the students would learn more and learn better. In proof of this we may state that the most successful of the private tutors in mathematics really do what ought to be the work of the college lecturer; that is, they deliver lectures to their classes and examine written work for them. We may also state that in the department of classical studies most of the students depend solely upon the college lectures.

We have made these remarks because it seems to us that the Tripos examination is only one phase of the broader question of the whole system of mathematical teaching. Certain proposals will to-day be made to the University, and if these be carried the scheme of college lectures will have to be remodelled: if at the same time a new spirit and energy could be infused into them, it would be a good thing for Cambridge teaching.

The proposed alterations in the examination may be briefly described as follows:—The subjects are in the first place thrown into two grand divisions. On the one side there are what may be called the easier subjects, covering all the ground which a moderately good candidate, whether his tastes incline him towards analysis or physics, may be expected to take up. On the other side there are the higher subjects of pure mathematics and physics. It is proposed that those two divisions should constitute the subjects of two distinct examinations.

The examination in the first division will, as regards class lists, take the place of the present Tripos, that is, the results will be given in the old form of Wranglers, Senior Optimes, and Junior Optimes, arranged in order of merit. This examination will take place in the June of the third year of residence, and only the Wranglers will be permitted to take up the second examination.

The subjects of the second examination which will take place in the following January are subdivided into four groups, and the results upon them will be given in classes, the names in each class being this time arranged in alphabetical order. It will be possible to attain a first class by doing well in one or two groups.

Those who bear unqualified hostility to competitive examinations, especially in their intensified form, when they are followed by an order of merit, will probably be satisfied for the present, hoping that at some future time they will succeed in abolishing the order of merit entirely. There are others who approve of the proposed changes, and who yet think that when confined within reasonable bounds such competitions can do no harm and may do good. Undergraduate human nature being as it is, a good contest, even such as can be had in a Tripos examination, is rather enjoyable than otherwise, and brings out qualities which are worth reckoning for something. It is also a good thing that an undergraduate should learn to have a piece of hard work well done in a given time. What has to be seen to is that the competition does not react injuriously on the course of study. There are many reasons which commend the proposed changes in that connection, and to one or two of these we will now advert.

In the first place, the higher subjects are not suitable for purposes of examination, because the questions which are likely to be put on them require long work and probably much reflection. It is good, therefore, not only that these subjects should be studied leisurely, but that the element of hurry should be as far as possible excluded from an examination upon them. It is true that the last remark will apply also to the lower subjects, but we are to consider that the latter subjects which usually consist of a few simple principles, admitting of an almost infinite variety of simple applications, are in a measure the tools of the mathematician who ought to be well skilled and expert in their use.

Again, if there is to be strict competition it is as well that the area should be narrowed and that the combatants should meet one another on common ground. In the present state of things that cannot be, but under the proposed system it will be possible for a clever lad who has read but little when he enters the University, to hold his own against a competitor, his inferior, who does not begin his undergraduate course till he has been pushed, or has plodded on a good way towards his mathematical degree.

The opponents of the proposed changes affirm that the Tripos will lose its prestige, and that the students under the new system will entirely neglect the second examination. It seems sufficient, in answer to the first of these objections, to point out that the first examination will not be so insignificant either in extent or difficulty, as not to compare in those respects with the Tripos examination of forty or fifty years ago. And as to the best students neglecting the second part, that is a circumstance which seems very unlikely to occur, but the colleges will have the matter in their own hands, and it is to be expected they would be patriotic enough to refuse their fellowships and certainly

their lectureships to students who had not distinguished themselves in both parts.

It is impossible to touch on all the points which suggest themselves in connection with this question, but we may point out in conclusion that the examination in Cambridge has to adapt itself to two classes of candidates, viz.: There is the class who may be called professed mathematicians, because they spend their lives in cultivating mathematical science and in teaching it to others, and there is the class who abandon their mathematics as soon as their undergraduate course has terminated. Of the former class we believe that their tastes and the necessities of their position will alike carry them beyond the subjects of the first examination. In the case of the second class, which is a large one, it is undoubtedly a wise thing to restrict their studies within the limits of the easier subjects. For under the present system, in their eagerness to secure good places they attempt subjects which are either beyond their powers or their opportunities, and so fail to gain the advantages which a strict mathematical training is supposed to afford.

"CRAM" BOOKS

Notes on Physiology, for the Use of Students Preparing for Examination. By Henry Ashby, M.B. (London: Longmans, Green and Co., 1878.)

THIS book, being a fairly creditable and careful specimen of its kind, seems to offer a fitting opportunity for denouncing the whole class of "cram" books of which it is a member. It purports to be notes on physiology, compiled originally, while the author was a demonstrator in the Liverpool School of Medicine, for the use of those students of the school who were preparing for the primary examination of the College of Surgeons; and it is confessedly based upon Foster's "Physiology" and the two chief anatomical text-books used in England. It is a small 18mo of about 230 pages, clearly printed in a large type, and it contains a number of condensed and dogmatic statements in all departments of physiology. It is, we rejoice to be able to say, written perspicuously and compiled with evident care. Most of what Mr. Ashby has read in Foster he has accurately digested and dogmatised. But though he has thus almost disarmed criticism as to his particular book, the book still remains infected with the vices of its class; it is a delusion and a snare to the student; and we heartily wish Mr. Ashby's talents had found a worthier object for their exercise. "Notes" are undoubtedly of the greatest value to a student—nay, they are indispensable, if he is to acquire a large view of his subject; but they are only valuable when the student has compiled them himself from the larger text-books, or, better still, from original memoirs, or when he has seen them digested and set down, so to speak, before his eyes by his teacher. Each of the sentences in his book Mr. Ashby doubtless could and would make the text of a lucid explanation in his lectures or demonstrations. He would lay before his hearers the different views of observers on different physiological questions, as he had learnt them, and, balancing the evidence, he would abstract for them a trustworthy judgment in a careful and concise statement: and the student who took down his notes, on re-reading them, would have the whole discussion refreshed in his mind with more or less vividness—would, in fact, have almost all the benefit of condensing the notes for himself. But when these concise statements or formulæ are put into the hands of students who have not been thus prepared for them, the case is wholly different. Aladdin has the lamp, indeed, but he can conjure up no powerful genii with it.

But if this were all we might be content to let books like this sink to their own level; their inutility would lead to their speedy death. But while the good student would never for a moment think of reading notes that he had not made himself, or if he did read those of another, would quickly find out the cause of their uselessness to any one but their author, the bad student is misled to believe that 230 small pages of fair-sized type contain the whole of the physiology that he needs; he looks through the list of contents and finds set down there almost every physiological fact and problem of which he has ever heard, and he naturally concludes that he has only to equip himself with this little book in order to cope with his examiner.

Mr. Ashby's book, admirable for the purposes of his own students, is useless or worse than useless to the students of any other teacher; published to the world, it is like a creature in an improper medium, and we are constrained to wish that, with all similar books, it may quickly meet the usual fate of creatures so circumstanced.

After this we need not say much about the book itself. On the whole it is well done. The histological sections are decidedly the weakest. The "ossification of bone" (p. 29), and the "development of tooth" (p. 107) might as well have been omitted altogether, as put in so meagrely. The extremely important histological researches of Heidenhain on the pancreas seem to be ignored on p. 16, where "probability" only is allowed to the elaborating functions of glandular epithelia. The pigment layer of the eye on p. 17 is assigned to the choroid coat instead of to the retina, and again on p. 194. No nucleus is given to striated muscular fibres on p. 81. On p. 179 Prof. Ferrier's name is put down at the end of a paragraph as if he were the prime authority for certain facts regarding the *corpora quadrigemina*, which we rather owe to Flourens, Longet, and Goltz. These errors are not of vital importance, and some of them have probably been due to inadvertence. But there are two more mistakes which are of greater weight, and show the danger of mere book-making. On p. 34, where the properties of muscle are discussed, we find that "On contraction . . . O is absorbed and CO_2 . . . given off." This is left unexplained here and in the rest of the book. What Mr. Ashby doubtless meant was that during contraction more arterial blood passes into muscle, and more O is taken up than during rest, while CO_2 is at the same time emitted; but, in the above unguarded way of statement, the fundamental fact of the independence of the actual absorption of O and disengagement of CO_2—a fact of the utmost moment in our conceptions of muscular work—would seem to be passed over. Again, on p. 168, under nervous conductivity, we have the curious statement that "the axis-cylinder probably conducts the impression, *the medullary sheath acting as a sort of insulator to prevent the currents from becoming mixed and confused*"—a physical explanation which no physiologist would now for a moment think of offering.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Divisibility of the Electric Light

THE English and American periodicals devoted to electrical science now announce, "on authority," that the electric light discovered by Edison is a light by incandescence. If this be true there is nothing new or startling either in the discovery of the light or of its divisibility. Lighting by incandescence has been studied for a long time; indeed, it has been studied much more thoroughly than any other kind of electric lighting. Thirty-three years ago a method of producing and sub-dividing the light was patented in England by a Mr. King. The light was produced by heating to white heat in a vacuum, by means of the electric current, either platinum or carbons; and, the specification adds, "when the current is of sufficient intensity, two or a larger number of lights may be placed in the same circuit." For some years after this discovery several improvements on King's invention were patented in America, France, and England; "but," says M. Fontaine, "none of these appear more complete, more explicit, and more practicable than King's; it is, then, useless to continue our nomenclature." The principle of lighting by incandescence, although not neglected or forgotten, seems to have made but little progress until 1871, when M. Lodyguine showed an experiment in the Admiralty Dockyard in St. Petersburg, when he divided the circuit into no less than two hundred lights. This naturally made a great sensation at the time—as great a sensation as that caused by Mr. Edison's telegram of the 7th ult. The Academy of Science awarded to M. Lodyguine the large Lomonossow prize of 50,000 roubles. A company was formed in St. Petersburg with a capital of 200,000 roubles, and the excitement in Europe was then almost as great as has been witnessed in England lately. It was soon found, however, that Lodyguine's discoveries, like those of his predecessors in the same field were, after all, impracticable, and that his illimitable division of the light, however ingenious, was only a fanciful experiment. Every penny subscribed to the company referred to was lost, and Lodyguine's great discovery is now, where it was then—in his laboratory.

It has, however, been urged that these early inventors of the electric light knew only of the galvanic battery as a generator of a powerful current, and that had they known of the Gramme machine, or other dynamo- or magneto-electric machine, the results might have been different. The remark, however, only applies to King and the improvers who immediately succeeded him. The great division of the light by Lodyguine, to which reference has just been made, was in a circuit produced by two "Alliance" machines. Even, however, if such were not the case, there are at present before the world, in more or less detail, four recent inventions for the production of a divided light by incandescence. These are the inventions of M. Reynier, of M. Arnaud, of Mr. Edison, and most recent of all, M. Werdermann. From the way in which these discoveries—if they are discoveries—have been ushered into the world, it is found that great claims are made on their behalf, and there are, therefore, naturally great expectations on the part of the public in regard to them. It cannot be urged now in mitigation of the shortcomings of the incandescent light, as it has been urged in the past, that it has not had a fair trial, on the ground that the lamps in existence were imperfect in conception, and complex in construction. The lamp of M. Reynier seems admirable in its way, and if light by incandescence were to be the light of the future, the claims of this lamp would have to be very carefully considered, and, in any case, it will certainly hold an important place in all investigations into the subject. The lamp of M. Werdermann appears to be identical in principle with, and only slightly different in detail from, that of M. Reynier, and we may fully expect that these inventors will have to come to terms with each other—so much alike are their inventions. Of the details of Mr. Edison's invention—if there are any, nothing is known beyond the fact stated in the *Scientific American*, that it is a light produced from a spiral of incandescent platinum; while the reports in the American daily press show such an effervescent ignorance of the fundamental principles both of electricity and of dynamics, that no reliance whatever can be placed upon them.

Experience, then, has shown that a light by incandescence comes before us in a very questionable shape, and it is essentially a light which discourages the notion of its practical application. The question indeed may be very properly asked: How is it that light by incandescence has always proved such an utter failure? It has had a period of thirty-three years in which to develop; it has been divided into various lesser lights, numbering from two to two hundred: and it has arrested the attention and taxed the skill of the greatest electricians in the world. How is it that it is obliged to give way to light by the voltaic arc? The answer is at hand. The light by incandescence can only be obtained and divided by a great sacrifice of light and power. This is imperative from the fundamental principles of electrical science. The diminution according to the "square," and not according to simple proportion, applies to electricity just as it applies to light, heat, sound, gravitation, and other physical phenomena. Thus if a circuit be divided into two branches whose resistances are equal, a current of half the strength passes through each branch, producing at the point of resistance, not half the light, but only a quarter, because the effect follows the square of the current strength. If the current had been divided into three equal branches, in each branch only one-ninth part of the original light would be obtained, and so on; so that if an electric light of 1,000 candles were divided into ten equal lights, the result would be ten lights of ten candles each, instead of one of 1,000 candles. When this law is borne in mind, and when it is also remembered that to produce the electric light by incandescence at least one-half of the current is lost, it will easily be imagined what a wasteful light it is. Recent experiments prove this. It was recently stated, in reference to M. Werdermann's incandescent light, that he produced two lights of 320 candles each (total, 640 candles), with a prime mover of 2 horse-power; and this was considered a great result—as indeed it was for an incandescent light. But how this sinks into insignificance when compared with the results of lighting by the voltaic arc. A few days ago M. Rapieff, with two of his regulators and a small Gramme machine known as the M machine, and which M. Gramme says requires only 1½ horse-power, produced two lights, which, when carefully measured by the photometer, were found to be each equal to 1,150 candles, or a total of 2,300 candles, while with one of M. Gramme's A machines, requiring 2½ horse-power, a light of 6,000 candles can be obtained from one of M. Rapieff's regulators. Some experiments detailed in M. Fontaine's book on "Electric Lighting" gave a similar result. M. Fontaine's experiments with an incandescent light show that, under the most favourable circumstances, with a Bunsen battery of forty-eight cells, eight inches high, the diminution of the sub-divided light was so great that, where he put five lights in one circuit, he only obtained a total illuminating power of a quarter of a burner, with four lamps only three-quarters of a burner, with two lamps six-and-a-half burners, and with one lamp fifty-four burners. These numbers give the following ratio: 1, 3, 8, 26, 216, thus showing how rapidly the light diminishes when divided. With the voltaic arc, however, and with the same battery, he was able, by a Serrin lamp, to obtain a light of 105 burners.

It will be seen, then, from what has been above stated, that the production and the divisibility of the light by incandescence is a very wasteful process—so wasteful, indeed, as to render its practical application impossible for general lighting. If, therefore, all Mr. Edison has to announce to the world is that he has succeeded in dividing an incandescent light—and the announcement that such is so is made on authority—his discovery amounts to very little. Both the light and its divisibility were discovered long ago. It will easily be seen that it is not in that direction that any great practical results can be obtained. The voltaic arc supplies the only divisible light of any utility and economy, and it is in its development that any real progress must be looked for.

WILLIAM TRANT

Duplexing the Atlantic Cable

I HAVE read with surprise in your number of the 14th inst. (vol. xix. p. 38), an article, in which it is implied that the application of the duplex method of signalling to an Atlantic cable has now for the first time been successfully accomplished by Mr. Stearns.

The publication in the *Times* of Sir James Anderson's letter on "the duplex system in telegraphing," on the day after the publication of your article, was a coincidence of which I trust

you will in fairness allow me to take advantage, to prove that your article does scant justice to Mr. Stearns' predecessors in the application of the duplex system to long submarine cables, and that their success has been something more than "only partial" in the opinion of those who have employed their system.

Mr. Stearns' first success on a long cable dates from a few days ago. In February, 1876, Dr. Muirhead and myself obtained experimentally a perfect balance on the Suez-Aden cable, which, though shorter in miles, is electrically longer than either of the Anglo Company's cables from Valentia on which Mr. Stearns has worked.

In March of the same year Mr. J. Muirhead and myself duplexed the Marseilles-Malta cable, which, though only 825 miles in length, is worked by Sir W. Thomson's syphon recorder, and our system has been in commercial operation on the line ever since.

Early in 1877 Dr. Muirhead applied the system to the Aden-Bombay Cable, which is longer in miles and far longer electrically than either of the cables from Valentia, and since that time this line, as well as that from Suez to Aden, has been worked "duplex" whenever the traffic required it, to the entire satisfaction of the company.

Next, as to your remark that "Mr. Muirhead has been at work duplexing the Direct United States Cable with some prospect of success," the facts of the case are these:—

The cable, in its linear measurement, exceeds the longest Valentia cable by 543 miles; electrically it is twice as long.

It is worked with the mirror galvanometer, and not with the recorder, and these circumstances render the difficulty of obtaining a duplex balance upon it immensely greater than upon any of the other lines referred to.

Notwithstanding the difficulties mentioned, Dr. Muirhead and myself, in April last, obtained a perfectly satisfactory balance, enabling us to transmit sixteen words a minute in both directions at the same time, between Ireland and Nova Scotia, a cable distance of 2,420 nautical miles. HERBERT TAYLOR

7, Pope's Head Alley, Lombard Street

P.S.—Since writing the above my attention has been called to NATURE, vol. xv. p. 180, containing an article on this subject, in which the applications of Muirhead's system to some of the cables referred to in my letter are spoken of as being the first practical successes in submarine duplex telegraphy.

Remarkable Colour-Variation in Lizards

MR. WALLACE's observations in NATURE, vol. xix. p. 4, on a black variety of the common lizard of Capri, as met with on the neighbouring islet of Faraglioni, induces me to refer to a similar appearance in the lizards frequenting the islet of Filfla, on the southern coast of Malta. As recorded in my book, "Notes of a Naturalist in the Nile Valley and Malta," p. 80, I have stated that during a visit to Filfla I was surprised to find that all the lizards on the rock were a beautiful bronze black and so much tamer than their timider brethren on the mainland. Many individuals were so tame that they scrambled about our feet and fed on the refuse of our luncheon. I subsequently sent specimens of this variety, or rather race, to Dr. Günther, F.R.S., who pronounced them identical with the *Podarcis muralis*, so extremely plentiful in Malta and Gozo. Now although the denizens of the two latter islands present divers shades of colouring, I never observed (and I looked carefully during several years) a black or dark-coloured individual. Filfla is about 600 yards in circumference and three miles distant from Malta. It is formed of the upper miocene limestone, and marks an important fault or down-throw which runs along the coast of Malta opposite, by which, as seen in the sketches Figs. 1 and 2 of the work referred to, it appears clear that the severance took place long subsequent to the days of the pigmy elephants, hippos, giant dormice and tortoises, whose remains have been found in such abundance in the crevices of the rocks opposite Filfla. There is no verdure on this bare rock-islet, the surface of which is dark-coloured, whilst its crevices shelter the lizards and furnish abodes for the nests of Manx and cenereous shearwaters, whose docility at the breeding season is equally remarkable, both reptile and birds being like their compeers of Enoch Arden's island, "so wild that they were tame."

Probably the dark colouring is protective, and thus consorting well with the surrounding surfaces, would tend to preserve them from the harriers, buzzards, and hawks which tarry in the Maltese Islands during the spring and autumn migrations

November 11 A. LEITH ADAMS

THE remarkable case of local colour-variation in lizards communicated by Mr. A. R. Wallace to NATURE (vol. xix. p. 4), had already been investigated by Dr. Theodor Eimer, an abstract or translation of whose memoir on the subject, entitled "*Lacerta muralis cærulea*, a Contribution to the Darwinian Theory," is to be found in *Ann. and Mag. Nat. Hist.*, 1875, 4th ser., vol. xvi. p. 234. J. WOOD-MASON

54, Claverton Street, S.W., November 16

The Drought

AT the present time, when more attention is paid to the influence of meteorological phenomena upon society, it would be useful to give some information as to the bearing of the local droughts and famines on our trade and the prospect of its revival. The China and Indian trades have not yet recovered. The droughts have also affected Egypt and Morocco. In the West Indies, Guiana, Venezuela, Colombia, and Brazil they are still operative.

They act to prevent the growth of produce, and in many countries, by reducing the water-ways, they impede its shipment. The people cannot consume our imports, the transit of which is in some cases impeded. The whole of these difficulties affects the exchanges and interferes with the money market and remittances.

The severity of the crisis is abating, but we can hardly feel assured of the revival of trade in Europe and the United States till there is a complete recovery over the vast areas of producing and consuming countries.

Thus the study of meteorological phenomena and facts acquires a new value for practical men and society at large, as stated by Prof. Jevons in your last number. HYDE CLARKE

Sewerage and Drainage

IN NATURE, vol. xix. p. 1, you touch upon a most important point in sanitary engineering which I have for ten years been striving by every means in my power to press upon the public, and I therefore venture to trouble you with a few lines on the subject.

The most important argument in favour of the exclusion of storm water from sewers consists, as you say, in the liability of road detritus to form deposits on the wide flat surface of any channels large enough to convey to one point an exceptionally heavy fall of rain over the area covered by a town, and the inevitably slow course of the infinitely smaller volume of sewage flowing or stagnating in dry weather along the same channels.

When separate sewers are provided for sewage they can be made of such smaller capacity as to keep up a constant flow from the houses in which the sewage is produced, to the land upon which it is to be purified, because the volume of liquid will very nearly correspond with the water supply, and the engineer has safe data upon which to adjust his means to the desired end.

In every town there are, or were, lines of natural watercourses, and if the scavengers' work is properly done the rain-water from roofs and streets may safely be discharged into any of these by short lengths of drains, less liable to be encumbered with deposits of road detritus, and with the certainty that if such accumulations should occur, they will be perfectly harmless from the absence of sewage.

The experiments of Mr. Way with London street water have been seized upon by Mr. Baldwin Latham in order to cover his retreat from the false position unfortunately taken up by himself and most of our senior engineers in the earlier days of sanitary science, and as he knows as well as any one else that it was a grand mistake to confuse and combine *sewerage* and *drainage* in one system, I agree with you in thinking it a pity that he has not acknowledged the facts more distinctly in the recent edition of his well-known work.

The greater proportion of the impurities detected by Prof. Way in the few samples of London street water which he tested are mineral ones which would be comparatively harmless, and, in the opinion of Dr. Voelcker, the experiments must have been vitiated by some mistake. Now as the latter authority has

publicly stated this and added his opinion that no sanitary authority could possibly object to water from streets and roofs of houses, uncontaminated by sewage, being passed directly into any river, Mr. Baldwin Latham might have been expected to have investigated the subject further before adducing the one statement without the other.

There are many other arguments of a sanitary and economical nature in favour of the collection of sewage without more dilution than necessary with rain water, but I must not trespass further on your valuable space.

ALFRED S. JONES

Rayons du Crépuscule

MY father tells me that he sees this phenomenon about five times in the year on the average in this climate. But the display at 4.40 this afternoon was unusually brilliant. The morning had been very wet, and when it cleared in W.N.W. at 2.30 we noticed the clear sky to be of an unusually green tint.

Very distant cirro-stratus on the south horizon ceased to be illumined by the rosy sunlight about twelve minutes before the phenomenon became visible. The latter consisted of very bright rosy rays, in a *very clear* sky, converging near the E.N.E. horizon, the moon shining very brightly on the left of the place of apparent convergence. The sky in interspaces between these rays was of a deep blue; these interspaces being, I suppose, the shadows thrown from distant cumuli and shower-clouds, some of which could be seen upon the western horizon.

Ashby Parva, Lutterworth, November 10 ANNIE LEY

The Power of Stupefying Spiders Possessed by Wasps

WITH reference to my letter on this subject (NATURE, vol. xviii. p. 695), Mr. Arthur Nicols writes me his disbelief that the pretended taking of the poison of snakes internally as a supposed antidote or prophylactic against the bite is anything more than a juggle of those chartered charlatans, the snake-charmers of India; or that it can be so taken with impunity. Of the wasps he says: "I dare say you know that one of the mason wasps of Australia glues its egg to the inside wall of the mud nest, and always at the top, while the rest of the space is filled with spiders. The sting of this wasp is a terrible affair. I was rendered quite comatose for several hours by being stung in the knee" [which, by the way, is precisely the condition of the spiders], "and the pain was most excruciating, with aching and swelling of the inguinal and axillary glands. I don't know whether the wasp stings the spiders, but they are always in a good state of preservation, even when the egg is on the point of hatching. I never found one in the least decomposed. The nest, however, is hermetically sealed, and decomposition could hardly take place, because so very small a quantity of oxygen is inclosed within." It will be remembered that Mr. Armit remarked "a constant movement in the legs of the spiders," and that observation has been made before.

If the word "wasps" in the above heading (which is not mine) be understood to refer to any of the true vespæ (*vulgaris*, *rufa*, *britannica*, or *borealis*), I agree with Mr. Frederick Smith (NATURE, vol. xix. p. 32), that it is misleading. But the solitary insects whose habits have been referred to, have been called wasps in all the popular books of natural history with which I am acquainted; and the correctness, or incorrectness, of the English name does not affect the points brought forward. I may, perhaps, be allowed to recall that the one object for which I referred to the Athenian insect—in connection with a correspondence then going on in your columns as to the senses of insects—was the remarkable circumstance that it seems to hunt down its prey *by scent*. Mr. Armit, of Queensland, referring to that letter, asks the further question, How are the spiders stupefied, and not killed, by a sting, formidable enough in one species to endanger the life of a man? Mr. Smith's letter throws no light on either of these points.

Bregner Bournemouth HENRY CECIL

The Ayrshire Crannog

THE remarks made by Dr. Buchanan White in your last issue in regard to the supposed existence of beech and the absence of Scotch-fir in the Ayrshire Crannog will be carefully attended to. Birch and hazel, so easily recognised by the bark, are certainly in greater abundance than any other kind of wood. I shall, however, collect as many specimens as I can find and submit them to the examination of competent authorities and publish the result in due course. We have now made a large addition to our list of relics, among which I may mention the following:—three daggers (one of which has a gold band round the handle); one knife, one gouge—all these are made of metal, of which the gouge alone has been tested and found to be bronze; a polished stone celt; a clay spindle whorl partially perforated; a curious fringe-like object made of vegetable material; several implements of bone and deers'-horn; a piece of wood with carving on it; portions of a flat dish cut out of wood; a wooden scraper cut out of a trunk of a tree with the handle formed of a branch growing straight out from it—(beside this scrape about a handful of short black hair was found);—a double paddle of a canoe together with various other wooden implements. Hitherto not a single fragment of any kind of pottery has been found on the Crannog. Being merely an amateur in this kind of research, I shall be glad to receive any suggestions from experienced gentlemen as to important points that should be looked after. ROBERT MUNRO

Kilmarnock, November 16

ON THE UTILISATION OF THE AFRICAN ELEPHANT

THE *Colonies and India*, of November 2, contains a short but suggestive article under the heading "Notes," "Elephants in Cape Colony," which deserves consideration. It states that elephants are numerous in the interior of Cape Colony as well as in Central Africa, yet no one seems to have attempted to catch and tame them. The subject has already been mooted that there is a good field for their use both in Central Africa and in Cape Colony, and that they would prove a new and important method of opening up and utilising the wealth of the Colony and of furthering the explorations in Central Africa, which are now of such general interest.

It appears that a troop of wild elephants has been observed within fifty miles of Port Elizabeth—on these the attempt might first be made—and it is well known that they abound in Central Africa, where, indiscriminately slaughtered for the sake of their ivory, the destruction of these animals is so great, as at no very distant period to threaten their extinction. It seems worthy of consideration whether it would not be better to attempt to utilise them as beasts of burden, as is done in India, where they are of inestimable service to the Commissariat, the Public Works Department, the planters, and many others. The African differs from the Asiatic elephant in some points, but is equally well adapted for labour, and, there can be no doubt, would be as easily tamed and trained as his Indian congener. That this is the case is amply proved by the docile and submissive state into which the male and female African elephants now in the Regent's Park Gardens have been brought by Mr. Bartlett and their keeper, Scott. They appear to be just as obedient, intelligent, and free from vice as Indian elephants, and there is, I think, little doubt that the one species, under proper training and discipline, would be as useful in Africa as the other is in India.

There is every reason now to hope that the wealth and resources of our South African possessions will undergo development—might it not be well to revive the suggestion that the elephant should be enlisted in the good work? The importation of one or more of the numerous officers who have been trained to the work of catching and domesticating wild elephants in India with a fitting establishment, and, perhaps, a few Indian elephants to commence the work, would very soon put the value of the undertaking to the test, and probably show that a vast source of working power now unused might be made available.

It is probable that in ancient times the African elephant was domesticated, and any one who has studied the two magnificent specimens in the Society's collection in

Regent's Park, will, I think, be satisfied that they may again be so, and that in temper, docility, and working power, they would be equal, if not superior, to the Indian elephant.

Through the medium of the columns of NATURE, perhaps an impetus may be given to a matter that is certainly worthy of consideration, and may elicit further remarks from some of the Indian Keddah officers, who are practically experienced in the mode of dealing with elephants. It is, at all events, worthy of Sir Bartle Frere's consideration. J. FAYRER

SYNCHRONISED CLOCKS

RAILWAYS, among many other services which they have rendered, have made us more particular about keeping our watches and clocks in accordance with some common standard of time, and during the past few years various systems have been tried for the distribution of a standard time from a common centre—in this country Greenwich Observatory. For purposes of public life it is more important to have all the public and even private time-pieces of a country set according to one standard, than that they should show the correct local time. The latter can easily be ascertained by any one who desires it, if he can be sure of knowing the exact Greenwich time. Of all the systems that have been tried for ordinary public use, that recently organised by Messrs. Barraud and Lund, of Cornhill, seems to us to answer all the requisite conditions. We had the pleasure the other day of inspecting the arrangements made by Messrs. Barraud and Lund for the distribution of Greenwich time from Cornhill as a centre, and we are bound to say that the perseverance and ingenuity displayed deserve success, and we believe that wonderful success has been obtained. Messrs. Barraud and Lund have spared no pains and no expense to perfect their system, which, as a practical and widely useful application of science, is full of instruction.

Any system for the public service of time-signals by synchronising currents which lays claim to approximate perfection, naturally divides itself into three distinct departments; 1. The maintenance of a standard time-keeper at absolutely correct Greenwich mean time; 2. The distribution of the time at hourly intervals with the needful apparatus for testing the work done; and last but not least, the particular means adopted to enable the time currents to control or set the various clocks in circuit to true time. As to the standard adopted by Messrs. Barraud and Lund, and which is in direct communication with Greenwich, this is a mercurial regulator of the very best construction with a Graham dead-beat escapement, having the contact springs for the time-current between the dial and upper plate, the dial being pierced so as to allow free access to all parts of this mechanism without otherwise stopping or interfering with the regulator. However excellent such a time-keeper may be some error will always exist; it will have a daily rate however small, and it becomes important that this error should be corrected at least once a-day. In order that this may be effected without actually using any physical connection, Messrs. Barraud and Lund have adopted the following method:—A permanent bar-magnet about 6 inches long, is secured to the pendulum-rod, so as to vibrate about $\frac{1}{32}$ of an inch from a resistance-coil fastened to the case by a projecting bracket; a current of electricity passing through this coil, accelerates or retards the vibrations of the pendulum according as the current sent is positive or negative, and the power of the current is adjusted to give one second of influence for one hour of duration. The wires of this adjusting current are connected with a commutator, a small instrument about two inches by three-and-a-half, having three holes—that in which a plug is normally placed when no effect is required to be produced, a second marked "fast," and a third "slow," for the reception of the plug, according as the standard requires to be accelerated or retarded. In order to secure the continuance of the current for a period neither longer nor shorter than will produce the desired result, a small ordinary clock is interposed between the commutator and

FIG. 1.

the standard clock. This clock answers a double purpose; it has no hour hand, but only a minute hand, which stands normally at twelve, in which position the clock is "stopped," and no current can pass through to the resistance coil. The plug having first been placed in the "fast" or "slow" hole, as occasion may require, the

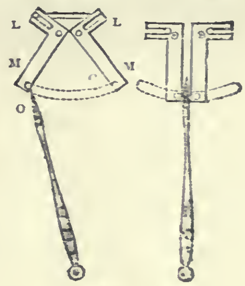

FIG. 2.

clock is set back, say one half-hour for half a second error of the standard. The mere fact of setting back this hand, *starts the small clock and closes the current* at the same moment, continuing to keep a closed current, till the hand returns to 12, when it again breaks contact and stops itself. It will thus be seen that one operation alone

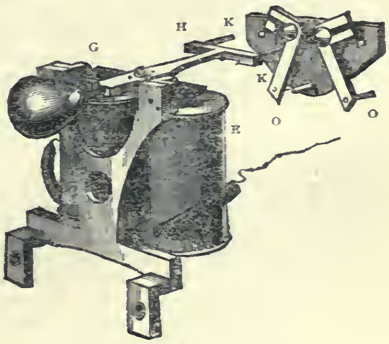

FIG. 3.

is required to set the standard, the whole action automatically ceasing the moment the standard is "to time."

The "distributor" is, in fact, a compound "relay" for twelve circuits, it having been determined to use independent batteries for each line wire in preference to split currents, as used in the chronopher at St. Martin's-le-

Grand. The appearance of this "distributor" is very much like a miniature cart-wheel, about five inches in diameter, inclosed in a circular glass-case, the spokes being the twelve contact springs and the nave a small insulated table which, when the standard automatically closes the contact springs of the time-wire at the exact sixtieth second every hour, is instantly carried down upon and closes the twelve pair of contact-springs connecting the twelve line-wires with their batteries.

Many interesting methods have been adopted for testing and other purposes of maintenance, but we have only space to mention two. One is for the purpose of warning in case of a break-down. This consists of an ordinary electric clattering bell, with a small clock attached, and also connected with the line-wire. So long as the time-currents pass regularly through this instrument the alarm-bell remains silent; but five minutes after the failure of any time signal to pass out upon its duty, the alarm-bell begins to ring violently, continuing to do so for five minutes, and then leaving a small drop indicator, showing on which line the break-down has occurred. There is only one alarm-bell, but each line requires its attendant little timepiece (which, being "synchronised" by the passing current, never can be wrong) and drop indicator. The other instrument is an alarum answering the following important purpose:—A break-down being reported, a plug is placed in a "bridge" corresponding to the number of the line, which causes a very weak current (too weak to affect any of the synchronisers) to pass out along the line for the use of the linesman when searching for the fault; this found and repaired it is important that the plug should be immediately removed: to call attention at the office in Cornhill the linesman has only to break contact at the nearest clock anywhere on the line to set the alarum going in the instrument room, when, the plug being removed, the alarum is shunted off and the line clear for work.

We now come to the chief specialty of the system of Messrs. Barraud and Lund, namely, the synchroniser. This is an automatic finger-and-thumb action, brought to bear hourly on the minute-hand, and bringing it, whether fast or slow, "to time." Each synchroniser is complete in itself, and is simply screwed behind the dial of the clock to be synchronised. It is as follows:—

Two levers carrying pins, and representing the human finger and thumb, project through a slot in the dial (Fig. 1), and either close upon the end of the minute hand itself, or upon a small block fastened to it underneath; the two levers have at their other end slots in which two pins work, projecting from the keeper of an electro-magnet, which, when magnetised at the given moment by the closing of the contact springs on the standard clock, draws the pins together, and sets the hand to time. A reference to Figs. 2 and 3 will at once make the *modus operandi* clear. E is an ordinary electro-magnet, G the keeper, carrying a bar, H, and two projecting pins, K K; these act in slots L L in the levers M M, from the ends of which project the pins O O (representing the human finger and thumb); the passage of the current draws down the keeper and bar, acts upon the levers M M, closing them upon the minute hand of the clock and setting it, whether fast or slow, to time.

The following are some of the special advantages claimed for this system:—1. That any number of clocks, few or many, of any varying sizes, can be synchronised to any agreed standard time-keeper. 2. That the mechanism is, when not in its momentary use, entirely detached from the works of the clock. 3. That it can be applied to existing clocks by simply being screwed in its place, and a narrow slit cut in the dial. 4. That any failure in the transmission of the time-current leaves the clock going in the ordinary way, to be "*set to time*" by the next completed current. 5. That the clocks are kept to time, whether having otherwise either a gaining or losing rate, even if such rate amounts to many minutes a day.

It will seen from the above description that the system owes its success not to the discovery or application of any new fact, such as that for which such eager search is now being made to secure a perfect electric light, but from the simplicity and efficiency of the synchronisers, and their adaptability to every kind and size of clock. By the simple expedient of winding the coils of all the instruments with one size wire, any number, all of varying sizes and powers, can be connected up in circuit. In the City circuit alone, which is wholly controlled by the standard at 41, Cornhill, 108 clocks on eleven different lines of an aggregate length of ten miles, and connecting over eighty different establishments, are efficiently kept to true time. Many more, we believe, are kept to time, not only in other widely distant parts of London, but in various parts of the country; for the latter purpose Messrs. Barraud and Lund have a contract with the Postal Telegraph Department for the delivery of time currents at certain hours each day. There is now no reason why all our public clocks at least should not be included within the correcting power of this system, and lead us astray no longer.

THE TELEPHONE, ITS HISTORY AND ITS RECENT IMPROVEMENTS[1]

III.

The Carbon Telephone

IN the columns of this journal (NATURE, vol. xvii. p. 512) the present writer remarked in the early part of the year "that it was unlikely the telephone of the future would employ the voice to generate the driving power, as it does in the magneto-telephone, but only to modulate the flow of a current obtained by coarser means. It is in this direction that Mr. Edison is working, and his practical triumphs in the past are the earnest of success to those excellent telephonic investigations wherein he has already won an enduring fame." Since those words were written Mr. Edison has brought his telephonic experiments to so successful an issue that his carbon transmitter and his new receiver leave little to be desired in electric telephony, except the automatic record of the received speech, and this, it is not impossible, may ere long be accomplished.

The object Mr. Edison sought to attain was a variation in the resistance of the circuit proportional to the motions of the vibrating diaphragm of the transmitter. Gray employed for this purpose a liquid resistance, but owing to the fact that all suitable liquids are decomposed by the current, Edison abandoned them and tried solid conductors. He remarks in Prescott's work on the telephone:—

"In the spring of 1876, and during the ensuing summer, I endeavoured to utilise the great resistance of thin films of plumbago and white Arkansas oil-stone, on ground glass, and it was here that I first succeeded in conveying over wires many articulated sentences."

A spring attached to the vibrating diaphragm was arranged so as to cut in and out of the circuit more or less of the plumbago film. But the results were not very favourable. In January, 1877, a new device occurred to Edison, namely, the employment of a peculiar property which semi-conductors have, of varying their resistance under pressure.[1] For this purpose

[1] Continued from p. 14.

[2] We have already considered in a previous article the historical facts connected with this discovery, and therefore it will be needless to refer to this point here. A reference to the *Journal Télégraphique* of Berne for 1874, wherein it was asserted M. Clérac had anticipated the use Mr. Edison has made of the varying resistance of carbon dust under varying pressures, fully confirms the statement we made in our last article that the merit of this application is not due to M. Clérac at all, who simply used *permanently compressed carbon dust* as a rheostat.

the diaphragm was made to press against a little cylinder of crude plumbago. The articulation was poor, though conversation could be understood. Investigation showed that the difference of resistance produced by varying pressure was exceedingly small. As so small a change in a circuit of large resistance was but a small factor, whereas a slight change of resistance in the primary circuit of an induction-coil would be an important factor, it occurred to Edison to associate an induction-coil with his arrangement. But difficulties arose from the high resistance of the plumbago cylinder he first used. Ultimately he constructed a transmitter in which a thin slice or button of a semi-conducting substance was placed between two platinum discs. Electrical connection between the button and discs was maintained by the slight pressure of a piece of rubber tubing which was secured to the diaphragm, and also made to rest against the outside disc. The vibrations of the diaphragm were thus able to produce the requisite variations of pressure on the button and thereby create corresponding variations in the resistance in the primary circuit of the induction coil; which in turn gave rise to a corresponding series of induced currents in the secondary. Finally, these induced currents were transmitted through the line and received at the far end by an ordinary magneto-telephone. Fig. 1, for which we are indebted to the *Telegraphic Journal*, shows the arrangement.

At first a button of solid plumbago, such as is employed by electrotypers, was used, and the results obtained were considered excellent, everything transmitted coming out moderately distinct, but the volume of sound was no greater than in that of the magneto-telephone. Numerous other semi-conductors were tried until Edison hit upon some lamp-black that had been taken from a smoking petroleum lamp and preserved as a curiosity on account of its intense black colour. A small disc was made of this substance, and when placed in the telephone it gave splendid results, the articulation being distinct, and the volume of sound several times greater than with a pair of telephones worked on the magneto principle. It was soon found upon investigation that the resistance of a disc formed of this substance could be varied from 300 ohms to the fractional part of a single ohm by pressure alone,[1] and that the best results were obtained when the resistance of the primary coil, in which the carbon disc was included, was six-tenths of an ohm, and the normal resistance of the disc itself three ohms.

The rubber tube between the diaphragm and the disc gave some trouble on account of its tendency to become flattened. Experiments undertaken with a view to remedy this defect led Edison to discover that not only could a rigid substance be interposed with advantage, but that the vibrating diaphragm even was unnecessary; that, in fact, the sound-waves could be transformed into electrical pulsations without the movement of any intervening mechanism. Edison states that the manner in which he arrived at this result was as follows:—

"I first substituted a spiral spring of about a quarter-inch in length, containing four turns of wire, for the rubber tube which connected the diaphragm with the discs. I found, however, that this spring gave out a musical tone which interfered somewhat with the effects produced by the voice; but, in the hope of overcoming this defect, I kept on substituting spiral springs of thicker wire, and as I did so I found that the articulation became both clearer and louder. At last I substituted a solid substance for the springs that had gradually been made more and more inelastic, and then I obtained very marked improvements in the results. It then occurred to me that the whole question was one of pressure only, and that it was not necessary that the diaphragm should

[1] The present writer can confirm this statement, but everything depends on the exact quality of the lampblack, the least trace of overheating lessens its intense blackness and enormously diminishes its conductivity.

vibrate at all. I consequently put in a heavy diaphragm, one-and-three-quarter inch in diameter and one-sixteenth inch thick, and fastened the carbon disc and plate tightly together, so that the latter showed no vibration with the loudest tones. Upon testing it I found my surmises verified: the articulation was perfect, and the volume of sound so great that conversation carried on in a whisper three feet from the telephone was clearly heard and understood at the other end of the line."

The present and modified form of the instrument is shown in the next diagram, where A A is the thick iron diaphragm, B the rigid connecting-piece pressing together the metal discs D D and the carbon disc C. The pressure can be regulated by the screw S acting upon the sliding stem E, which terminates in an insulating cup that encloses the carbon and metal discs. Wires lead from D D to binding screws.

It has been urged that Edison was led to adopt this arrangement by the discovery of the microphone, it is therefore of historical interest to note that in the *American Journal of the Telegraph* for April 16, 1878, it is stated "In the latest form of transmitter which Mr. Edison has introduced, the vibrating diaphragm is done away with altogether." A week or two later the discovery of the microphone was announced, and the transmission of speech without a vibrating diaphragm aroused universal surprise; the loose contacts which are essential to the microphone are, however, fatal for telephonic purposes.

It must be understood that the carbon telephone only acts as a transmitter; it is incapable of reconverting the electric pulsations into sonorous vibrations. For this purpose the ordinary magneto-telephone is employed.[1] The accessories and electrical connections requisite for use in a carbon telephone circuit are shown in the next diagram (Fig. 3).

P P is the primary wire of an induction coil having a resistance of several ohms and placed outside instead of, as is usual, inside the secondary coil *s*, which has a resistance of some 200 ohms.[2] R is the receiving magneto-telephone and T the transmitting carbon telephone; either one or the other can be thrown into the circuit by means of the switch K. When a plug is inserted at the bottom of the switch between 3 and 4, the relay or sounder S, battery B, and key in the centre of the figure, are included in the main line circuit. This is the normal arrangement of the apparatus for signalling purposes. To call the distant end the key is pressed down two or three times; by this means battery currents are sent through the primary coil P, the currents thus induced in the secondary coil *s*, pass to line, and actuate the relay or sounder in the distant instrument. When a plug is inserted at the top, between 1, 2, and 4, the apparatus is available for telephonic communication. By tracing out the connections it will be seen that in this latter case the battery, B, the primary wire of the coil, P, and the transmitter, T, are in short circuit, and at the same time the line wire is in circuit with the secondary coil, *s*. A general view of the arrangement is shown in Fig. 3, for which we are indebted to the publishers of Count du Moncel's book on the telephone. The lettering is different, but the respective parts can be readily understood. In this case a polarised relay and electric call-bell are employed instead of the sounder, a necessary addition in long circuits.

Concerning the actual performance of the carbon tele-

[1] The use of the Bell telephone as a receiver in Edison's instrument is at present the subject of legal proceedings; Edison, however, claims to have used the magneto-receiver before Bell invented it, and we learn that a letter has recently arrived from Mr. Edison stating that he has now constructed a still better and novel receiver for his telephone. Edison remarks: "Batchelor, one of my assistants, heard a whisper last night fifteen feet away from the receiver, and ordinary conversation comes out as loud as originally spoken." Further information about this receiver is given at the close of this article.

[2] In the last improvements the usual position of the primary and secondary coils has been reverted to; the resistance of the former for short circuits should be about a third of an ohm and of the latter somewhat over seventy ohms.

phone, it is stated in Prescott's work on the telephone that Mr. Bentley, President of the local telegraph company at Philadelphia, has succeeded in working with it over a wire of 720 miles in length, and has found it a

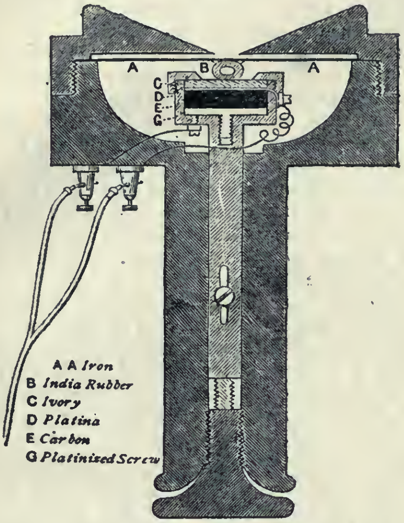

FIG. 1.—Section of Carbon Telephone—Early form.

practicable instrument upon wires of 100 to 200 miles in length, notwithstanding the fact that the latter were placed upon poles with numerous other wires, which occasioned sufficiently powerful induced currents in them

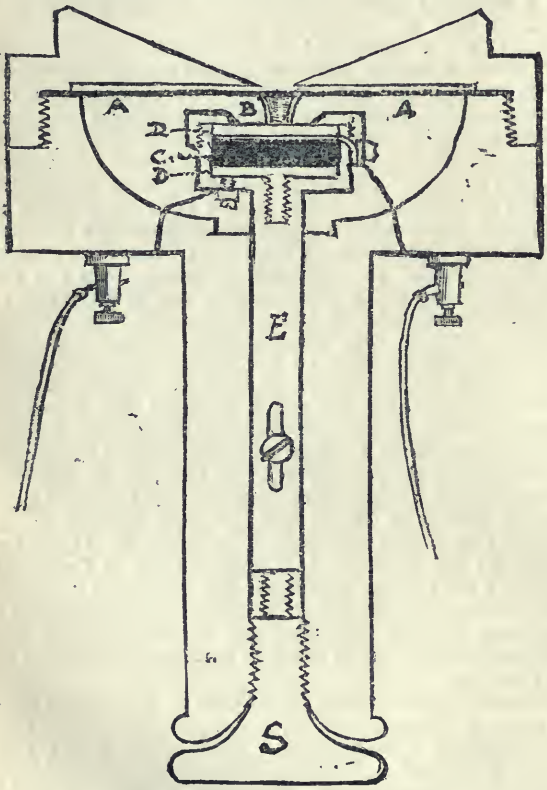

FIG. 2.—Section of Carbon Telephone—Latest form.

to entirely destroy the articulation of the magneto-telephone. Further, he has found the instrument practicable "*when included in a Morse circuit,* with a battery and eight or ten stations, provided with the ordinary Morse apparatus; and that several way stations could exchange business telephonically upon a wire which was being worked, quadruplex, without disturbing the latter, and notwithstanding, also, the action of the powerful reversed currents of the quadruplex on the diaphragms of the receiver. It would thus seem as though the volume of sound produced by the voice with this apparatus more than compensated for the noise caused by such actions." Mr. Edison's assistant, Mr. Adams, now in England, states that conversation has been carried on during the night between New York and Chicago, places nearly 1,000 miles apart; and that under less favourable circumstances during the day the carbon telephone has been successfully used over a line of about half this length. Mr. Adams also informs the writer that at the Paris Exhibition he was able to transmit the music of a piano from Paris to Versailles, a distance of more than 20 miles; the piano standing 50 feet from the carbon telephone and yet not a note was lost at Versailles.

The present writer has had an opportunity of testing

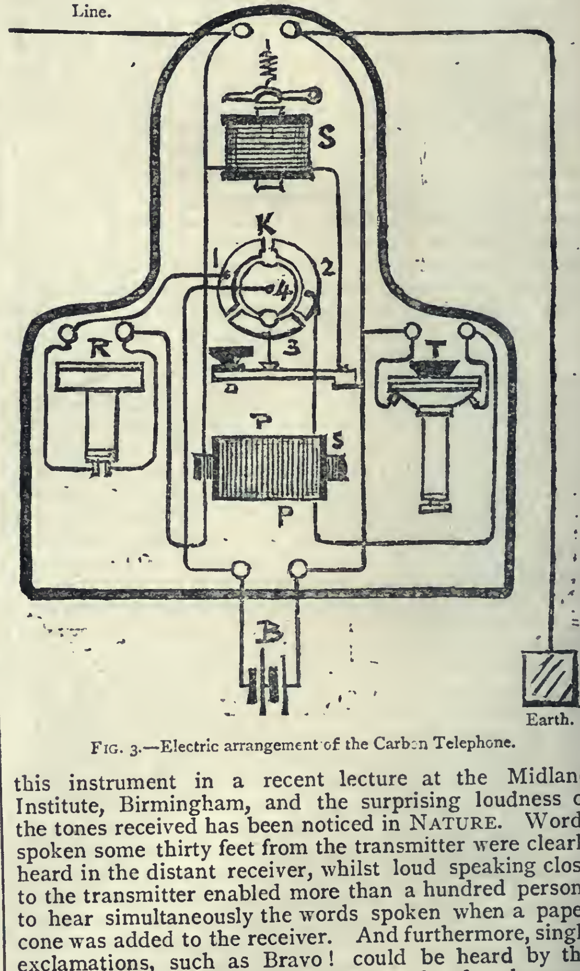

FIG. 3.—Electric arrangement of the Carbon Telephone.

this instrument in a recent lecture at the Midland Institute, Birmingham, and the surprising loudness of the tones received has been noticed in NATURE. Words spoken some thirty feet from the transmitter were clearly heard in the distant receiver, whilst loud speaking close to the transmitter enabled more than a hundred persons to hear simultaneously the words spoken when a paper cone was added to the receiver. And furthermore, single exclamations, such as Bravo! could be heard by the whole of a crowded audience of upwards of a thousand people. Even when the line wire was broken, the fractured ends being near to, but not touching each other, conversation could still be carried on through the circuit with the carbon telephone, though communication by the magneto-telephone and the ordinary telegraphic instruments was entirely interrupted. The writer has also just made further and more severe trials with this instrument on, he believes, the longest private wire in constant use in England, namely, that belonging to Messrs. Colman,

of Norwich and London, which firm have before this kindly allowed their wire to be freely used for experimental purposes. This wire stretches from Messrs. Colman's works at Norwich to their office in Cannon Street, a distance of a little over 115 miles. The wire runs on the same poles as the numerous other wires of the Great Eastern Railway, and is carried overhead from the terminus in London to Cannon Street. At 4 o'clock the experiments began, and the incessant crackling and bubbling sounds in the receivers revealed the fact that the adjoining telegraph wires were at their busiest, and that induction could hardly be worse. Nevertheless, the first exclamation uttered into the hastily adjusted carbon telephone at Norwich was heard perfectly in the

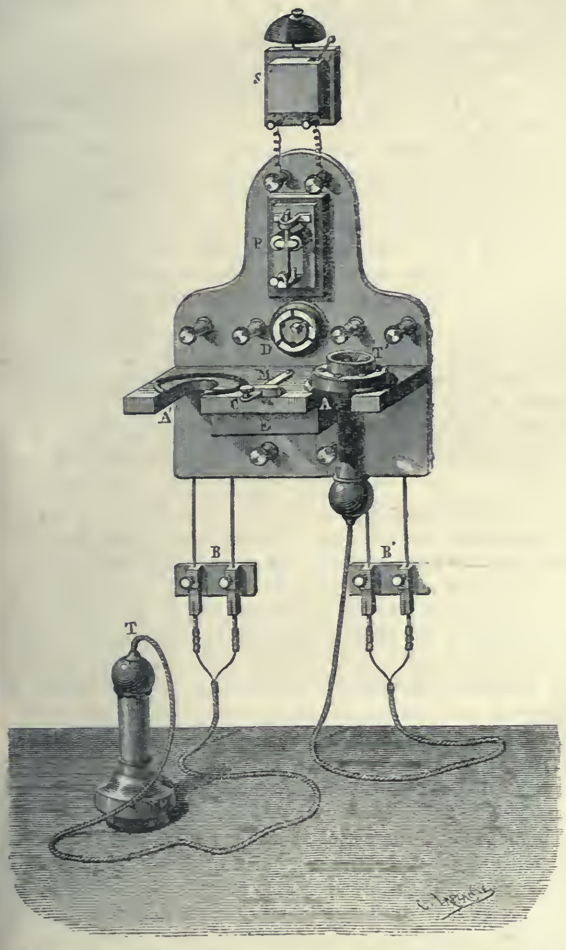

FIG. 4.—General view of the arrangements and accessories of the Carbon Telephone.

counting-house at Cannon Street. Conversation then ensued between the two places; some words were occasionally lost, but the American accent of Mr. Adams, Mr. Edison's professional assistant, who had charge at the Norwich end, was distinctly recognisable in London. Remarks passed on the weather showed that a storm of snow and sleet was going on at both ends, and the insulation therefore almost at its worst. Later on, towards 9 o'clock in the evening, the effects of induction grew less, but were still considerable. The voices from Norwich were now louder, the individuality of the speakers more marked, and conversation could be carried on without difficulty, the voices of certain speakers being remarkably distinct.

Twelve months previously the writer had an opportunity of trying Bell's telephone on the same circuit, when not a word could be transmitted during the day, owing to induction, but at night everything was clearly heard; hence the foregoing experiments established the important fact that in spite of powerful induction operating against it, the Edison telephone is a practicable instrument. It is true that before this telephone can be commercially used, especially during the day and on long lines, special electrical adjustments of the instruments must be made such as the pressure on the carbon and probably the resistance of the induction coil relatively to the line, but in this there is no inherent difficulty, and the adjustment once made no further change is likely to be necessary. Meanwhile we shall await with curiosity the new receiver, which, in a recent letter to the writer, Edison says will arrive in England soon; they differ from other telephones in having "no ear pieces or magnets about them," and according to Edison, "are about twenty times louder than any magnetic telephone, and can, if desired, reproduce the voice at the distant end louder than originally spoken, whilst the whole affair is even cheaper and simpler than the receivers now in use."

It is not impossible that before very long, by means of the Edison telephone, speeches in Parliament may be telephonically transmitted to the newspaper offices and to the country, whilst honourable members, if their articulation be distinct, are speaking from their ordinary places in the House. W. F. BARRETT

NOTES

THE Corporation of Penzance are, we hear, making preparations to celebrate the centenary of Sir Humphry Davy's birth next month. The Paris Academy of Sciences, who awarded Davy a prize in 1807, when war was raging between France and England, will probably take some part in the celebration.

A COMMITTEE has been formed at Heilbronn with the object of erecting a monument to the memory of Dr. Robert Julius Mayer in his native place. Every one knows that Dr. Mayer's name is associated with the establishment of the mechanical equivalent of heat (see NATURE, vol. xvii. p. 450).

THE Observatory of Geneva has received the gift of an instrument of large dimensions by the generous munificence of its director. Prof. Emil Plantamour, who has occupied this position for about forty years, has constructed, at his own expense, in the existing building, a turret of 7 metres in diameter, surmounted by a cylindrical cupola, in which will be placed an equatorial telescope of 10 French inches aperture and 3·70m. focal distance. The object-glass has been ordered from Merz, of Munich, and the equatorial mounting is being manufactured at the workshop of the Geneva Society for the Construction of Physical Instruments. It is hoped that the new instrument will be in working order about the end of next spring.

DR. CREIGHTON, Demonstrator of Anatomy at Cambridge, has joined the editorial council of the *Journal of Anatomy and Physiology*, which henceforth adds to its title the words "normal and pathological."

WE would call the attention of our readers to a paper, likely to be of some interest, to be read at the meeting of the Physical Society on Saturday, by Messrs. Ayrton and Perry. The title of the paper is "The Music of Colour and of Visible Motion," and from what we can learn of Messrs. Ayrton and Perry's investigations, they claim to have hit on a new emotional art. By means of a new machine which they have devised they can produce combinations of harmonic motions with greater variety than can be obtained with any existing machine. Their idea, we believe, is that, judging from their experience partly of the feelings produced by large bodies in rapid motion and partly from the fact that in Japan posturing takes the place of the operatic

singing of the West they think that colour and motion may be made to produce emotions like good music, and therefore may very likely be employed as adjuncts in the entertainments of the future intended to work on the emotions.

AT a meeting of the Medical Society at University College, Gower Street, on December 3, at 8 P.M., an address on "The Use of Physiology to Medical Students," will be delivered by Dr. Michael Foster, F.R.S.

AT the monthly meeting of the Linnean Society of New South Wales, on September 30, the committee appointed to consider Baron Miklucho-Maclay's suggestion for the establishment of a zoological station at Sydney, presented their report affirming the desirability of forming such an institution. The Hon. W. Macleay having very liberally offered to afford facilities for a temporary zoological station in the vicinity of his residence, and promising the use of his museum, library, and microscopes to students of natural history, the Society adopted the report, and it was agreed to commence operations at once.

FROM a promising new American fortnightly journal, *Science News*, edited by Messrs. W. C. Wyckoff and E. Ingersoll, we learn that Prof. S. P. Langley, director of the Alleghany Observatory, has just started on a voyage to Europe, being commissioned by the United States Coast Survey to make observations to serve as a standard of comparison in determining the requisites for astronomical stations in American territory. The inquiry will have particular reference to the effects of different elevations and atmospheric conditions upon the fitness of various localities for the practical work of astronomy. Prof. Langley goes direct to Paris and thence to Italy; the trip will include an ascent of Mount Etna. In addition to the routine work of the Alleghany Observatory, Prof. Langley has been busily engaged in completing a direct experimental comparison between the heat of the sun and the highest heats attainable in the arts. The results indicate that the sun's *intrinsic* heat is almost beyond comparison greater than that of any blast furnace, and far larger than was reckoned by the French physicists. Prof. Langley has also nearly finished a memoir embodying great numbers of measurements and drawings at the extreme lower end of the solar spectrum, particularly the A group. These are parts of a research supported by the Rumford fund, requiring also a new study of the distribution of energy in the spectrum, as shown by the thermopile: Prof. Edison's tasimeter will probably be tested in the course of the work, using the Rutherfurd gratings to supply the spectra. A great improvement, Prof. Langley hopes, has recently been made by him in the accessories of the diffraction spectroscope, by means of which the use of collimators of extraordinary length will become practicable.

MR. C. E. ALLAN writes that he has constructed a rough pencil microphone, using cinders instead of carbon. This construction was not sensible to small sounds, but speech was transmitted very clearly. The pressure at the points of contact was increased by winding wire round the cinder pencil, and by this means the jarring sound of the cinders was almost totally removed, so that songs, the notes of an organ, and the ordinary tones of the voice were distinctly transmitted.

THE annual exhibition of the Haggerston Entomological Society takes place at 10, Brownlow Street, Dalston, to-day and to-morrow, at six P.M. This exhibition is always well worth a visit.

A CORRESPONDENT in the *Times* states that Mr. Edison, in reply to a telegram, avers that, according to his system, the altering of one light does not in the slightest degree affect others in the same circuit. He can adjust the brilliancy of each light at pleasure, it is stated, so that thus electrical lighting would be as steady and as much under control as gas itself. "Whatever method he uses, Mr. Edison appears confident of the success of his system." Mr. Sawyer, being interviewed by a New York reporter on the subject of his lamp, said he did not claim the incandescence of carbon in a sealed receiver as an idea of his own, but had utilised it in the development of other ideas. He claimed a form of conductor which would radiate the heat produced by the incandescence in such a way that the globe would be heated only at the point where the light was. He had also patented a process for charging the receiver with pure nitrogen gas and entirely displacing the atmospheric air. He had patented a means of so closing the receiver that no atmospheric air could find its way into the interior. The nitrogen, he claimed, would last for ever. Besides, there was a substance in the bag at the bottom of the lamp which would absorb oxygen and carbonic acid gas. These, he said, were his improvements. One lamp had been in use two or three hours daily for three months, until a sudden jarring of a door broke it. There had never been any flaking or change in the carbons used. The only change the carbon underwent was its purification. Before being lit it was a dead black; after being incandescent for a time it took on the silver gray colour characteristic of pure carbon. The sub-division of the light, as produced by Mr. Sawyer's system, consists of branch wires leading from the two main wires of the engine. Each of these branches is calculated to supply a number of lamps. The extension of the main wires necessitates an increased heaviness of the wire for each mile. In lighting New York a radius of only half a mile from each supply station will be actually necessary.

THE Liverpool Corporation are taking steps to utilise the electric light as a public illuminating medium as soon as it is utilisable. They intend to apply to Parliament next session for powers to this purpose, have appointed a committee to watch over the subject, and have despatched their engineer to the Continent to examine into the use of the light in Paris and elsewhere.

A *Daily News* correspondent writing from Naples on the 13th inst. states that the stream of lava from Vesuvius was slowly extending from the cone towards the Atrio del Cavallo, the ravine or valley which separates Vesuvius from Somma. The stream extended almost the whole way into the Atrio del Cavallo, and divided into no less than three large streams. These were increasing in size and extent, and the slight shower of lava had also increased, but it was not sufficient to be observable from Naples.

AN earthquake took place at Sierra Leone on the morning of October 11, shaking every house in the colony, and causing great alarm to the inhabitants, but fortunately no damage of any moment was done. There were three successive shocks felt, travelling inland to a distance of about sixty miles, and the end of each is said by some to have resembled three very heavy peals of thunder following quickly upon each other. The natives in the interior were so terrified that in many cases they deserted their villages. An earthquake of a similar character occurred about fifteen years ago.

A M. BAILEY, of Paris, has invented an electric spark pen which possesses some points of interest. If a sheet of thin paper is attached to a plate of copper or zinc, it is stated that an engraving may be made with extraordinary facility by means of this pen. If one of the poles of a Ruhmkorff machine is attached to the plate and the other to the upper end of the pen, the current will run through, and in drawing the paper is perforated. When the drawing is finished, ink is laid on with an ordinary roller, and the greasy fluid penetrates through the holes. The plate is then plunged in water, which detaches the paper, and it is ready for immersion in the acid. The advantage claimed for this method is that the artist does all parts of his work and has no more trouble than if he were working with an ordinary pencil. He can even work in a dark room without any other light than the glare from the induction spark.

THE *Times* Geneva correspondent states that the remains of another lake village have just been brought to light at Lorcas by the shrinkage of the waters of the Lake of Bienne. This appears to be one of the most interesting discoveries of the sort we have had for some time, rich as have been the last few weeks in notable lacustrine finds. The station at Lorcas, assigned by experts to the age of stone, is situated at a short distance from the lake shore, not far from another and similar station which was explored in 1873. An exploration, conducted by Dr. Gross, of Neuveville, has resulted in the gathering of many novel and interesting objects, pierced stone hatchets similar to those found in Denmark, large flint lance-heads, jade hatchets with stag-horn and wooden hafts fastened with pitch; vessels in wood, among others a colander, and a vase in a good state of preservation. Near these were found several arms and instruments of pure copper, a circumstance which points to the probability that intermediate between the age of bronze and the age of stone was a period when prehistoric man had not discovered the art of alloying copper with tin. This was the age of copper. Still more remarkable is a find of human skulls which bear unmistakable marks of having been trepanned. Round pieces have been cut out, doubtless after death, as is supposed, for use as amulets. In some instances pieces were cut from the craniums of living infants in order, as M. Broca conjectures, to let out the spirit by whose malignant influence they were afflicted with fits, convulsions, and other maladies. These bits of infants' skulls were sometimes used in a way of which an example has been found at Lorcas; they were put inside the heads of the dead to protect them from the wiles and assaults of evil beings in the world of spirits.

A PORTRAIT of the Rev. M. J. Berkeley has been presented to the Linnean Society; it was painted by Mr. Peale at the instance of some of Mr. Berkeley's friends.

M. ROUX has sent to the Society of Encouragement of Paris the results of his experiments on nitro-glycerine, from which it appears that bottles of tinned iron falling from a great height and breaking do not cause a dangerous explosion.

WE have received an interesting syllabus of a course of ten lectures on literary and scientific subjects, to be delivered in the lecture theatre of the Bristol Museum, during the winter.

MR. BROTHERS, photographer, Manchester, asks us to state that the portrait of Sir George Airy, which we gave in a recent number, was from a photograph by him. The copy from which our portrait was taken did not indicate by whom it was photographed.

THE *Yama Sentinel* of California gives an account of a singular specimen of meteoric iron, which resembled steel, that had been found in the Mohave desert. It weighs about a pound, has some free gold on the surface, is not magnetic, and has successfully resisted the action of various acid baths. One of its surfaces shows a fracture of crystalline appearance, the colour of which is steel gray tinged with yellow. It has defied the best cold chisels, and has neither broken nor chipped under heavy blows. If its composition could be imitated it would be the hardest and toughest alloy known.

BEING at Osaka recently, a correspondent of the *Kobe Advertiser* was invited to inspect the cotton-mills and spinning-factory which was established at Sakai some years ago, but has attracted little notice. The account which he gives of his visit furnishes additional testimony of the progress which the Japanese are so rapidly making. The premises in question cover 7,000 tszuboes of ground, and the buildings thereon are substantially and well built, and the greater part of the machinery was imported from England. It is not necessary to enter upon the description of the internal arrangements of the establishment, but it is interesting to learn that "in this factory are employed about 150 hands, some 60 men and boys, a few elderly females, and about 80 girls. These latter resemble much the factory girls at home; the same merry countenances and laughing twinkling eyes, unabashed, but perfectly orderly, though perhaps a little negligent upon the appearance of visitors. . . . . We were highly gratified with our visit, showing as it did that there is a wide and very hopeful field for the development of industries in Japan."

WE have on our table the following books :—"Mathematical Drawing Instruments," by W. Ford Stanley, E. and F. N. Spon; "Crystallography," Henry Palin Gurney, S.P.C.K.; "Outlines of the Geology of Northumberland," G. A. Lebour, H. Sotheran and Co.; "Coal and Iron in all Countries of the World," John Pechar, Simpkin and Co.; "Abriss der praktischen Astronomie," Dr. A. Sawitsch, Wilhelm Manke; "A First Catechism of Botany," John Gibbs, Durrant; "The Present State of Electric Lighting," J. N. Schoolbred, Hardwick and Bogue; "The House-Surgeon," Alf. Smee, F.R.S., Accident Insurance Company; "The Geological Record for 1876," edited by William Whitaker, Taylor and Francis; "Etna," G. F. Rodwell, Kegan Paul and Co.; "Spiritual Science," Kuklos, John Harris; "Instructions for Testing Telegraph Lines," Vol. I., Louis Schwendler, Trübner; Health Primers—"Premature Death—Alcohol," "Exercise and Training," "The House," Hardwick and Bogue; "Gegenbaur's Elements of Comparative Anatomy," translated by F. Jeffrey Bell and E. Ray Lankester, Macmillan and Co.

MR. E. P. RAMSAY, Curator to the Australian Museum, Sydney, has prepared and issued "Hints for the Preservation of Specimens of Natural History for Museum Purposes." This short pamphlet contains useful directions for unskilled taxidermists, and notes on the preservation of entire animals of small size. It contains occasional remarks on Australian animals, and suggestions specially appropriate to the wants of naturalists in the bush; these are the only novelties. It will be seen that the title is rather too comprehensive for the contents of the paper; and now that we are beginning to look a little beyond the mere collection of dried skins, it is disappointing to find the internal organs of animals treated as so much matter to be got rid of.

THE last volume of *Medical Reports*, issued by order of the Inspector-General of Customs in China, contains a contribution of considerable interest to our knowledge of the geographical distribution of disease. The notes we refer to, which are by Mr. E. Rocher, of the Customs' Service, prove that the plague exists in China, and that it has in late years spread over a larger area than is generally known. They also show that the disease did not, as some believe, entirely disappear between 1844 and 1873, and it is thought by no means improbable that it may have passed from Yünnan to Mesopotamia or Persia. In Yünnan the disease is known as *Yang-toze*, and is believed to have been originally imported from Burmah. When that was it is impossible to determine, but since the commencement of the civil war it has spread over the whole province, decimating the population. There is one fact which inclines Mr. Rocher to think that the epidemic is owing to exhalations from the soil, viz., that those animals which live in the ground, in drains, or in holes, are the first to be attacked, and this is particularly noticeable with rats. As soon as these animals are ill they leave their holes in troops, and, after staggering about and falling over each other, drop down dead. The same phenomenon occurs in the case of other animals, such as buffaloes, oxen, sheep, deer, pigs, and dogs; all are attacked, but the dog less severely than the others. When these phenomena appear it is not long before the disease spreads to man, and, knowing this, the people do

what they can to guard themselves against it. They begin to purify their houses by lighting fires in every room, and in certain towns they abstain from pork. Mr. Rocher gives details as to the symptoms and course of the disease. With regard to the track of the epidemic Mr. Rocher observed a peculiar fact both in the north and south of the province. Instead of visiting every village in its direct line of progress it would pass some completely by, visiting places near them and on both sides, to return to those forgotten spots several months afterwards, when the epidemic would appear to have passed far away. Another fact not less curious is that after having appeared in almost every one of the villages scattered about the plains, it frequently ascends the mountains, where, among the aborigines who inhabit the high lands, it claims many victims. We may add that Mr. Rocher's notes are accompanied by a map, compiled from private and official memoranda, which shows the course followed by the plague in 1871, 72, and 73; it was not possible, however, to include in it the towns in the west of the province, which was at that time the theatre of the war between the Imperialists and the Mahometan rebels, as the information obtainable was quite untrustworthy, but it is certain that the epidemic was constantly present among the Imperialist troops.

THE additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Miss G. E. Marryat; a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. F. Hinde; two Horsfield's Tortoises (*Testudo horsfieldi*) from Turkestan, presented by Dr. Alex. Strauch, C.M.Z.S.; a Wanderoo Monkey (*Macacus silenus*) from Malabar, two Egyptian Jerboas (*Dipus ægyptius*) from Egypt, a Sun Bittern (*Eurypyga helias*) from South America, deposited; a Woodcock (*Scolopax rusticola*), European, purchased.

CHARLES ADOLPHE WURTZ

IN connection with the Faraday Lecture which follows, it may interest our readers to have a few particulars as to the life and work of the lecturer, Prof. Wurtz.

Charles Adolphe Wurtz was born at Strassburg on November 16, 1817. He commenced his chemical career as assistant to Dumas, and first acquired an independent position as professor at the Agricultural Institute at Versailles. For the last thirty years he has been Professor of Chemistry at the École de Médecine, Paris; in addition to which he now holds the post of Professor of Chemistry at the Sorbonne.

Prof. Wurtz is a member of the Institute (Académie des Sciences), and a foreign Fellow of the Royal Society.

Some idea of the energy which he has displayed as an investigator is conveyed by the fact that a list of no less than seventy-three titles of papers is appended to his name in the Royal Society Catalogue, which only includes papers published previous to 1864. Much of his work is of the first importance in connection with chemical theory, and he undoubtedly takes rank as one of the chief pioneers of modern organic chemistry.

His first investigation, published in 1842, was on the constitution of the hypophosphites; this was followed by researches on phosphorous acid, sulpho-phosphoric acid, &c., which greatly added to our knowledge of the phosphorus compounds. It was in the course of his experiments on the hypophosphites that Wurtz discovered hydride of copper, Cu_2H_2, one of the most remarkable hydrides with which we are acquainted, and especially interesting as, with the exception of potassium, sodium, and perhaps palladium, none of the metals appear to be capable of combining with hydrogen. Hydride of copper is formed as a yellowish precipitate on adding a concentrated solution of copper sulphate to a solution of hypophosphorous acid, and warming the mixture to about 70° C.; in the dry state it slowly decomposes into its constituents at about 55° C.; concentrated hydrochloric acid at once dissolves it with evolution of hydrogen, although copper is not in the least affected by this acid, and what is most remarkable, both the hydrogen of the acid and of the hydride of copper are given off as shown by the equation—

$$Cu_2H_2 + 2HCl = Cu_2Cl_2 + 2H_2.$$

The study of certain cyanogen compounds—the cyanic and cyanuric ethers—next engaged his attention, and his researches on these bodies culminated in the remarkable discovery, in 1849, of the so-called compound ammonias formed by the displacement of one of the atoms of hydrogen in ammonia, NH_3, by organic radicles, such as methyl, CH_3, ethyl, C_2H_5, &c.

A third investigation to which we may here refer is that on the alcohol radicles published in 1855. Frankland had shown that the hydrocarbon radicles which it was assumed were contained in the alcohols could actually be isolated; that, from ordinary or ethyl acohol, for example, which may be regarded as a compound of the radicle ethyl, C_2H_5, with the radicle OH, we may obtain ethyl by acting with zinc on the iodide which it yields on treatment with hydriodic acid, thus withdrawing the iodine from it, just as the iodine is withdrawn from the hydrogen in hydriodic acid by the action of metals; and Kolbe had obtained similar results with acids, such as acetic acid, by submitting solutions of their salts to the action of a powerful electric current. These chemists, however, supposed that the radicles thus withdrawn from combination with other radicles remained in the free state, but Laurent and Gerhardt, and Hofmann argued on theoretical grounds that the bodies thus produced were not the radicles themselves but compounds of the radicles with themselves—that ethyl, for example, was not C_2H_5, but C_4H_{10} or $C_2H_5 + C_2H_5$. Conclusive evidence of the correctness of this latter view was afforded by Wurtz's discovery that if a mixture of the iodides of two different radicles were treated with metallic sodium, a hydrocarbon formed by the union of the two different radicles was obtained. This discovery has afforded one of the chief arguments in favour of the view now almost universally entertained by chemists, that free hydrogen is a compound of hydrogen with hydrogen.

The mere recapitulation of the titles of his remaining investigations would alone occupy a large amount of space. We can only refer to those on the glycols and on ethylene oxide; on the action of nascent hydrogen on aldehyde; on the action of chlorine on aldehyde, both in the anhydrous state and in presence of water; on the action of hydrochloric acid on aldehyde; on the synthesis of neurine; and on abnormal vapour densities, as being, among others, of especial interest.

ON THE CONSTITUTION OF MATTER IN THE GASEOUS STATE[1]

LADIES AND GENTLEMEN,—

I ESTEEM it a great honour to address you within these walls, about which there still hovers the ever fresh memory of him whose name we celebrate to-day, while we deplore his loss. I am fully sensible both of the great value of this honour and of the danger that attends it, and I have need to shelter myself under the authority of the great name of FARADAY. I have, therefore, chosen a subject connected with his earliest discoveries. The constitution of matter is a question of the highest importance with regard both to physics and to chemistry.

The word "gas" was introduced into science by Van Helmont, who, at the beginning of the seventeenth century, first pointed out, with some degree of precision, the differences existing be-

[1] The Faraday lecture, delivered before the Fellows of the Chemical Society, in the Theatre of the Royal Institution, on Tuesday, November 12, 1878, by Ad. Wurtz, Membre de l'Institut; Doyen Honoraire de la Faculté de Médecine de Paris.

tween certain aëriform fluids. He it was who first spoke of *Gas silvestre*, formed by the combustion of charcoal, and given off during the fermentation of beer. To him, also, we owe the distinction—which kept its ground for two centuries—between gases and vapours. He regarded gases as aëriform fluids, incapable of reduction to the liquid state by cooling, whereas vapours require the aid of heat to maintain them in the gaseous state. An important difference of constitution seemed, therefore, to exist between these two kinds of aëriform fluid. This difference, however, is not fundamental, and the distinction between gases and vapours has disappeared, in a theoretical point of view, being, in fact, reduced to a simple question of temperature and pressure.

On March 13, 1823, Faraday, then a young man engaged as chemical assistant at the Royal Institution, read before the Royal Society a note entitled "On Fluid Chlorine." He had succeeded in condensing this gas to a liquid by a process which has become classical. This process consists in heating in a closed vessel placed in a water-bath crystals of chlorine hydrate. This compound, very rich in chlorine, is resolved at a gentle heat into chlorine and liquid water, the quantity of which is not sufficient to dissolve the whole of the chlorine. The latter is therefore disengaged in great part in the state of gas, which accumulates in the small space remaining to it, and is liquefied by the pressure which it exerts upon itself.

On the same day Sir Humphry Davy read a note "On the Liquefaction of Hydrochloric Acid Gas," which he effected by decomposing sal-ammoniac with sulphuric acid in a closed vessel. These researches were completed by Faraday, who, on April 10 of the same year, described the liquefaction of a large number of gases, directing his efforts, by Davy's advice, chiefly to those which are dense, or very soluble in water, such as sulphurous acid, ammonia, sulphuretted hydrogen, carbonic acid, and protoxide of nitrogen.

To enumerate the special processes adopted in each particular case would occupy too much time. We shall therefore merely observe that the chief, if not the only, means of condensation adopted in these experiments was compression, that is to say, the reduction of the gas to a small volume, and that this compression was exerted by the gas upon itself, as it accumulated in the very strong sealed glass tubes in which it was disengaged. Sir Humphry Davy, in the note above cited, had remarked that pressure appeared to be a more efficacious method of condensation than cooling, inasmuch as a double pressure reduces the volume of the gas to one-half, whereas a depression of temperature of 1° F. reduces the volume by only $\frac{1}{480}$, the lowering of temperature, moreover, soon attaining an impassable limit. It must, however, be especially observed that, even in his first experiments, Faraday made use of differences of temperature, if not to liquefy the gases, at all events to distil and isolate the liquids. Thus it was in the case of chlorine, for example, and in that of ammonia, which he liquefied by heating ammoniacal silver chloride in a bent tube sealed at both ends, the liquid ammonia then distilling over and collecting in the empty branch of the tube, which was cooled to a low temperature.

Similar phenomena will be exhibited in the experiment which I am about to show you, consisting in the liquefaction of cyanogen gas by heating cyanide of mercury in a small glass tube terminated by a long capillary tube bent in the form of the letter U. The figure of this curved portion will be projected on a screen by the electric light, and in a few seconds you will see the liquid cyanogen collect in the bend.

Before leaving this part of my subject, I would recall to your attention two of Faraday's discoveries resulting from the application of the principles just explained. Having compressed coal-gas to twenty-five atmospheres, Faraday in 1825 discovered two important bodies, namely, butylene, a compound of great importance in a theoretical point of view—and benzene—so named by Mitscherlich several years afterwards—which in our own time has become the object of numerous and important applications, and the pivot of an entire department of chemistry.

Another instance is afforded by sulphurous acid gas (SO_2), which was liquefied by Bussy in 1824, at the ordinary atmospheric pressure, by the effect of a cold of 12° to 15° below zero.

Whether we condense gases by pressure or reduce them to the liquid state by diminution of temperature, the result of either method is to bring their particles closer together. It would seem then, in accordance with Davy's view, that pressure ought to be more efficacious, as a means of condensation, than cooling. Nevertheless it is not so. The mere approximation of the particles of certain gases does not suffice to effect their liquefaction, and moreover, the distances between the particles cannot be diminished indefinitely by pressure alone. M. Natterer, of Vienna, has compressed oxygen, hydrogen, and nitrogen to 3,000 atmospheres without effecting their liquefaction. These gases, hitherto called permanent, cannot be liquefied by pressure alone, and their liquefaction, which has quite recently been effected, is the joint effect of strong pressure and a great degree of cold. This is the important point, and I request your permission to offer in this place a few explanations which will serve to place it in its true light.

The impossibility of liquefying certain gases by pressure alone is in accordance with the ideas which are current at the present day respecting the nature of aëriform fluids, and likewise with a discovery made in England within the last few years, on the continuity of the gaseous and liquid states. I will explain myself briefly on these two points.

Daniel Bernouilli first enunciated the idea that gases are formed of material particles, free in space, and animated by very rapid rectilinear movements, and that the tension of elastic fluids results from the shock of their particles against the sides of the containing vessels. Such is the origin of the kinetic theory of gases, which has been revived since 1824 by Herapath, Joule, and Krönig, and developed chiefly by Clausius and Clerk Maxwell.

The law of Boyle and of Mariotte follows as a natural consequence of this idea. Suppose a gas occupying a certain volume, and composed of a definite number of material particles —or molecules so-called—to be contained in a closed vessel, such as the cylinder of an air-pump; the pressure on the piston will be determined by the number of shocks of the molecules diffused through the neighbouring stratum of gas. If, then, the volume of the gas be reduced, the number of particles in this layer will be increased, as well as the sum of the shocks, and the pressure will be increased in proportion thereto.

The velocities with which these molecules move are enormous. Clausius supposes that the molecules of air move with a mean velocity of 485 metres per second, and those of hydrogen with a mean velocity of 1844 metres per second. I say mean velocity, for all the particles of a gas do not move at the same rate. But can the particles freely traverse these wide spaces? By no means; their number is so immense, that at every instant they enter into collision with one another, and rebound in such a manner that their motion is altered both in velocity and in direction. It follows, therefore, that the molecules of a gaseous mass are continually moving in all directions with variable velocities, their motion in the intervals between the collisions being sensibly rectilinear. The distribution of the velocities has been made the subject of important researches by Clerk Maxwell.

These movements of gaseous molecules determine a very important physical condition, namely, temperature. In fact, the energy of the rectilinear movements, that is to say, the mass of the gaseous molecules multiplied by the square of their velocity, gives the measure of the temperature, which consequently increases proportionally to the energy of the rectilinear movement, or, for the same gas—since the masses remain constant—it increases as the square of the velocity. If the velocity were reduced to nothing, the calorific motion would be annihilated, that is to say, the gas would be entirely deprived of heat. This state corresponds with the absolute zero.

The gaseous molecules moving in all directions and coming into collision with one another in space, are very nearly emancipated from cohesion. Nevertheless this attractive force makes itself felt for the infinitely short time during which the molecules actually touch one another, or are on the point of doing so. This influence of cohesion is one of the causes of deviation from the law of Boyle or of Mariotte.

In liquids the influence of cohesion is manifest, preventing the molecules from separating, though it allows them to glide one over the other. This molecular cohesion, or attraction, is in continual strife with the force of expansion, or kinetic energy, which, if unopposed, would launch the molecules into space.

To understand the antagonism between these two forces, consider for a moment a saturated vapour in contact with the liquid from which it has been formed. When it is reduced to a smaller volume, a certain number of its molecules are brought within the sphere of action of cohesion; they are consequently aggregated together and precipitated in the liquid state, while the rest, being

now diffused through a wider space, continue to move with the same velocity and to exert the same pressure as before. In this case the force of cohesion of the liquid particles exactly balances the expansive force or kinetic energy, and serves to a certain extent as a measure of its amount.

Now let the vapour be heated, after it has been withdrawn from the action of the liquid; its expansive force will then increase; it will dilate, and may then be compressed, until, by the approximation of its particles, it is again brought within the sphere of action of the cohesive force, that is to say, to the point of saturation corresponding with the temperature to which it has been raised. With the increase of temperature, the expansive force or kinetic energy of the vapour likewise increases, whereas the cohesion of the liquid becomes less: hence the necessity of further diminishing the distances between the particles by increase of pressure. But this double effect of increased kinetic energy of the gaseous molecules, and diminished cohesion of the liquid molecules, going on progressively as the temperature rises, a point will at length be attained at which the energy of the molecular movement will finally gain the victory over the force of cohesion, *whatever be the pressure to which the vapour is subjected*. The minimum temperature at which this effect is produced, and at which, therefore, a vapour can no longer co-exist with its liquid under any pressure whatever, has been called by my friend, Dr. Andrews, the critical point, and by M. Mendelejeff, the absolute boiling point. Above this temperature, whatever may be the pressure, the gas, whether dilated or compressed, will maintain the same physical state, characterised by freedom of molecular or calorific movement.

I can show you by an experiment this peculiar phenomenon of the sudden passage of a liquid mass to the state of gas, by heating liquid carbonic acid in a closed vessel, just as Cagniard de Latour formerly heated ether. Here is a tube, half filled with liquid carbonic acid, which we are about to immerse in water at 35°; you observe that the liquid first rises quickly in the tube, its coefficient of expansion being greater than that of gases; at the same time the meniscus flattens more and more, indicating a diminution of cohesion in the liquid (Andrews), and finally disappears altogether; in fact the liquid itself has disappeared, having been entirely and suddenly transformed into gas. What now must we do to cause it to reappear? We must lower the temperature, so as to diminish the kinetic energy of the gas, and increase the cohesion of the liquid. A moment will then arrive when the cohesive force will again be able to resume the contest, and the liquid will be reconstituted.

We are now in a position to understand why certain gases, hitherto called *permanent*, cannot be liquefied except by the combined action of very strong pressure and a very great degree of cold. The critical points of these gases are situated at very low temperatures. They have quite recently been liquefied, this great discovery having been made by MM. Cailletet and Raoul Pictet.

The principle of Cailletet's apparatus is the following:—The gas to be liquefied is introduced into a cylindrical glass vessel and transferred by means of mercury to a very strong glass tube sealed into the reservoir. This latter is firmly fixed in a cylindrical cavity hollowed out of a block of iron, and serving as a kind of closed mercurial trough. The cylindrical cavity communicates with a hydraulic press which injects water on to the surface of the mercury, driving it into the gas reservoir, which is ultimately quite filled with that liquid, the gas being thereby driven into the tube, where it is liquefied.

In this manner we shall be able by a few strokes of the piston of the hydraulic press to liquefy carbonic acid. Other gases less easily condensable may be liquefied in a similar manner, if the tube be cooled to – 20° or – 30°. But these temperatures do not suffice for the liquefaction of the so-called permanent gases. To cool these gases to lower temperatures, M. Cailletet avails himself of sudden expansion (*détente*). The gas, compressed to several hundreds of atmospheres, when allowed to expand suddenly and drive the air before it, consumes a certain quantity of heat, and is thereby reduced to a kind of mist, which will appear on the screen, and pass away like a cloud, if we suddenly expand the strongly compressed carbonic acid gas, which we have here, in default of oxygen or hydrogen.

M. Raoul Pictet has succeeded in condensing oxygen and hydrogen in the form of liquids, properly so called, and even in obtaining the latter of these gases in the solid state. To produce this effect, he employs condensing apparatus of incomparable power, combining the action of a cold of 120° to 140° below zero with that of enormous pressures amounting to 550 and even 650 atmospheres. The pressure is produced by the accumulation of the gases in a closed space consisting of a long copper tube of very thick metal. The oxygen was produced by heating potassium chlorate in a howitzer shell, having a copper tube soldered into its orifice. The hydrogen was prepared in a similar apparatus, by decomposition of a dry mixture of potassium formate and potassium hydrate.

To produce very low temperatures of 120° or even 140° below zero, M. Pictet resorts to a very ingenious artifice. Over the reservoir-tube which surrounds the copper tube, and in which these low temperatures are intended to be produced, he superposes another system of concentric tubes, intended to produce a first fall of temperature amounting to – 65°, by the volatilisation of liquid sulphurous acid. By means of this first depression of temperature it has been found possible to liquefy carbonic acid gas in the inner tube of the system just mentioned, by a pressure of only a few atmospheres. The carbonic acid thus liquefied being introduced into the lower reservoir-tube of the apparatus, produces by its volatilisation, a second fall of temperature round the copper tube containing the compressed oxygen which is to be liquefied. M. Pictet has in fact established a double circulation, one of sulphurous acid, the other of carbonic acid. I will describe the former. Sulphurous acid gas is liquefied by a pressure of three atmospheres and collects in a strong vessel, from which it passes through a tube into the upper reservoir. The pressure is exerted by means of a force-pump. A suction-pump connected with the force-pump, and acting in concert with it withdraws the liquid sulphurous acid from the reservoir-tube, and transfers it to the force-pump, which brings it back to the vessel, and thence to the upper reservoir-tube.

The circulation of the carbonic acid is established in the same manner, by means of two pumps, one of which condenses the gas by forcing it into tubes cooled to – 61°, while the other, which is a suction-pump, sends it back to the force-pump. The volatilisation of the carbonic acid produces round the copper tube the low temperatures above-mentioned. The copper tube is in fact surrounded by solid carbonic acid.

In this manner M. Pictet has liquefied oxygen, and has approximately calculated its density. He has also liquefied and even solidified hydrogen, which he has seen to issue from the tube in the form of a steel-blue liquid jet, which partly solidified. The solid hydrogen, in falling on the floor, produced the shrill noise of a metallic hail, thus confirming the bold and ingenious idea of Faraday, who first suggested that hydrogen is a metal.

The experiments of MM. Raoul Pictet and Cailletet have then removed from science the distinction between permanent and condensable gases. Permanent gases exist no longer. All aeriform fluids may be liquefied with a facility greater in proportion as their critical points are situated at higher temperatures. From a physical point of view, therefore, gases and vapours have the same constitution, being formed of molecules which move freely in space. In what, then, do they differ? They differ by the nature and constitution of these molecules; and here we enter on the domain of CHEMISTRY.

It is supposed, in chemistry, that the molecules of each species of gas or vapour are formed of a definite number of atoms. The simplest molecules, like those of mercury-vapour, are formed of single atoms. Others include several atoms of the same or of different kinds: and these latter molecules may be very complex, that is to say, formed of a large number of atoms held together by affinity, and vibrating in concert in a system to which they are attached, viz., the molecule. In this system, which has a definite form, extent, and centre of gravity, the molecules execute their own proper movements, and are at the same time carried forward with the entire system in the molecular paths.

I cannot here dilate on the nature and chemical properties of the several gases and vapours. I wish merely to throw light on a single point, which is of great importance, inasmuch as it constitutes one of the foundations of chemical science.

The proposition which I am about to enunciate is generally adopted by chemists, resting as it does on an imposing array of facts: *Equal volumes of gases or vapours, under the same conditions of pressure and temperature, contain equal numbers of molecules.*

The Italian chemist, Amadeo Avogadro, in discussing the discoveries of Gay-Lussac respecting the simple relations which exist between the volumes in which gases combine, was the first to recognise that there likewise exists a simple relation between the volumes of gases and the number of molecules which they contain. The simplest hypothesis, said he, that can be made regarding this matter consists in supposing that all gases contain in equal volumes equal numbers of "integrant molecules." By this term he denoted what we now call simply *molecules*, and he distinguished these integrant molecules from the "elementary molecules" which we call *atoms*. According to him the integrant molecules of gases are all equally distant one from the other, and these distances are so great in proportion to the dimensions of the molecules, that the mutual attraction between the latter is reduced to nothing.

These integrant molecules are composed of a greater or smaller number of elementary molecules, not only in compound, but likewise in simple bodies; the integrant molecules of chlorine, for example, are composed of four elementary molecules, and the same is the case with the integrant molecules of hydrogen. What happens, then, when chlorine and hydrogen combine together? The integrant molecules of these two bodies are then resolved into elementary molecules, which combine, two by two, to form hydrochloric acid.

Ideas analogous to those of the Italian chemist were enunciated in 1814 by Ampère, and thus there has been introduced into chemical science the notion that there exist two kinds of small particles, namely, molecules and atoms, the former being diffused in equal numbers through equal volumes of gases.

But this notion, so clearly enunciated more than sixty years ago, was afterwards destined to be obscured. Berzelius, taking up Ampère's proposition, altered it by substituting atoms for molecules, and saying that "equal volumes of gases contain equal numbers of atoms." This proposition, which has given rise to long discussions, must now be rejected, for it is inexact. It is to Gerhardt, and more recently to Cannizzaro, that is due the honour of having restored the thesis of Avogadro and Ampère, and pointed out its importance in connection with chemical theory. This I must explain in conclusion.

In the first place Gerhardt simplified the rule of Avogadro. The latter supposed that a molecule of chlorine or of hydrogen contains four atoms, whereas Gerhardt regards it as consisting of two. Avogadro's proposition thus modified, assumes a very simple form, and may be enunciated in the following terms. Suppose that a volume, or the unit of volume, of hydrogen contains one atom; then the molecules of all gases and vapours will occupy two volumes. Thus, a molecule of hydrogen formed of two atoms will occupy two volumes, and a molecule of chlorine formed of two atoms will likewise occupy two volumes. What now will happen when chlorine combines with hydrogen? The molecules will be cut in two, and each of the two chlorine-atoms uniting itself to an atom of hydrogen, two molecules of hydrochloric acid will be formed, each occupying two volumes. Thus if an atom of hydrogen occupies one volume, a molecule of hydrochloric acid will occupy two volumes. The same is the case with the molecules of all other gases and vapours.

A molecule of water formed of 2 at. H and 1 at. O occupies 2 volumes.
" ammonia " 3 at. H and 1 at. N " "
" marsh gas " 4 at. H and 1 at. C " "

This list might be prolonged by taking as examples a large number of gaseous or volatile bodies belonging both to mineral and to organic chemistry, and including chlorinated, brominated, and oxygenated compounds of the metalloids, and of a large number of metals. The countless volatile compounds of organic chemistry, hydrocarbons, alcohols, chlorides, bromides, organo-metallic compounds, compound ammonias, aldehydes, ketones—all this legion of various compounds—conform to the law of Avogadro and Ampère, their molecules occupying two volumes if an atom of hydrogen occupies one volume. Hence it follows that the relative weights of two volumes represent the relative weights of the molecules, or the molecular weights. To find these latter, therefore, it is sufficient to double the numbers which express the weights of a single volume, or of the unit of volume, that is to say the densities. The densities of gases may be referred to that of hydrogen as unity, and the atomic weights to that of hydrogen. The unit being then the same, it follows that the numbers which express the double densities referred to hydrogen will also represent the molecular weights.

Chemists represent the constitution of molecules by formulæ, each of which shows the number of atoms condensed within the molecule. Now the molecular weights being known, it is very easy to deduce the formulæ from them, as these formulæ must represent the number of atoms comprised in two volumes. Such is the relation which exists between the Law of Volumes and Chemical Notation. The rule of Avogadro and Ampère has, in fact, become one of the bases of this notation. There are, however, certain exceptions to its generality, but they are probably more apparent than real. Sal-ammoniac, ammonium sulphhydrate, phosphorus pentachloride, iodine trichloride, sulphuric acid, calomel, amylene hydrobromide, and chloral hydrate, have vapour-densities such that their molecules appear to occupy four volumes. Such, however, is not the case; and it may be shown that the bodies in question do not volatilise without decomposition, but that, when they are heated, their molecules split up into two, each of which occupies two volumes. Being unable to analyse all the cases above-mentioned, I will confine myself to the last, viz., chloral hydrate, which has given rise to a long discussion.

The question to be decided is, whether this compound is or is not decomposed by conversion into vapour? If it really suffers decomposition, it should be resolved into anhydrous chloral and water. That this decomposition really takes place may be shown by a method based on the theory of dissociation developed by M. H. Sainte-Claire Deville.

Here is the case in a few words. We have here in a tube a certain volume of the vapour of chloral hydrate under a certain pressure; it is required to show that this vapour contains vapour of water. For this purpose we are about to introduce into it a body capable of emitting vapour of water, crystallised potassium oxalate, for example. If the atmosphere is dry, this salt will give off vapour of water just as it would in dry air or in vapour of chloroform at the same temperature, and it will continue to emit this vapour until the atmosphere shall have taken up a degree of humidity corresponding with that which is designated by M. H. Sainte-Claire Deville the *dissociation tension* of the hydrated salt in question. If, on the other hand, the chloral atmosphere is moist, and exhibits exactly the degree of humidity just defined, the crystallised oxalate will not emit any water. In this first tube, then, we have the vapour of chloral hydrate; the second contains vapour of chloroform. This latter is dry, and I am about to prove to you that the former is moist. In fact, the crystallised potassium oxalate which we are introducing into the chloroform tube will rapidly depress the level of the mercury by emitting vapour of water, whereas in the atmosphere of chloral hydrate it will not emit vapour of water, and consequently will not depress the level of the mercury. This shows that chloral hydrate undergoes decomposition when converted into vapour, and this supposed exception to the rule of Avogadro and Ampère vanishes, like all the rest, when submitted to the test of experiment. This rule appears, then, like a grand law of nature, as simple in its enunciation as it is important in its consequences.

Such are the considerations which I wished to lay before you on the physical and chemical constitution of gases. Does not this exposition seem to show that, of all the states which matter can assume, the gaseous state is the most accessible to our researches, and the best known—not, indeed, that we can affirm the certainty of the theoretical considerations which I have brought before you, for they are but probable. In the physical sciences nothing is certain but well-observed facts and their immediate consequences; and, whenever we attempt to make these facts the basis of any general theory, hypothetical data are apt to mix themselves up with our deductions. In the present case the hypothesis consists in assuming that gases, and matter in general, are formed of molecules, and these latter of atoms. No one has ever seen these molecules and atoms, and it is certain that nobody ever will see them. Does it follow then that we ought to reject or disdain this hypothesis? By no means. Our theories may be verified in their consequences, and may thereby acquire a certain degree of probability. The theory under consideration has been subjected to this ordeal, and nothing has hitherto been found to contradict it. It is probable, indeed, that gases are composed of small particles moving freely in space, with immense velocities, and capable of communicating their motion by collision or by friction. It is probable that these molecules are diffused in space in numbers so enormous that the most rarefied spaces still contain legions of them; and it is this circumstance which explains the possibility of the movements of the radiometer.

Be this as it may, the idea of Daniel Bernoulli has been developed into a beautiful theory—the kinetic theory of gases—a theory which has shed a sudden clearness, an unexpected light, on matters which seemed to be veiled in the deepest obscurity. The molecules, as already stated, are invisible. Nevertheless, attempts have been made to pene rate this invisible world by the force of scientific reasoning, and by an effort which does honour to the human mind, even if it be destined to remain barren. The illustrious authors of the kinetic theory of gases have sought to determine, not only the velocities of the gaseous molecules, and the prodigious number of their collisions during a unit of time, but likewise their distances, their absolute dimensions, and their number in a given volume. And here we arrive at results which bewilder the imagination, but which, in this lecture, I must not attempt to unfold.

Permit me only to add that these great labours mark a resting place in our course, and are, perhaps, an approach towards the solution of the eternal problem of the constitution of matter—a problem which dates from the earliest ages of civilisation, and though discussed by all the great thinkers of ancient, as well as of modern, times, still remains unsolved. May we not hope that in our own time this problem has been more clearly stated and more earnestly attacked, and that the labours of the nineteenth century have advanced the human mind in these arduous paths, more than those of a Lucretius, or even of a Descartes and a Newton. From this point of view, the discoveries of modern chemistry, so well expressed and summarised by the immortal conception of Dalton, will mark an epoch in the progress of the human mind; and to one of the most important among these discoveries—that of the liquefaction of the gases—grateful posterity will for ever join the glorious name of FARADAY.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

BIOLOGISTS will be pleased with the frank recognition of Dr. Foster's services contained in the statute proposed by the Council of the Senate at Cambridge for the new Professorship of Physiology to be founded by Trinity College. It is to be permanently recorded that Dr. Foster's lectures have always been open to the entire University, and that he "has successfully promoted" the study of physiology. Thus the continued self-denying effort and enthusiasm which have in eight years developed a school of over seventy students, and which have called forth the original talents of a score of ardent young investigators, will find still fuller scope. We understand that Dr. Foster resolutely declined to sanction any arrangement by Trinity College to secure for him the first tenure of the professorship, preferring to leave the University perfectly unfettered in its choice. But the Council of the Senate, which is a thoroughly representative body, chooses to signify the inseparable connection of the work with Dr. Foster's name by the very wording of the statute. The professor is to be elected by a board consisting of the Vice-Chancellor, of four members nominated by the Council, and four nominated by the Board of Natural Science Studies. One of each four must be neither resident in the University nor officially connected with it.

THE Cambridge mechanical workshops, organised by Prof. Stuart, bid fair to become of importance to research in the country generally, as well as in Cambridge. Prof. Stuart, on his own responsibility, has completely fitted up the workshops with all machinery necessary for the construction of philosophical apparatus. He has engaged a number of the most competent workmen as teachers, and to construct apparatus required by professors and investigators who are often deterred from researches because of lack of appliances or time to make what they want. Classes are formed for the regular instruction of university men in the use of tools and the construction of machines, and these are attended at present by a dozen students, several of whom intend to become engineers.

MR. A. C. HADDON, of Christ's College, has been nominated by the Board of Natural Sciences Studies, Cambridge, to study at the Zoological Station at Naples during the ensuing season.

DR. GREENFIELD, of St. Thomas's Hospital, has been appointed by the Senate of the University of London to succeed Dr. Burdon Sanderson as Professor of Comparative Pathology at the Brown Institution.

THE subscriptions already received or promised for the extension of the buildings of University College, London, amount to upwards of 14,000*l.*

BY the will of the late Mr. Charles Randolph, engineer, 60,000*l.* has been left to the building fund of Glasgow University.

THE third annual report of the Johns Hopkins University, Baltimore, issued by President Gilman, is of the highest interest, and shows that the attempt to establish a purely philosophical university has been eminently successful. Our readers are no doubt familiar with the principles on which this institution has been based. It was not sought to add one more to the many colleges already existing in the United States, but to found a genuine university in which those who had the inclination and the capabilities would have every facility for carrying their elementary or collegiate studies into the region of research in the various departments of human knowledge. The method of work has been carefully planned; the best men obtainable have been got to superintend the work of the students, who are admitted only on showing that they are really able and willing to pursue the courses which have been arranged. It is a many-sided and active centre of the highest learning, and cannot but have an invigorating result on science in all its departments in the United States. We would recommend those of our readers interested in the higher education to procure a copy of this report, which deserves a more detailed notice than we have space for.

THE Budget for Public Instruction will be deposited this week in the Bureau of the French Chamber of Deputies. A large increase is asked for in favour of public instruction. The credit granted will exceed two millions sterling. In 1823 it was only two thousand pounds, consequently in a little more than half a century it has been multiplied a thousand-fold. M. Bardoux will propose the creation in each department of a high school for popular education according to the models which have proved so successful in Paris. The benefit of the organisation realised in the capital will be extended to the whole of France if the scheme of the active minister is adopted, as will most probably be the case.

AT Stockholm the "Free" University was opened on October 14 last. The funds collected for its foundation now reach the sum of 820,000 Swedish crowns. It is intended to establish a similar university at Gothenburg.

SCIENTIFIC SERIALS

The American Journal of Science and Arts, October.—Besides two valuable papers by Professors Mayer and Draper, reproduced in our columns, we have here an account of the curious artificial mounds of North-Eastern Iowa, by Mr. McGee. They consist of tumuli, smaller conical mounds, embankments, and animal mounds, and from numerous measurements the builders seem to have used a unit which either was, or grew out of, the pace or yard. A slow southerly migration of the mound-builders is supposed to explain the evident increase in geometrical knowledge attested by various works found in passing across the United States from north to south.—Prof. Young furnishes details of observations of the Princeton Eclipse Expedition.—The flour-mill explosion at Minneapolis in May was probably due to the running dry of a set of stones which ground middlings, one of six sets discharging into a spout which communicated with a dust-house. Mr. Peckham studies the case, pointing out that there is greater danger with middlings, because it is dryer, and is ground at a higher temperature, and finer. The dry stones may heat the last part of the grist remaining, sufficiently to make it like tinder, so that it readily ignites on receiving a spark from the stones. The practical problem is how to prevent or detect dry stones, especially those for middlings.—Mr. Becker indicates the *rationale* of correction for vacuum in chemical analysis.—Prof. Smith writes on the composition of the new meteoric mineral, Daubreelite, and its frequent, if not universal, occurrence in meteoric irons.—Prof. Watson gives a more careful determination (than previously) of the intra-Mercurial planets.

Annalen der Physik und Chemie.—No. 9, 1878.—The excitation of electricity on contact of solid and gaseous bodies, forms the subject of an opening paper by Herr Beetz, who thinks the case is either one of differences of tensions produced by different conducting liquids, or of change of metals by gases which have ceased to be in the gaseous state, either through occlusion in the metals, or condensation on their surface.—From experiments on production of

galvanic currents by streaming of liquids through tubes, Herr Dorn infers that the motion of the liquid in itself produces no considerable part of the electromotive force observed; the influence of the tube wall, on the other hand, is undoubted.—Herr Wiedemann shows that an examination of magnetic behaviour of iron oxide salts is well adapted for determining with accuracy, even quantitatively, their dissociation in solutions at different temperatures, the conditions of their fixation by acids, and their exchange with other salts.—In the first portion of an inquiry into the divergences of some gases from Boyle's law at 0° and 100°, Herr Winkelmann gives an interpolation formula, expressing this divergence in the case of ethylene.—A new proposition in the theory of diffraction proved by Herr Frölich, is, that with small angles of diffraction, the kinetic energy of the incident light for an aperture of any shape is equal to the kinetic energy of the diffracted light.—Some experiments on the nature of the phases and change composition in telephonic transmission are described by Herr Hermann.—There are also notes on the relation between refraction equivalent and wave length, and on excitation of electricity by pressure and friction.

Journal of the Franklin Institute, October.—This contains a short account by Prof. Henry Draper, of his eclipse observations at Rawlins, Wyoming Territory, together with a photograph of the corona, showing the unequal distribution of its matter in the plane of the ecliptic and ray-like forms towards the poles of the sun.—Mr. Bell furnishes an account of the now historic "Camel" locomotive engine of Ross Winans, built in June, 1848. It first practically demonstrated the superiority of the eight-wheel connected engine for heavy traffic; it had also an inclined firebox, and other features of novelty.—The new system of electric lighting by Profs. Thomson and Houston, is described, consisting in causing one or both the carbon electrodes to vibrate to and from each other, so that the effect of the light produced is continuous. This allows of a feebler current being used.—Mr. Isherwood analyses some Scotch experiments on economic vaporisation of water and expansion of steam.

SOCIETIES AND ACADEMIES

London

Linnean Society, November 7.—Prof. Allman, F.R.S., president, in the chair.—Sir Joseph D. Hooker, C.B., presented to the Society, in the name of a committee of gentlemen, a portrait of the Rev. M. J. Berkeley. A great matted mass in sheet of a Chara (*Nitella* sp. ?) was exhibited by Dr. Thos. Boycott. It had been got from a dried-up pond in St. Leonard's Forest, Sussex, June, 1877; within its meshes many interesting microscopic forms were obtained.—Mr. Thos. Christy next called attention to living specimens of West African indian-rubber trees, the *Urostigma Vogelii*, and another undetermined species recently arrived. He likewise showed the fruit, flower, and leaf in spirit, with a dried ball of the gum of the commercially valuable *Landolphia florida*.—Dr. Maxwell Masters read an extract from a letter of Dr. Beccari describing a gigantic Aroid found by him in Sumatra, side by side with the *Rafflesia arnolii*. The species has a large tuber 5 feet round, from which is pushed up a single leaf, with a long, stout petiole, the divided blade covering an area of 45 feet, or 15 metres.—Dr. R. C. A. Prior showed a specimen of *Colletia cruciata* in blossom, grown out of doors in Somersetshire by the Rev. W. Sotheby.—"Notes on Euphorbiaceæ," by Mr. G. Bentham, read in title, was a paper treating of the history, nomenclature, systematic arrangement, and the origin and geographical distribution of this remarkable order of plants. Among Dicotyledons, Euphorbiaceæ stands fourth in point of numbers, having above 3000 species and 200 genera. In investigating the origin of the order the geological record, unfortunately, is of no assistance. Their evident, generally tropical nature, is a striking feature, and, judging from various data, it is conjectured that their most ancient home was in the old world. Their affinities have repeatedly been discussed by botanists, but though there are individual genera which may exhibit some one character supposed to ally to other orders, yet no real connection has hitherto been pointed out. Their isolation is produced, not so much by any one special character, as by a special combination of several. As to position in the linear series, unless the order be broken up, practically it must remain among the Monochlamydeæ, in spite of occasional presence of corolla in some forms. The author has a most interesting chapter on nomenclature and synonymy, well worthy the study and serious attention of biologists generally.—Mr. Lewis A. Bernays, in a letter to the secretary, records the undoubted existence of *Carpesium cernuum*, in Queensland, and suggests its being indigenous there.—In a paper given in abstract, "Descriptions of New Hemiptera," by Dr. F. Buchanan White, the diagnosis of 2 new genera (*Helenus* and *Neovelia*) and 17 new species are entered. These mainly are the results of Prof. Trail's late exploration of the regions bordering the River Amazon.—Mr. Alfred W. Bennett read a communication, "Notes on Cleistogamic Flowers; chiefly of *Viola*, *Oxalis*, and *Impatiens*." According to him there are two kinds:—(1) Those which hardly differ from the perfect open flowers, other than the partial or entire suppression of the corolla, and the closing of the calyx (= homocleistogamic); and (2) those with a distinct modification in the flower to aid self-fertilization (= heterocleistogamic). He was at first disposed to regard those two kinds as having arisen, one by degradation, the other by a rudimentary form of the organ: but subsequent examination convinced him that both kinds owe their origin to degradation. In the extreme cleistogamic flowers a large number of organs have been correlatively modified. Most interesting phenomena occur in the mode of emission of the pollen tubes, these travelling through the air in a straight line from the anther vertically upwards in *Oxalis*, horizontally in others, and creep along the surface and even back of ovary in *Viola canina*. An unseen agency directs, for none wander with uncertainty; and this is all the more remarkable because, when not in proximity to the stigma, the pollen grains protrude their tubes in all possible directions.—The Rev. G. Henslow orally delivered the gist of a paper "On the Absorption of Dew and Rain by the Green Parts of Plants" (*vide Science Notes*).—The Rev. W. W. Fowler and Messrs. Wilfred Huddlestone and T. M. Shuttleworth were elected Fellows of the Society.

Chemical Society, November 7.—Dr. Gladstone, president, in the chair.—The following papers were read:—Contributions from the Laboratory of Tôkio, Japan. On the red colouring matter of the *Lithospermum erythrorhizon*, by M. Kuhara. The purple colouring matter was prepared from the root by extracting with alcohol, purifying by treatment with lead acetate, &c.; it forms a dark, resinous, uncrystallizable mass, with a metallic green reflection, soluble in alcohol, ether, benzol, almost insoluble in water; it resembles in some respects anchurin, the colouring matter from alkanet. A bromine and a chlorine compound were prepared.—A second report on some points in chemical dynamics, by C. R. A. Wright, and A. P. Luff. The authors have continued their previous research and have determined the temperature of initial action of carbonic oxide, hydrogen, and carbon on various oxides of iron, manganese, lead, cobalt, and nickel. They find that the general law holds good, that the temperature of the action of carbonic oxide lies below that of hydrogen, which again is below that of carbon; this rule appears to be a special case governed by the general law that *cæteris paribus* the greater algebraically is the heat evolution taking place during a reducing action on a metallic oxide, the lower is the temperature at which the action is first noticeable during a few minutes' action.—Note on the constitution of the olefine produced by the action of zinc upon ethylic iodide, by Dr. Frankland and Mr. Dobbin. The gas given off was passed through a cohol and sulphuric acid, and then absorbed by antimonic chloride; on heating with water and distilling a chloride was obtained, boiling at 83° C.; it was therefore ethylenic, and not ethylidenic chloride.—On the occurrence of certain nitrogen acids amongst the products of the combustion of coal gas and hydrogen flames, by L. T. Wright. The author proves that the origin of the nitrogen acids found in the condensed water procured by burning coal gas or hydrogen in the air is ammonia, either free or combined, no such acids being produced when the gases are carefully freed from ammonia.—On the action of bromine upon sulphur, by J. B. Hannay.—Researches on dyeing. Part I. Silk and rosanilin, by Dr. Mills and Mr. G. Thomson. The authors have investigated the nature of the transaction which occurs when a vat is exhausted of its tinctorial ingredients. The experiments consisted in immersing a constant area of white silk in a solution of a rosanilin salt at a constant temperature for varying times, and then determining the loss of strength of the rosanilin solution.—Comparison of the actions of hypochlorites and hypobromites on some nitrogen compounds, by H. J. H. Fenton. The compounds selected were, ammonium carbamate, guanidine, and biuret.—Notes on two new vegeto-alkaloids by

F. von Müller and L. Rummel. The authors have prepared alstonin from the bark of alstonia constricta, and duboisin from the leaves and twigs of duboisia myoporoides; the latter closely resembles piturin.—On the determination of lithia by phosphate of soda, by C. Rammelsberg. The author confirms his previous results as to the formation of a double salt of sodium and lithium phosphate and the consequent inaccuracy in lithia determinations made by Mayer's method; he also gives some analyses of lithia micas.

Physical Society, November 9.—Prof. G. C. Foster, vice-president, and afterwards Prof. W. G. Adams, president, in th- chair.—The following candidate was elected a Member of the Society:—Sir Frederick Elliot.—Prof. W. G. Adams explained a simple appliance made by Mr. S. C. Tisley for exhibiting the coloured bands due to interference with thick plates. The bands due to regular reflection and refraction were produced by two thick plates nearly parallel to each other and fixed in a brass box with rectangular apertures on its flat faces so that the light fell on the first plate at an angle of 60°, the whole apparatus being of a convenient size for the waistcoat pocket. On a previous occasion (June 23, 1877), Prof. Adams exhibited these bands to the Society, but not in a portable form. The elliptical interference bands, due to the scattering or diffusion of light at a point on the front surface of one of the plates, were shown by means of a precisely analogous arrangement, except that the inclination of the plates to each other was somewhat greater; in this case the interference bands, formed by *regular* reflection and refraction, fall in another direction, so that they are not received by the eye; the diffusion interference fringes obtained were clearly visible when thrown on the screen. They are formed by rays once diffused from points on the first surface and afterwards regularly reflected and refracted from the front and back faces of the two plates in succession. Prof. Adams pointed out that this instrument would form a convenient means of obtaining polarized light in cases where the length of a Nicol's prism is objectionable, for instance, under the stage of a microscope; the light will be completely polarized if the plates be placed to receive the light at the polarizing angle, and the field will be much brighter than when a plate of tourmaline is employed.—Prof. W. F. Barrett exhibited and explained Edison's microtasimeter and carbon telephone. These have been recently described in NATURE. In the course of a brief recapitulation of the history of these instruments, he referred to Du Moncel's early observations, published in 1856, that variations in the resistance of a circuit can be produced by varying the pressure on metallic surfaces in contact, and after referring to Clérac's plumbago rheostats he stated that Edison was probably the first to apply the diminished resistance of carbon under pressure to a practical use, which he did early in 1877 in his carbon relay, the progenitor of the carbon rheostat, micro-tasimeter and carbon telephone. In all he uses compressed lampblack, a button of which may be formed as follows. The wick of a paraffin lamp having been cut so that it smokes, a quantity of lampblack is formed in the chimney; the lower portion, which has the more intense black colour, is collected from time to time, and all brown particles must be carefully removed, since they offer a greater resistance. The mass is compressed into a disc about the size of a sixpence, crushed, passed through a fine sieve, and again compressed, and this operation may be two or three times repeated in order to attain to perfect uniformity. The original form of tasimeter, in which the hard rubber or other substance was placed horizontally, has been modified so that the whole is vertical. The carbon button rests on a smooth metallic surface in connection with a binding screw, and a similar conducting surface rests upon it leading to a second binding screw. A strip of hard rubber 1 inch long, $\frac{1}{4}$ inch wide, and $\frac{1}{16}$ inch thick, is supported vertically above it, its upper end being attached to a fine screw worked by a tangent screw with graduated head. The whole is inclosed in a heavy conical brass box. Prof. Barrett suggested that it would be preferable to make this jacket cylindrical, and that the whole should be inverted, because the weight of the strip on the button is found to prevent the needle of the galvanometer returning at once to zero. Employing one Daniell's cell and inserting a shunt, Wheatstone's bridge, and resistance coils in the circuit, it was shown that the hand, at some distance, caused a considerable deflection, and Prof. Barrett stated that in a still room the instrument becomes so sensitive as to be almost unmanageable. By replacing the hard rubber by a strip of gelatine varnished on one side, a very slight change in the hygrometric state of the atmosphere can be detected by the absorption of moisture causing expansion of the gelatine, and, therefore, compression of the carbon. Its action as an aneroid baroscope was suggested by Prof. Barrett, the button being associated with an exhausted box. He pointed out that before the tasimeter can be used as a measuring instrument, experiments must be made in order to ascertain the exact relation between the resistance of carbon and the pressure to which it is subjected. The carbon telephone, full particulars of which will be found in NATURE, vol. xix. p. 12, was next described, and Mr. Adams, Mr. Edison's assistant, now in England, exhibited a complete transmitting apparatus, with call, &c. A very ingenious and simple form of shunt, received from Mr. Edison with the tasimeter, deserves mention. A row of brass studs fixed on a board are united by plugs, so that if the current enters at one end it can pass out at the other without meeting with any appreciable resistance. But if a plug be removed it throws in about 4 inches of a resisting wire wound over two rows of pins, underneath the board, one row of which is in metallic connection with the studs; thus the entire length of wire is in circuit when all the plugs are removed. Finally, Prof. Barrett mentioned that a communication has just been received from Mr. Edison stating that he has succeeded in arranging an efficient receiving instrument in which no form of magnet is employed.—Mr. Ladd then showed several forms of electric lamp arranged so as to render the use of clockwork unnecessary. In that known as Wallace's workshop lamp the spark passes between the edges of two plates, the lower one being fixed while the upper one is raised to a suitable distance by an electro-magnet brought into action immediately on the passing of the current. A second form, in which an annular magnet was employed, acted on the same principle, the armature carrying the upper plate being specially arranged so as to give a maximum of attractive force. In the third form, the V-lamp, two rods of graphite were inclined at an angle of 45° to the vertical, resting in contact on a piece of china. Immediately on the current passing an electro-magnet is caused to act, and, after the rods have been firmly gripped, they are separated and the support removed. Should the circuit be broken they will at once fall together.

VIENNA

Imperial Academy of Sciences, October 10.—The deaths of Dr. Rokitansky and Prof. Tomaschek were referred to.—The following among other papers were read:—The dolomite ridges of South Tyrol and Venetia (Heft 2 and 3), by Dr. Mojsisovics.—On electric penetration of glass, by Prof. Mach.—On the relation of diffusion-phenomena to the second proposition of the mechanical theory of heat, by Prof. Boltzmann.—Calorimetric research on the heat of combination of carbonic acid gas and ammonia gas, to carbonate of ammonia, by Herr Lecher.—Action of radiant heat of the sun on a body in shade—time of occurrence of maximum temperature, by Herr Schlemüller.—Description of a telescope, by means of which, with one objective, you may point on two objects simultaneously, one distant, the other near, (sealed packet) by Herr Schneider.—Physical experiments, by Dr. Gross.—Distance reflector with precision-reading, by Herr Kuczera.—Discovery of a comet, by Mr. L. Swift.—Meteorite fall at Tieschitz in Mähren, July 15 last year, by Herr Tschermak.—Development-history of the prothallium of *Scolopendrium vulgare*, Sym., by Dr. Beck.

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THURSDAY, NOVEMBER 28, 1878

RAMSAY'S MANUAL OF BRITISH GEOLOGY

The Physical Geology and Geography of Great Britain: A Manual of British Geology. By A. C. Ramsay, LL.D., F.R.S., &c., Director-General of the Geological Surveys of the United Kingdom. Fifth Edition. (London: Edward Stanford, 1878.)

THIS well-known work has now reached its fifth edition, and has undergone such changes that it may be almost regarded as a new book. Not only has the quantity of matter in it been almost doubled since the last edition, and its bulk and price augmented in the same proportion, but its plan has been very greatly modified, as is indeed indicated by the second title now prefixed to it.

The original work was justly deserving of the very great success which it achieved. It consisted of a corrected report of one of the well-known series of lectures to working-men delivered by the author in his capacity of Professor of Geology in the Royal School of Mines, at the Museum in Jermyn Street; and it was a model of clear exposition of a branch of science by one who was a master of his subject, and who at the same time had acquired great experience and skill in presenting it to popular audiences. Probably no better introduction to the principles of geology could possibly be recommended to the English reader than this little book of Prof. Ramsay's.

We must confess to feeling that this complete remodelling of the plan of a work that has already proved so successful, is a somewhat hazardous experiment. The original chapters of the book, which still retain the characteristics of popular lectures, do not always harmonise in style with the portions that were primarily intended for the pages of an encyclopædia. Indeed, as is admitted in the preface, the book now consists of two distinct works fused into one, and the reader is again and again reminded of the fact by somewhat awkward transitions and by abrupt changes in style and in the mode of treatment of the subject.

In almost every other respect we find the work to be worthy of the highest praise. The clearness and general accuracy of the information imparted by the book are as conspicuous in this as in the earlier editions, and Prof. Ramsay amply proves that he has not lost the most important gift which a teacher can possess, that of communicating his earnestness and enthusiasm to his readers. Many of the questions treated of at considerable length are of a somewhat controversial character, and the author has again and again to remark that he is teaching, not the universally accepted facts of the science, but the views which he himself has been led after long and careful study to adopt, and which he is sanguine enough to believe will be eventually accepted by all his brother geologists. Prof. Ramsay has certainly the merit of never being uncertain or hesitating in his convictions, and those who differ from, equally with those who coincide in opinion with him, will be glad to have the opportunity of reading his latest and most perfectly matured deliverances on questions in the discussions upon which he has long taken a very prominent part. We are bound to say that in respect to these matters he writes with the most perfect candour, and is ever ready to admit that there are subjects in which the timidity or caution of other geologists does not permit them to follow him in his bold generalisations.

The space at our disposal will not permit of our entering into detail on the numerous interesting questions suggested by a perusal of this book. The author's attempts to picture to the mind of his readers the ancient physical geography of our portion of the globe during successive geological periods may be cited as among the most graphic pieces of writing, and at the same time the most valuable portions of the work. Here Prof. Ramsay is evidently entirely in his element; he writes with an enthusiasm which is perfectly contagious, and his arguments, if not always sufficient to carry conviction, are at all times worthy of serious consideration.

We cannot resist quoting Prof. Ramsay's latest views on the important and much-vexed question of the classification of the Cambrian and Silurian rocks. On this subject he remarks:—

"If these strata were to be classified for the first time in England, with my present knowledge, I would divide them into three, as the most convenient method. The first series would include the purple and green grits and slates of the Longwynd and Wales, and range upwards as high as the top of the Tremadoc slates; the second would range from the base of the Arenig slates to the top of the Bala or Caradoc beds; and the third from the base of the Upper Llandovery beds to the top of the Ludlow rocks."

It is true that after this statement, which is in such perfect harmony with the results arrived at by palæontologists in Bohemia, Scandinavia, America, and our own country, Prof. Ramsay announces his intention of still adopting the nomenclature of Murchison and the Geological Survey, which he admits to be "old-fashioned;" but he states that his reason for doing so is simply that this plan will be the most convenient for those who wish to consult the geological maps and sections published by the Government. As the date fixed for the completion of the Government Survey is now passed, we may perhaps hope that the Director-General will be able to devote his attention to the much-needed reform of that old-fashioned classification and nomenclature. At all events, every geologist will be gratified by learning from so high an authority that any difference which may now exist concerning the classification of the older palæozoic rocks is mainly one as to the employment of certain terms, and that on the actual facts of the case something like substantial agreement has at last been arrived at.

On another question, that of the date of the earliest traces of human workmanship in this country, Prof. Ramsay's remarks are certainly not calculated to give quite so much satisfaction to his readers, He writes:—

"The antiquity of man being thus clearly established, it becomes obvious that his advent into our area was either of pre-glacial or of inter-glacial date. I say inter-glacial, because Mr. Skertchly has lately discovered palæolithic flint implements in certain brick-earths. Similar, and I believe identical, brick-earths underlie the 'chalky boulder-clay' in the neighbourhood, the boulder-clay having been removed by denudation from that

portion of the brick-earth in which the implements were found at Botany Bay, near Thetford, in Suffolk. The announcement at once provoked strenuous opposition, and therefore, on a tour of inspection of Mr. Skertchly's work with Mr. Bristow, we took care to examine into this point. The result was that I satisfied myself of the truth of Mr. Skertchly's observations that the implement bearing brick-earth in places underlies a boulder-clay, which, in his opinion, is not of the earliest date, in which case the men who made these tools must have been of inter-glacial age."

The "strenuous opposition" to which Prof. Ramsay refers, was directed, it will be remembered, not against the possibility of human remains being found under glacial deposits, but against such a conclusion being accepted without the clearest and most irrefragable evidence being adduced in its support. And it must be borne in mind that a number of most competent observers have examined the sections in question, and have arrived at conclusions directly opposed to those announced by the officers of the Geological Survey. When, therefore, our author, still speaking of this question of the contemporaneity of man with the glacial epoch, goes on to exclaim: "Perhaps we cannot prove it, but there is nothing improbable in the hypothesis, and I am not the only one who believes it," we cannot help entertaining the feeling that this is hardly the spirit in which a scientific question should be treated, and that the method which he adopts is one scarcely calculated to carry conviction to the mind of any competent judge of the matter.

In laying down this book we cannot refrain from once more expressing our opinion that it is a work of the highest value, and one worthy to take a foremost place among popular manuals of science. The illustrations are excellent; the woodcuts, by Mr. Sharman—giving a very faithful representation of species which have been selected by Mr. Etheridge as characteristic of the several formations—are quite new, and some views of scenery have also been added to those contained in former editions of the book. The little geological map of Great Britain, which we are glad to see reproduced, is a marvel of clear and accurate printing in colours, and well sustains the reputation of the publishing firm which has produced it.

FLORAL DIAGRAMS

Blüthendiagramme. Construirt und erläutert von Dr. A. W. Eichler, Professor der Botanik an der Universität Kiel. (Leipzig: W. Engelmann. Theil i., 1876, Theil ii., 1878.)

THIS book supplies a want that every real student of systematic botany must have felt. The introductory chapters are devoted to an inquiry into the morphology of the flower and its parts, and the inflorescence; while the subsequent chapters are a full exposition under the head of each family and order of the floral type and its most important modifications. Preceding each order is a list of the most important works bearing on it, and every quotation is accompanied by a full reference. Hence the book is both a Thesaurus of the literature of its subject, and moreover a Prodromus of phanerogamic morphology. Despite the modest title, the vegetative arrangements are explained wherever they present interest, and the same ungrudging pains are often extended to fruit and seed. Unlike too many authors Prof. Eichler is utterly free from provincialism. He cites as freely and constantly foreign botanists as those who have used the German language. Unfortunately we are but poorly represented, for morphological research is all but unknown in England, and is untaught by both our swarms of botanical lecturers and the great institutions which are the outward and visible sign of what Government recognises as botany. The medical curriculum has overborne original teaching by the former, the herbarium has stunted all else in the latter. Hence few of our botanists are able, like an Eichler or a Baillon, to check observations on the adult flower, with its parts distorted by drying and soaking, by their own knowledge of the growth of the living plant. Even the greatest sagacity and experience must be at a loss sometimes from this weakness in the very foundations of their work. For this reason one regrets the more that Eichler makes not a single reference to the works of Griffith, perhaps the greatest botanical genius England ever possessed, who found out for himself the value of developmental research and worked out many a flower by its aid.

A word on the method of Eichler. The actual editor of the "Flora Braziliensis," he adds to his thorough knowledge of morphology proper a rare acquaintance with systematic botany. Hence he belongs to no school, though awake to the value of workers in more limited fields, in all of which he himself has done good service. A firm evolutionist, he accepts the testimony of systematist, anatomist, teratologist, organogenist, and histogenist, and believes that all of these can in turn shed light on doubtful points. Hence his opinion must be respectfully considered by those who differ from it, and it is worth while to note a few of his conclusions.

He regards the nature of the "calyx tube" as varying with the order; truly receptacular in Rosaceæ, for instance, it is, partly at least, appendicular in some cases. The petals of Primulaceæ are regarded as true petals, and not as appendages of the stamens, a view which our descendants will have forgotten or unearth with the lazy amusement with which we look on some of the naif theories of our ancestors. The nature of the placenta and ovule is a more difficult question, and our author, who, in the preliminary chapters of Part i., published in 1876, held it essentially variable, has been led chiefly by Celanowsky's arguments to regard it as in all cases an outgrowth from the carpellary leaves. Similarly, the ovule, regarded in the First Part as a bud, is now viewed as an emergence. Of course the last word is not yet said on these points, but it is worth noting that Warming also avows his final conversion by Celakowsky, in his brilliant paper on the ovule in the first volume of the *Annales des Sciences Naturelles* for 1877; and Eichler is at one with Warming in adopting Brown's view of the female flower of Gymnosperms. It is much to be regretted that this point was not really discussed at the late congress in Paris, or that its principal advocates do not answer the latest arguments in its favour. But the question cannot at all be regarded as settled.

The cup of *Euphorbia* is regarded as an inflorescence; but though the *pros* and *cons* are fairly stated, no new light is shed on the matter.

Enough has been said to show the extreme value of the book to the scientific botanist. May its teachings quicken sound study in England. MARCUS M. HARTOG

OUR BOOK SHELF

Manuel du Voyageur. Par D. Kaltbrunner, Membre de la Société de Géographie de Genève. (Zurich, J. Wurster und Cie, Editeurs. Paris, C. Rheinwald und Cie, 1879.)

A GREAT difficulty of writing a treatise for the use of travellers, on "What to Observe," lies in the impossibility of presenting to the imagination an ideal average traveller to address. If the great mass of intending travellers had much the same amount of scientific knowledge and were well grounded in the elements of science generally, a very useful and compact work might, no doubt, be composed. But as a matter of fact such persons are usually very ignorant, or variously ignorant, and a book fitted to instruct the whole of them must omit none of the more elementary considerations, and therefore would assume the shape of a collection of encyclopædic treatises. It is hard to define the level of previous knowledge to which "Kaltbrunner's Manuel du Voyageur" is best adapted. Every reader is sure to think it too diffuse for his own wants in some parts, neither deep nor full enough in a great many, and probably beyond his depth in others; but take it all in all, it is perhaps better adapted for persons of moderate culture than any similar book that could be named. It is beautifully got up, with abundant illustrations, and to say the least, would be often useful for reference and as a reminder. The range of its topics is wide enough to touch the interests of everybody, and it would be a capital present to give to a friend bound for foreign parts. Considerable space is allotted to subjects connected with social life and other anthropological questions.

There seems to be some irony in the fact that when the world is so nearly explored, manuals for the use of travellers should begin to appear. They were greatly needed many years ago, when the Admiralty Manual had the field nearly all to itself, but now that the need is less, these works are at last composed in abundance. The present one, however, is by no means intended to supply the wants of those travellers only who are exploring unknown countries, much of it being applicable even to home districts. It is less solid and more comprehensive than the recent German publication, "Anleitung, &c.," by Neumayer.

Pleasant Ways in Science. By R. A. Proctor. (London: Chatto and Windus, 1879.)

OF this book, which has been sent me for review, I can truly say that it is an excellent specimen of what has been well called (I forget by whom) *Paper Science.* A very few quotations will amply justify this verdict.

At pp. 8, 152, I find "heat" several times standing for "temperature." But the author (in these columns, vol. xvi. p. 227) insisted that

"What *mathematicians call* the *moving force* exerted by the earth on the moon is eighty-one times greater than the corresponding force exerted by the moon on the earth."

To put "heat" for "temperature" is after all not very strange for one who puts "moving force" for "accelerating force."

In the account given of the experiments of Andrews and Tait on ozone, the action of "iodine" is given as that of "mercury": and the now-received idea of the nature of ozone—though *twice* mentioned in the paper referred to—is described as a "beautiful" and "ingeniously conceived" hypothesis suggested after the publication of the paper (pp. 351-2). . . .

The following passage, which refers to friends of my own, I quote without comment:—

". . . . no one, I think, would believe so ill of his fellow-men as to suppose for one moment that *advantage could be taken of the sympathies which have been aroused by the Indian famine*, or which may from time to time be excited by the record of great disasters by sea and land, *to advocate bottomless schemes merely for purposes of personal advancement.* We must now, *perforce*, believe that those who advocate the erection of new observatories and laboratories for studying the physics of the sun have the most thorough faith in the scheme which they proffer . . . ." (p. 51).

From p. 194 I gather that I know nothing about the motion of waves, and p. 240 proves me equally ignorant of voltaic electricity. I cannot read any more.

P. G. T.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Receiving Telephones

I HAVE roughly tried two experiments which seemed likely to supply new forms of receiving telephones, and have had such partial success as seemed satisfactory in preliminary trials. As I have not time to continue the experiments I request the insertion of this note in the pages of NATURE, in the hope that some one else will follow the matter up.

In one experiment a spiral wire (I used German silver, but it may be of any material) was wound closely so that the spires were in contact, or nearly so. One end of the spiral was fixed and the other end attached by a thread to the middle of a small parchment drum-head, such as is sold by the toy-makers for thread telephones. A slight tension was put on the thread to draw the spires of wire slightly asunder. The spiral wire was then made part of a circuit, including one or two cells of Grove's battery and a line wire going to another room. By this apparatus such sounds as the scraping on a file were satisfactorily heard, although the spiral was only one coil of about an inch long. It will be observed that in this arrangement there are no magnets; the whole effect is produced by the varying induction of the current upon itself. The apparatus could obviously be rendered more efficient by using a longer spiral, or a coil consisting of a number of concentric spirals not quite in contact, so as to allow small motions to exist. The induction might be still farther increased by using a spiral of two wires, so that a powerful local current might be kept up in the alternate spires, while the varying line-current is passing through the intermediate ones. Another improvement would consist in using iron wire wound in a sufficiently loose coil. The self-magnetisation of this coil would co-operate with the electric induction to heighten the effect.

In the other arrangement an iron or steel spiral (in my experiment it was an ordinary steel spiral spring, of which the spires lay close) was placed inside a coil of copper wire in circuit with a battery and the line. The spiral, as before, was fixed at one end, and kept slightly stretched by a string connecting its other end to a drum-head. In this arrangement no current passes through the spiral, but it is the core of an electro-magnet, and becomes magnetised in a degree which changes with the alterations in the intensity of the line-current. This causes the spires to attract one another with varying intensity, and the tremulous motion so produced is propagated by the string to the parchment. By this arrangement singing, whistling, &c., were heard when a Reiss transmitter was used. Probably a soft iron coil would have been better than the steel spring I used, and the apparatus is susceptible of other obvious improvements which would add to its sensitiveness.

Before concluding permit me to thank Prof. Barrett for allowing me to make the experiments in his laboratory.

Dublin, November 25 G. JOHNSTONE STONEY

The Microphone for Military and Tonometric Purposes

MY attention has just been called to a paper on the microphone, by Prof. W. F. Barrett, in NATURE (vol. xix. p. 12), in which it is asked whether the latter "has ever been tried by military men to detect the mining operations of an enemy?" Will you allow me to state that this application of the microphone suggested itself to me many months ago, and that I have begun to make experiments both in this direction and also with a view to ascertain to what extent sounds can be transmitted to a microphone immersed in water. Unfortunately the pressure of other matters has hindered me from completing the work, which, however, I hope shortly to be able to do.

May I take this opportunity of saying that I have been endeavouring with some success to apply the principle of the microphone to the counting of the beats of two slightly dissonant tuning-forks. In one experiment the two forks were inverted and screwed through a board above which the ends of their stems protruded. A thin piece of carbon was laid over these ends and the arrangement was placed in circuit with a Bell telephone. The beats were loudly heard and continued audible long after their direct sound had ceased. A reflecting galvanometer being placed in the circuit the beats were shown by deflections of the light spot, but irregularities in the current made it difficult to count them satisfactorily. The forks were then screwed horizontally into a vertical board and a screw was inserted about 1" from the stem of each and on the same horizontal level. A small piece of carbon was laid over each stem and its adjacent screw. This plan gave even better results, and admits of the forks being any reasonable distance apart. The experiments were tried with two forks whose vibration numbers were about 60, and also with a pair of octave forks with vibration numbers of about 256 and 512. A small piece of copper wire was then attached to each of the two large forks, and mercury cups were so placed that the points of the wires were just not touching the mercury surface when the forks were at rest. Both mercury cups were connected to one pole of the battery, and the current was arranged to branch through the forks uniting at the telephone. The beats were very loud. This plan, however, involves difficulties on account of the delicacy of adjustment required for the mercury surfaces and also the amalgamation of the copper-points. Several other experiments have been tried, but the method first described seems worth a trial by those interested in tonometry. The counting of beats, which is not an easy matter for aged or unaccustomed ears, may thus be immensely facilitated, while the period of the forks under observation is absolutely unaffected. The carbon used was that employed for the electric light, and it is probable that more carefully prepared and homogeneous material would have given better results in a galvanometer experiment.

GEORGE S. CLARKE

Cooper's Hill, November 19

The Microphone as a Receiver

ON the 3rd of June last, in a paper read before the Royal Society of Edinburgh, I described an experiment which showed that the microphone could be used, not only as a transmitter, but also as a receiver of articulate sounds. An abstract of the paper appeared shortly after in NATURE, and since then I have had communications from several experimenters, stating that they had failed to repeat the experiment, and asking for some details regarding it. I trust, therefore, that you will kindly give me space for a short explanation.

In performing the experiment the transmitting and receiving instruments which I used were precisely identical. Each was merely an ordinary white porcelain jam-pot, $3\frac{1}{2}$ inches in diameter and 4 inches deep, half filled with gas-coke, broken into coarse fragments and provided with electrodes whereby a current of electricity could be sent through the pieces of coke. Cinders from ordinary coal, if well burned, would, of course, do equally well. The electrodes were two strips of tin about two inches wide slipped down, opposite to each other, between the cinders and the sides of the jam-pot and fastened by being bent over the edges and bound round the outside with a cord. When these jam-pots were put in circuit like a pair of ordinary telephones, and a battery of two strong Grove's cells, or four ordinary Bunsen's, included in the circuit the arrangement was complete.

In this way I have had no difficulty in making myself and others clearly hear the transmission both of singing and speaking, although, as I stated in my paper, the articulation is not so distinct as I have no doubt it will be when proper forms both of transmitter and receiver are adopted.

I may mention that since then I have found the result to be greatly improved by including a stronger battery in the circuit.

Edinburgh, November 18

JAMES BLYTH

Wind-Pressure

I BECAME acquainted, some years since, with the singularly great wind-pressures registered at the Liverpool Observatory, and I should be rather disposed to attribute them to the exceptional position of the wind-gauge than to think (as the writer of an article in NATURE, vol. xix. p. 25, appears to do) that the gauge is erroneous. I do not remember to have seen it noticed, in the recent discussions on the Cleopatra's Needle, that there is probably a rapid increase of wind-pressure from the ground-surface upwards. In any river the velocity is least at the bottom, and near the bottom the change is rapid. Similarly, in the great current formed by the wind, I imagine there is a much less velocity near the ground than at some distance above it, and less on a plain than above a hill standing out of the plain. Now I believe the Bidstone Observatory is on a hill, with the great plain formed by the Atlantic in front of it. It is, therefore, in a position in which it receives an exceptionally heavy wind-pressure. The pressure on the wind-gauge is probably much greater than on the windows of the observatory, and that, again, is probably greater than the pressure on buildings more inland, where the current near the ground has been more interfered with by obstructions. On the other hand, Cleopatra's Needle is in a very protected position, where I should be much surprised to find that the wind-pressure ever reached even 40 lbs. per square foot.

As Mr. Dixon has referred to the case of a window to disprove the possibility of a pressure of 80 lbs. per square foot, it may be well to see whether it is really conclusive. I have not at hand any formula for the resistance of a simply-supported square plate, but it will not be very different from that of a circular plate. Now, let p = pressure per square inch on surface of plate, t its thickness, r its radius. Then the greatest stress in the plate is by Grashof's formula, $f = \frac{5}{6} \cdot \frac{r^2}{t^2} p$. Taking a plate of glass 2 feet diameter, $\frac{1}{8}$ inch thick, and loaded with 80 lbs. per square foot, we get $f = 4{,}270$ lbs. per square inch. In some experiments which I made under Sir W. Fairbairn's direction, the tenacity of glass was found to be from 4,200 to 6,000 lbs. per square in. Hence, surprising as it may seem, it is probable that a pane of glass 2 feet diameter would sustain a load of 80 lbs. per square foot, uniformly distributed, without breaking, or a load equivalent to that of a dense crowd of people. I don't, of course, think that a window would be safe if subjected to such a pressure, but it is always desirable to subject general statements of this kind to exact calculation; and I think we may at least infer that well-constructed glass windows would sustain a considerable wind-pressure without necessarily giving way.

W. C. UNWIN

Cooper's Hill, November 17

Was Homer Colour-blind?

I CRAVE some little space for a few remarks with regard to the recently much vexed question as to the traditional blindness of Homer.

It seems to have been overlooked that, apart from the statement made by Herodotus[1] (in his life of Homer), that in

[1] Homer, according to Herodotus, was born about 167 years after the Trojan war and, when still a child, adopted by his stepfather, to whom he succeeded in the management of a school. At an early age, however, it would seem, he set out for distant voyages and, at length, after having spent some time on visiting Tyrrhenia and Iberia, when about 32 or 34 years of age, lost his sight from what appears to have been some chronic disease of the eyes. Previously, when at Ithaca, he is said already to have had a narrow escape from that calamity. The text of this important narrative runs thus:—

. . . . *οἱ μὲν Ἰθακήσιοι λέγουσι, τότε μὲν παρ᾽ ἑαυτοῖς τυφλωθῆναι, ὡς δὲ ἐγώ φήμι τότε μὲν ὑγιῆ γενέσθαι, ὕστερον δὲ ἐν Κολοφῶνι τυφλωθῆναι; συνομολογοῦσι δὲ μοι καὶ Κολοφώνιοι τούτοις . . . . . . Ἀπικομένῳ δὲ ἐς Κολοφῶνα συνέβη, πάλιν νοσήσαντα τοὺς ὀφθαλμοὺς μὴ δύνασθαι διαφυγεῖν τὴν νόσον, ἀλλὰ τυφλωθῆναι ἐνταῦθα. Ἐκ δὲ τῆς Κολοφῶνος τυφλὸς ἐὼν ἀπικνέεται εἰς τὴν Σμύρναν* . . . .

From Colophon he sailed to Smyrna, where, for his sustenance, he began and, afterwards, continued, during his long wanderings, and for a good many years, the recital of his verses.

consequence of what appears to have been a chronic disease of the eyes, the poet lost his sight at the early age of about 32 or 34 years, and that he, once at least, styles himself a downright blind man, in his Hymn to Delian Apollo, ver. 169—73, which derives no mean authenticity from being so pointedly quoted by Thucydides, III., 104, and which runs thus:—

> . . . . *ὁππότε κέν τις ἐπιχθονίων ἀνθρώπων*
> *ἐνθάδ' ἀνείρηται ξεῖνιος ταλαπείριος ἐλθών·*
> *ὠ κοῦραι, τίς δ' ὔμμιν ἀνὴρ ἥδιστος ἀοιδῶν*
> *ἐνθ'δε πωλεῖται, καὶ τέῳ τέρπεσθε μάλιστα;*
> *ὑμεῖς δ' εὖ μάλα πᾶσαι ὑποκρίνασθ' εὐφήμως·*
> *τυφλὸς ἀνὴρ, οἰκεῖ δὲ Χίῳ ἔνι παιπαλοέσσῃ,*
> *τοῦ πᾶσαι μετόπισθεν ἀριστεύουσιν ἀοιδαί.*

> If some day an earthborn man, a wayfaring stranger,
> Asks you the name of whom best you like of all the minstrels you know,
> Whose songs are, oh lasses, the most delightful to you,
> Oh, then, unanimously, surely, you answer:
> It is the blind man who dwells in the rocky island of Chios,
> His songs are to us by far the sweetest of all.

I need not add, as a further argument, that Homer frequently was alluded to as the blind and humpbacked man, *ὁ κυφὸς καὶ τυφλὸς ἀνήρ*,[1] and it seems to me trifling to qualify, or mitigate, the racy juxtaposition of the two epithets.

To what extent colour hallucinations, so frequent in connection with certain forms of blindness, may possibly have impaired the poet's imaginative faculties with regard to the varying hues and shades of colour, it would be for the present, from want, for obvious reasons, of similar observations, difficult to settle. However, I cannot but think that what by some so recently has been called Homer's colour-blindness may be the natural consequence, on the one hand, of the increasing dimness of his recollections as well as owing to these optical hallucinations, and finally, to the defective chromatic terminology of his time. The following are some of the Greek and Latin authors who, together with Herodotus, aver and enlarge upon the blindness of Homer:—Plutarch, Vita Hom. 12; Thucyd., III. 104; Pausan., II. 33, 3; III. 4, 33; Lycophron, Cassandra, 422; Aristot., Orat., L. p. 703; Cicero, Tuscul, V. 39.

I refrain from discussing the question whether, from a physiological point of view, such a profound functional perturbation as is involved in the term of colour-blindness, viz., deficiency in the perception of any plurality of colours in the spectrum, would not seem to be symptomatical of most momentous organic disturbances in the nervous apparatus of the eye, generally conducive to the most hopeless forms of blindness.

Scientific Club J. HERSCHEL

IN reading Mr. Pole's article on Homer's sensations of colour, there is one point which seemed to me to call for explanation. Mr. Pole says that in the solar spectrum he sees only two colours, blue and yellow, and that the red space appears to him yellow. From this one would naturally infer that the whole of the spectrum visible to ordinary persons is visible to him also, but that it presents only these two colours, which graduate into one another without any break, and that the green space appears as yellow. And with a colour-blind person who has allowed me to test his capabilities, I found this actually to be the case. But later on Mr. Pole says that pure red and pure green appear to him not yellow but grey. I would wish, then, to ask Mr. Pole whether the spectrum presents to his vision, in place of the green, a neutral space or an interval of darkness? In other words, have the rays of that particular refrangibility no action at all upon his retina, or is it that they have no action peculiar to themselves, but simply produce the general effect of light? In either case the phenomenon seems more anomalous than if he saw all colours as colours, though he could only class them under two heads. To take a familiar analogy, it is as if a man should be perfectly able to distinguish the pitch of notes at either end of the scale, but the notes between should either not affect the auditory nerve at all, or should affect it simply as noise.

Pembroke College, Oxford FRANK PODMORE

Anthropometry

As I have stated in the preface that my object in publishing my "Manual of Anthropometry" is to invite criticism with a view to perfecting the anthropometrical chart which it contains, and which forms its chief feature, I may be excused for referring to the notice of the work which appears in NATURE, vol. xix. p. 29. The reviewer objects to the large number of measurements given in the chart, but he has overlooked my statement that many of them are of a secondary character, and that I leave the student liberty to select the measurements which best suit his purpose, requiring only of him that they shall be made and recorded in a uniform manner, and thus become the common property of statisticians. Anthropometry can make no progress as a science, so long as observers are at liberty to make the same nominal measurement of the body in four or five different ways, as is the case, for instance, with chest-girths.

I may add that my manual was not written for the three or four individuals in this country who have mastered the "theory of human proportions" as a mathematical curiosity, but for army surgeons, busy medical men, schoolmasters, and others who are much more concerned with actual facts than theories of probabilities. CHARLES ROBERTS

Bolton Row, W.

Divisibility of Electric Light

IN all communications on this subject in NATURE and elsewhere, the division of light is considered only with reference to parallel circuits, and this naturally causes great loss of light by the law that heating is proportional to the square of the current. But in electric circuits their resistance has always to be considered; and if two lamps are taken parallel, only half the resistance of the one lamp is obtained, and such resistance can be obtained by taking two parallel circuits of two lamps in series in each; the light obtained then is one quarter in each lamp, as half the current is flowing through each circuit, and as four quarters make a whole, no loss of light is caused by division in such a method of one current to any number of lamps. There are certainly practical difficulties in the way of burning lamps in series, though these are greatly diminished if incandescent wire is used as the light-emitting source. However, there is no inherent reason why the electric light should be wasteful in division, as is described by Mr. Trant. F. JACOB

Verification of Pervouchine's Statements regarding the Divisibility of Certain Numbers

THE statements of Pervouchine, reported in some recent numbers of NATURE, are equivalent to the following:—That the 2^{10} power of 16 is less by 1 than some multiple of $7 \times 2^{14} + 1$; and the 2^{21} power of 16 is less by 1 than some multiple of $5 \times 2^{25} + 1$.

Let r_n be the remainder after dividing the 2^n power of 16 by one of the above divisors. Then since the 2^{n+1} power of 16 is the square of the 2^n power, r_{n+1} differs from the square of r_n by a multiple of the divisor; or r_{n+1} is the remainder arising from the division of the square of r_n.

Use for the work the scale whose radix is 16. In this scale the above divisors are

1 12 0 0 1 and 10 0 0 0 0 0 1.

In the first case, calculating on the plan indicated, we find the remainders.

r_3 = –		5	2	4	9
r_4 = –	1	11	6	4	13
r_5 = –		5	9	10	6
r_6 = –		15	10	4	13
r_7 = –		1	0	10	15
r_8 = –	1	10	8	15	15
r_9 = –		14	5	2	11
r_{10} = –					1

[1] The very word of Ὅμηρος signified "blind" in the vernacular idiom of Κύμη, or Cumæ, one of the Æolian colonies in Asia Minor, where he lived for some time, and, as will be shown anon, accidentally came by the name of Homer, his original name being Melesigenes, from his happening to be born on the banks of the small river Meles, which flows by Smyrna and runs into the Smyrnæan sinus.

One day, pointing out how much of the poet's glory was certain to redound to their own city's glory, if the poet could be induced to settle among them, it was proposed to the people of Cumæ to provide during his lifetime for his wants, at the public expense, when somebody explained that such a resolution would be tantamount to inviting all sorts of blind, Ὅμηροι, and useless, people to their city, whereupon the proposal dropped. But it seems that, henceforth, the poet went by the name of Homer:—

Ὅμηρος ἐπεκράτησε τῷ Μελησιγενεῖ ἀπὸ τῆς συμφορῆς; οἱ γὰρ Κυμαῖοι τοὺς τυφλοὺς Ὁμήρους λέγουσιν. Ὥστε πρότερον ὀνομαζομένου αὐτοῦ Μελησιγενέος, τοῦτο γενέσθαι τοὔνομα Ὅμηρος.

Herodot. Halic., vita Hom., 2, 13.

Consequently the 2^{10} power of 16, or the 2^{12} power of 2, increased by 1, is divisible by $7 \times 2^{14} + 1$

In the second case, we find the remainders

$r_3 =$	− 4	0	0	0	0	1	10
$r_4 =$	− 7	9	9	9	7	0	11
$r_5 =$	− 9	8	2	10	9	9	4
$r_6 =$	− 4	6	6	11	8	8	11
$r_7 =$	− 2	2	3	12	10	8	5
$r_8 =$	− 9	14	1	0	14	6	10
$r_9 =$	− 6	9	14	11	8	11	0
$r_{10} =$	− 3	14	4	6	15	1	3
$r_{11} =$	− 7	7	10	5	12	10	4
$r_{12} =$	− 7	12	1	4	5	1	4
$r_{13} =$	− 7	3	7	12	8	1	2
$r_{14} =$	− 5	1	13	9	4	6	4
$r_{15} =$	− 3	15	2	2	4	13	3
$r_{16} =$	− 1	5	6	9	1	2	6
$r_{17} =$	− 2	5	9	12	5	9	3
$r_{18} =$	− 7	13	11	14	2	8	5
$r_{19} =$	− 8	3	0	0	6	15	7
$r_{20} =$	− 3	14	3	10	2	11	0
$r_{21} =$	−						1

Consequently, the 2^{21} power of 16, or the 2^{23} power of 2, increased by 1, is divisible by $5 \times 2^{25} + 1$.

The work involved in the first verification can be done by a good calculator in less than half-an-hour; the second is, I think, not more than thrice as long, for the divisions are more easily performed.

Here is one of the eighteen stages of the work of the second verification, namely, the getting of r_6 from r_5.

```
r5 = 9  8  2 10  9  9  4
   ---------------------
     9                         5  1
  1  2  8                         9  4  0
  1  3  0  2                         2  6  0  4
  1  3  0  4 10                         11 14  2 14  4
  1  3  0  5  4  9                         10 11  2 15  9  1
  1  3  0  5  5  2  9                         10 11  2 15 14  7  1
  1  3  0  5  5  3  2  4                          4 12  1  5  4 12  9  0
                               ---------------------------------------------
                               5 10  7  2  9 13  1  2
                               5  0  7  4  3  1  7, 4 10  4 11 13  0  0 +
                                                    9  0 11  7  6  1 11 −
                                                    -----------------------
                                            r6 = −  4  6  6 11  8  8 11
```

The last four lines of the work are made up thus:—In adding the parts of the square depress the last six places to line 2, leaving the rest in line 1; then proceeding to the extreme left, carry the tens figure, in this case 0, six places to the right, for subtraction into line 3, and depress the units figure (5) into line 2. Multiply the just depressed figure (5) by 16, and add to it what is found (10) in the place to the right of it in line 1 (giving 90); again carry the tens figure (9) for subtraction into line 3, and depress the units figure into line 2; repeat the process, moving to the right, until line 1 is exhausted; then the difference between line 3 and the last seven places of line 2 gives line 4, the result required.

For the sake of safety, before proceeding to calculate r_7, calculate r_6 again from the complement of r_6 with reference to the divisor, in this case from + 7 13 5 6 6 13. If the same result is again obtained, you may go on confidently.

Hampstead JOHN BRIDGE

Vulcan and Bode's Law

IN the year 1778—just a hundred years ago—the astronomer Bode published an approximation to a law respecting the planetary distances. He took the numbers

0, 3, 6, 12, 24, 48, 96, 192, 384,

each after the second being double the preceding; to these he added 4, giving

4, 7, 10, 16, 28, 52, 100, 196, 388,

numbers which, with the exception of the last, agree very well with the distances of the planets from the sun:—

3·8, 7·2, 10, 15·2, (27), 52, 95·3, 191·8, 300·3.

The publication of this law, at a time when the asteroids between Mars and Jupiter were as yet undiscovered, drew attention to Kepler's speculation that a planet was wanting between Mars and Jupiter. Twenty-one years after Ceres, the first of the asteroids, was discovered, and then others, until now there are nearly 200, the average distance of the whole being 27, and agreeing very well with Bode's number 28. All this is doubtless known to the majority of your readers.

In calling attention to the law, while not wishing to attach too much importance to it, I would point out one or two suggestions which present themselves. If we place 3 before the 0 in the first row of figures the line becomes

− 3, 0, 3, 6, 12, &c.

If 4 be now added the numbers are

1, 4, 7, 10, 16, &c.

The number 4 in this line represents the relative distance of Mercury from the sun; may not the number 1 represent the distance of Vulcan, or more probably the mean distance of a ring of asteroids, of which Vulcan is the brightest?

Referring now to the modified law, represented by the numbers

1, 4, 7, 10, 16, 28, 52, 100, 196, 388,

if 1 represents the mean distance of the Vulcan-asteroids, and 28 that of the Ceres-asteroids, it is a fact that after the first ring come four planets, Mercury, Venus, Earth, Mars, and after the second ring four planets, Jupiter, Saturn, Uranus, Neptune, the two sets of planets having marked differences as regards axial rotation and density.

What, then, is beyond Neptune? The law seems to say, a ring of asteroids at an average distance of 772. The motion of Neptune does not lead astronomers to suspect a planet beyond. Perhaps the optical instruments of the future may help to answer this question, Is there a ring of asteroids beyond Neptune? B. G. JENKINS

4, Buccleuch Road, Dulwich

Irish Bog Oak

CAN you or any correspondent kindly give me the scientific name of the Irish "bog-oak" (fossil)? It should be either *Quercus pedunculata* or *Q. sessiliflora*, the existing species, but though I have seen many specimens, I never got hold of one which would enable me to determine the species, and, for aught I know, there may be some of both. W. F. SINCLAIR

46, Guilford Street, W.C.

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE OF JANUARY 11, 1880.—The central line in this eclipse ends soon after reaching the coast of California, where it is possible totality may be witnessed close upon sunset. Tracing the previous path of the shadow through its long course across the Pacific with the aid of the Admiralty chart, it will be found that the only islands included within it are the Coquille, Bonham, and Elizabeth Islands, lying near together, between 169° and 170° E. longitude, and belonging to the Marshall Islands group. The eclipse passes centrally over the largest of the Coquilles, as laid down in the Admiralty chart of this group, according to a calculation in which the moon's place has been made to accord very nearly with Hansen corrected to Newcomb, which gives the following track:—

Long. E.	Lat. N. limit.	Lat. Cent. line.	Lat. S. limit.
°	° ′	° ′	° ′
168	... + 6 44·6	... + 6 28·0	... + 6 11·6
170	... 6 20·3	... 6 3·8	... 5 47·3
172	... 5 57·8	... 5 41·4	... 5 24·8

So that the breadth of the shadow in the direction of the meridian does not exceed 33′. Reading off from the chart, it will be found that the centre of the largest of the Coquille Islands is in about 169° 35′·5 E. and 6° 8′·5 N., and, calculating directly for this point, it appears that the total eclipse will commence at 8h. 41m. 25s. A.M. on January 12, local mean time, and continue 1m. 16s., and this represents the most favourable condition under which the eclipse can be observed on land. For any other point within the shadow in this vicinity the duration of totality may be determined by the fol-

lowing formulæ, where L is the east longitude from Greenwich, l the geocentric latitude, and t the Greenwich mean time of beginning or ending, according as the upper or lower sign is employed :—

$$\text{Cos. } w = +\ 109{\cdot}0051 - [2{\cdot}34285] \sin. l + [1{\cdot}98006] \cos. l \cos. (L - 15^\circ\ 15'{\cdot}9).$$
$$t = 11\text{h. } 10\text{m. } 48{\cdot}9\text{s.} \mp [1{\cdot}58154] \sin. w + [3{\cdot}16228] \sin. l.$$
$$- [3{\cdot}95668] \cos. l. \cos. (L - 126^\circ\ 35'{\cdot}7).$$

TRANSITS OF MERCURY.—Prof. Holden has published an "Index-Catalogue of Books and Memoirs on the Transits of Mercury," which he had prepared to aid him in a search for records of the physical phenomena which have been observed at such transits. The list is not quite a complete one, the publications of observatories not being included, but there is little inconvenience in the omission, as such observations and memoirs can be found by reference to the volumes for transit years, and Prof. Holden gives a list of the dates of all the transits of Mercury so far observed. Catalogues of this description must prove most serviceable to the student and to every one who has occasion to consult the general literature of an astronomical subject, and we hope the American astronomer may find leisure to continue them. Reference has already been made in this column to his very valuable "Index-Catalogue to the Literature of Nebulæ and Clusters," &c., forming No. 311 of the "Smithsonian Miscellaneous Collections." The publication above mentioned forms No. 1 of "Biographical Contributions," edited by Justin Winsor, Librarian of Harvard University. The copy before us is republished from the *Bulletin* of the library for October, 1878.

BIELA'S COMET AND JUPITER IN 1794.—It will be remembered by those who may have interested themselves in cometary astronomy, that between the first appearance of Biela's comet in 1772, and the next return at which it was observed, in the latter part of 1805, the elements had undergone alterations of a magnitude that occasioned doubts as to the identity of the comets, notwithstanding the general similarity of orbits, Bessel pronouncing against it, while Gauss pointed out that more than one revolution must have been accomplished in the interval, so as to admit of the comet having approached one of the larger planets and thereby experienced perturbation to account for the differences in several of the elements. The disturbing body is now known to have been the planet Jupiter, and there has been no difficulty in fixing the epoch when the comet's motion was most deflected, but we do not recollect to have seen the particulars of the near approach of the two bodies stated in any astronomical work. Starting from the final elements for perihelion passage in 1806, determined in the masterly investigation of the late Prof. Hubbard of Washington, it appears that neglecting planetary perturbation in the interval, the comet would have made its nearest approach to the planet at the beginning of June, 1794, when their distance was less than 0·47 of the mean distance of the earth from the sun. The following distances have been similarly obtained :—

1794	Distance from Jupiter.	1794	Distance from Jupiter.
March 2	0·654	May 31	0·469
April 1	0·562	June 15	0·473
May 1	0·496	,, 30	0·488
,, 16	0·477		

At the time of closest approximation, the heliocentric longitude of the comet was about 269° 40′, and the latitude + 4° 25′.

BIOLOGICAL NOTES

GALL-MAKING INSECTS.—At the St. Louis meeting of the American Association Prof. C. V. Riley read a paper on the gall-making *Pemphiginæ*. He said that the life-history and agamic multiplication of the plant-lice (*Aphididæ*) have always excited the interest of entomologists as well as of anatomists and embryologists. The life-history, however, of the gall-making species belonging to the *Pemphiginæ* has baffled the skill of observers more than that of any other group. Mr. Riley is about to publish some new biological discoveries relating to this family of insects, in connection with a descriptive and monographic paper by Mr. J. Monell, of the St. Louis Botanic Gardens. The paper laid before the Association simply records some of the yet unpublished facts discovered. All of the older writers, in treating of the different gall-producing *Pemphiginæ* of Europe, have invariably failed to trace the life-history of the different species after the winged females leave the galls, and, with few exceptions, have erroneously inferred that the direct issue from the winged females hibernates somewhere. The most recent production on the subject is a paper published in the present year in Cassel, by Dr. H. F. Kessler, which is entitled the "Life-History of the Gall-Making Plant-Lice, affecting *Ulmus campestris*." The author, by a series of ingenious experiments, rightly came to the conclusion that the insects hibernate on the trunk, but he failed to discover in what condition they so hibernate. Led by his previous investigations into the habits of the grape Phylloxera, Mr. Riley discovered, in 1872, that some of our elm-feeding species of *Pemphiginæ* produce wingless and mouthless males and females, and that the female lays but one solitary impregnated egg. Continuing his observations, especially during the present summer, he has been able to trace the life-history of those species producing galls on our own elms, and to show that they all agree in this respect, and that the impregnated egg produced by the female is consigned to the sheltered portions of the trunk of the tree and there hibernates—the issue therefrom being the stem-mother which founds the gall-inhabiting colony the ensuing spring. Thus the analogy in the life-history of the *Pemphiginæ* and the *Phylloxerinæ* is established, and the question as to what becomes of the winged insects after they leave the galls is no longer an open one. They instinctively seek the bark of the tree and there give birth to the sexual individuals, either directly or (in one species) through intervening generations.

LEAF ABSORPTION IN PLANTS.—The earlier experimenters on this subject, M. Perault, to wit, and Hales (1731), were persuaded that leaves absorbed dew and rain. For over a century the investigations of others supported this view, until M. Duchartre, in 1857, from his experiments, advanced a contrary opinion—that now held by most vegetable physiologists, and commonly taught in our schools. But, strange to say, gardeners, in their every-day operations, adopt a different notion from that prevailing in science. The subject has recently received the attention of the Rev. G. Henslow, who, in a paper read before the Linnean Society (November 7), shows that, while it may be true that, as Duchartre has said, dew is not absorbed by saturated tissues at night; yet, on the contrary, his (Henslow's) experiments go to prove that absorption *does take place* at and after sunrise, when transpiration recommences, and an indraught is caused by the moisture, wherever lingering on the leaves. He further corroborates M. Boussingault's late assertion, that, when leaves are purposely or naturally killed by excessive drought, they then do absorb water, as proved by the balance, or otherwise.

BRITISH NEWTS.—From an article by M. Ferrand Lataste in the last volume of the *Journal* of the Société Zoologique de France, it appears that the supposed fourth species of British newt—Gray's banded newt (*Ommatotriton vittatus*) of Mr. Cooke's "Our Reptiles"—may be altogether removed from the British catalogue. It was first introduced into the British list by Jenyns, in 1835, on the faith of some specimens found in a bottle in the

British Museum by the late Dr. Gray, which, being associated with some British newts, were supposed to have been obtained in the neighbourhood of London. Through a somewhat similar error, some specimens in the collection of the Jardin des Plantes at Paris were believed by Valenciennes to have been obtained in France, near Toul, and other examples were supposed to have been found living at Antwerp. It has thus come to pass that naturalists, copying one from another, have assigned "England, France, and Belgium" as the locality of this newt. It now turns out, from M. Lataste's researches, that all these localities are erroneous, and that the so-called *Triton vittatus* is no other than the *Triton ophryticus* of Berthold, an Eastern species of newt which is found in Syria and Asia Minor. The British newts are now, therefore, reduced to three in number—the crested newt (*Triton cristatus*) and the smooth newt (*Triton tæniatus*), both of ordinary occurrence, and the rarer palmated newt (*T. palmatus*).

SPERM WHALES ON EUROPEAN COASTS.—Prof. Turner, of Edinburgh, has been collecting and investigating a number of rare prints of sperm whales stranded on European coasts at the end of the sixteenth and beginning of the seventeenth centuries. One of these illustrates a whale caught in the port of Ancona in 1601, 56 feet long, 33 feet in girth; the scene is an active and lively one, representing a landscape, fishing-boats, men engaged in cutting up the whale, spectators, &c. The Netherlands seem to have had numerous specimens stranded. These, like those occasionally visiting the Scottish coast, are all males, which, when fully grown, appear to go singly in search of food. Other whales, as cachalots, visit the south in larger numbers. Over thirty cachalots, mostly females, were stranded in 1784 in the Bay of Audierne, department of Finisterre; and a school visited Citta Nuova, in the Adriatic, in 1853.

AMERICAN JURASSIC DINOSAURS.—Prof. O. C. Marsh publishes in the current number (November) of the *American Journal of Science and Arts* the principal characters of some new species of dinosaurs. On the flanks of the Rocky Mountains a narrow belt can be traced for several hundred miles, which is always marked by the bones of gigantic dinosaurs. The strata consist mainly of estuary deposits of shale and sandstone, and the horizon is clearly upper Jurassic; the dinosaurian remains in this series of strata are mostly of enormous size, and indicate the largest land animals hitherto known. One new species (*Atlantosaurus immanis*) must have been at least eighty feet in length and several others nearly equalled it in bulk. With these monsters occur the most diminutive dinosaurs yet found, one (Nanosaurus) not being larger than a cat. Some of these new forms differ so widely from typical dinosauria that Prof. Marsh has established a new sub-order to receive them, called Sauropida, from the general character of the feet. They are the least specialised forms of the order, and in some of their characters show such an approach to the mesozoic crocodiles as to suggest a common ancestry at no very remote period. In them the front and hind limbs are nearly equal in size; the feet are plantigrade with five toes on each foot. The carpal and tarsal bones are distinct; the precaudal vertebræ contain large, apparently pneumatic cavities; the sacral vertebræ do not exceed four, and each supports its own transverse process. The pubic bones unite in front by a ventral symphisis; the limb bones are solid. One of the species described and partly figured in Prof. Marsh's paper is called *Morosaurus grandis*; when alive it was about forty feet in length; it walked on all four legs, was probably very sluggish in its movements, and had a brain proportionately smaller than any known vertebrate.

ZOOLOGICAL STATION AT TRIESTE.—It may not be generally known that the University of Vienna in addition to having a zoological establishment in Vienna, has also founded a zoological station on the borders of the Adriatic Sea at Trieste. The general director of both is Prof. Dr. Claus. The assistant at Vienna is Dr. C. Grobben, and the inspector at Trieste is Dr. Ed. Graeffe. As a first fruits of these two excellent establishments Prof. C. Claus has published Part 1 of a handsome 8vo volume entitled "Work Done at the Zoological Institute of the Vienna University and at the Zoological Station in Trieste." The work done consists of 1. A very exhaustible memoir, by Dr. Claus, on a new species of Halistemma (*H. tergestinum*), with remarks on the minute structure of the Physophoridæ. This memoir is illustrated by five folding plates. 2. Contributions to our knowledge of the male reproductive organs in the Decapod Crustacea, with remarks on their comparative anatomy as compared with the same organs in the rest of the Thoracostraca, by Dr. C. Grobben, with six folding plates. 3. On the origin of the nervous vagus in the Selachians, with special regard to the electrical lobes in Torpedo; this is illustrated with woodcuts and one plate. The University of Vienna and Prof. Claus are indeed to be heartily congratulated at these first results from their new institution.

GEOGRAPHICAL NOTES

AT the meeting of the Royal Geographical Society on Monday last, a paper on "Usambara, East Africa, and the Adjoining Country," was read by the Rev. J. P. Farler, who has spent the last three years there in connection with the Universities' Mission. Usambara is described as the Switzerland of Africa, and forms a link in the great East Coast range, which extends from Abyssinia to Natal; roughly speaking, it lies between S. lat. 4° 20′ and 5° 25′, and E. long. 38° 20′ and 39° 10′. The mountains form four detached lines running due north and south, and rising in the higher peaks to about 6,000 feet above the sea-level. The range was evidently thrown up by volcanic action, and consists of granite mixed with spar, with sandstone in the lower spurs containing plumbago. Mr. Farler describes the scenery as varied and beautiful, now soft valleys and hill-sides with hanging woods, now again wild ravines with precipitous cliffs of bare granite. Usambara is drained by four rivers: the Zigi, with its affluent, the Kihuwi, the Mkulumuzi, the Ukumbini, and the Luari, the two first-named emptying into Tanga Bay; none of the four, however, are navigable. Trees are found in the region in great variety, but mostly of stunted growth; euphorbias, fan-palms, and mimosa thorns are seen everywhere, and occasionally baobabs, tamarind-trees, and clusters of the Borassus palm; there is also a kind of wild palm-tree. Various animals are found in the Mjika, or wilderness—antelopes varying from the size of a cow to that of a small goat, gazelles, lions, leopards, hyænas, and large apes. Mr. Farler mentions a noteworthy peculiarity in regard to the River Mkulumuzi, which in the rainy season becomes a torrent: "The stream has cut a deep bed for itself in the granite sides of the mountain, and exploring this bed in the dry season, I have found perfectly round, well-like basins in the rock, varying from a foot in diameter and depth to 10 feet in diameter, and from 8 to 12 feet in depth. There is always a stone at the bottom of these basins, and they must have been formed by the torrent giving, during the rainy season, a rotary motion to the stone." The soil throughout Usambara is a red disintegrated clay upon a granite and sandstone foundation, and covered with a rich vegetable loam; the bottoms of the valleys contain beds of alluvial clay. Probably no more fertile soil could be found in the world, and it is capable of producing every tropical plant. The flora of the region is extensive; in the forests are found ebony, copal, teak, acacia, the india-rubber tree,

the orchella weed, the betel-pepper climber, prickly smilax, with several varieties of the strychnos tree, and many other trees producing valuable wood. The inhabitants are many of them rather Semitic than Negro in their type, having high foreheads, while the prognathous jaw and spur heel are both wanting. They average 5 feet 7 inches in height, are strong, though not robust, and in form and figure resemble bronze statues. After describing the curious marriage customs of these people Mr. Farler concluded with some interesting remarks on the Masai country, which, sooner or later, must be thoroughly explored, so as to obtain a short route from the coast to the Victoria Nyanza.

At the same meeting Sir Fowell Buxton, at the special request of Sir Henry Rawlinson, gave an account of the progress of the road-making experiment from Dar-es-Salaam to the north end of Lake Nyassa. The work does not appear to proceed very rapidly, for but forty miles of road have been made in over twelve months, but it is satisfactory to learn that the natives give no trouble and willingly take to the good road provided for them; as, however, they still persist in their old habit of walking in Indian file, their traffic does not do much towards keeping down the rapidly growing vegetation.

It is now definitely settled that the Earl of Dufferin will preside at the meeting of the Geographical Society on Monday, December 9, and as an appropriate compliment to his lordship's early experiences as a traveller, the evening will be devoted to Arctic matters. We understand that the papers to be read will include an account of the Swedish Arctic Expedition now being so successfully carried out by Prof. Nordenskjöld, a review of the work done by the recent Dutch Arctic Expedition, suggestions as to the best route for future exploration, &c.

From a letter of Prof. Nordenskjöld's, published by Mr. Oscar Dickson, the liberal patron of the North-East Passage Expedition, we learn that during the short stay of the *Vega* at Vaigatz Island the scientific staff did some good work on the fauna of the sea and the flora of the land. A large collection of fishes was made, and special attention was given to Arctic phanerogamous plants. Nordenskjöld himself made some important purchases of "idols" from the Christianised Samoeides, who, notwithstanding their baptism, worship and sacrifice to their old divinities.

We have been favoured by a correspondent with the following extracts from a letter lately received from Mr. Carl Boch, who is exploring and collecting in Sumatra:—"I have been collecting for a month in the highlands of Mount Sago, and, considering the very bad weather, have been successful. My hut is on the south-eastern slope of the mountain, at an elevation of about 4,000 feet above the level of the sea. The mountain is about 8,000 feet high, and covered to the top with virgin forest. In about a week I purpose going on to Siedjoendjoeng, a place noted for its tigers, tapirs, and elephants, and said to be in every respect the best district for a naturalist. At Ayer Muntjer I met the celebrated Italian traveller, Signor Beccari, and stayed with him three days."

The Emperor of Austria has conferred the Order of the Iron Crown upon Drs. Gerhard Rohlfs and Georg Schweinfurth, the celebrated African travellers, and upon Drs. Alfred Brehm and Eugen von Homeyr, the well-known ornithologists.

The well-known African traveller, Dr. Nachtigal, has been elected president of the Berlin Geographical Society.

The *Russische Revue*, as referred to in Behm's monthly summary, contains some further details of Mushketow's recent exploration of the Pamir Mountains. He ascertained that the Pamir consists mainly of granite, metamorphic clay, and mica slate, covered with beds of triassic formation; at least in the northern part or Pamir Chorgosh. The direction of all the granite outcrops is that of the general direction of the Thian Shan, viz., east-north-east, or nearly so. North of the Pamir the granite soon ceases, and in the Trans-Alai Mountains diorite predominates, which takes the eastward direction of the main axis of elevation of the Trans-Alai Mountains, and forms the highest summits, which, as in Kaufmann Peak, reach a height of 25,000 feet. Further north, secondary formations prevail, with great diluvial accumulations. In the region explored by him M. Musketow could recognise no meridional elevation such as could favour the hypothesis of a meridional mountain-system, as was assumed by Humboldt.

In an article on foreign trade with Western China, contained in a recent issue of the *China Overland Trade Report*, we find some interesting notes on the intention of the Russians to push their trade southwards from the Siberian frontier. For this purpose a great commercial station is to be founded in the south-east of the province of Semipalatinsk—probably at the town of the same name, which is well situated for such a purpose, and is even now one of the chief commercial centres of Siberia. It occupies a good site on the east bank of the Irtisch, one of the most important rivers of Siberia, and has a population of several thousands. There are also many Tartar merchants in the place engaged in trade with the Chinese frontier towns in the north, Bokhara, Tashkend, &c. The Semipalatinsk caravans carry southwards printed Russian goods, copper, iron, and hardware, and return with tea, silk, dried fruits, &c. The warehouses of Semipalatinsk also contain carpets from Persia and Bokhara, costly silks and shawls embroidered with gold, ornaments and porcelain from China, diamonds, rubies, and emeralds, together with curiosities and jewellery of various kinds. There is likewise a large trade in cattle, herds of 4,000 or 5,000 being driven into the town by Kirghiz at one time; more than two million sheep are also sold there every year, most of them being forwarded on to Ekaterineburg, where they are killed and the fat used in the great candle-works of the town. It is thought possible that the Russians may intend to hold at Semipalatinsk the great *Yermak* or fair, which now takes place at Irbit, on the frontier, and to induce Chinese and Thibetan traders to go there.

An excellent little book has just been published by Hartleben, of Vienna. Its title is "Malta; Geschichte und Gegenwart, by Herr A. Winterberg. The work consists of three principal divisions. The first gives an exhaustive and well-written account of the topography, climate, position, and political division of the Maltese Islands, besides describing the agriculture, industry, commerce, and institutions of the little country. It closes with an interesting chapter on the physical and moral condition of the inhabitants. The second division treats of the islands from a military point of view, and contains minute descriptions of the fortifications, the various towns and villages, the harbours, bays, sources, and grottoes of the island. The final division, by far the most elaborate, is an ably-written summary of the history of Malta, which in its closing chapters has the additional interest of "showing us ourselves as others see us." The little book contains eighteen illustrations and two neatly-finished maps.

The first article in the November number of Petermann's *Mittheilungen* (it still retains the name) is on the use of elephants in African exploration, by the late editor, and was found on Petermann's table on the evening of his death. The number contains besides a short account by Dr. Miklucho Maclay of his visit to some of the Pacific Islands and New Guinea, and a paper by the same on Volcanic Phenomena on the north-east coast of New Guinea; an account of Bernoulli and Cario's travels in Guatemala and South Mexico in 1877; the

conclusion of Dr. C. E. Jung's Contributions to the Geography of Victoria; an important paper, with map, on the Chinese province of Kwang-tung and its people, by Herr J. Nacken; another on D'Albertis' New Guinea Exploration, with map of the Fly River; with papers on the Exploration of the Ogowé, Nordenskjöld's Voyage in the *Vega*, and Dr. Behm's monthly summary. Thus it seems that Dr. Behm, the new editor, is likely to maintain the reputation and value of this, the most important geographical organ.

THE October *Bolletina* of the Italian Geographical Society contains a short account of the Progress of the Italian African Expedition, and letters from Lieut. Bove who accompanies Prof. Nordenskjöld in his North-East Passage Expedition. In the *Bulletin* of the Paris Society is a translation of the Grand Duke Nicholas's paper on the Shortest Route for a Railway to Central Asia; a paper by M. L. Simonin on the Indians of the United States, with a map showing the Indian reservations; the continuation of Dr. Decugis' Account of his Journey in Morocco; a long article by the Abbé Ménager on Guinea, besides shorter papers on a Uniprojectional Atlas, and the Rio Casca of Peru.

THE Portuguese African expedition, under Serpa Pinto, which left Benguela a year ago, reached Bihé in March last, and was to enter the unknown interior in two divisions. The Lisbon Geographical Society are moving Government for a scientific expedition into Portuguese Senegambia.

ON SOME IMPROVED METHODS OF PRODUCING AND REGULATING THE ELECTRIC LIGHT[1]

AMONG the manifold functions which the elementary substance carbon performs in organic nature, not the least important is that by which it becomes the great source of artificial illumination, whether derived from oils, coal gas, or from coke rendered incandescent by the action of powerful electric currents. Since the time when Davy first produced the voltaic arc, between two points of wood charcoal, through which was transmitted the current from the great battery of 2,000 plates belonging to the Royal Institution, many experiments have been made to determine the best kinds of carbon for developing the electric light. The carbon which, until recently, was most commonly employed for this purpose, is obtained from the sides of gas retorts, where it accumulates in the form of coke during the destructive distillation of coal. The shells of coke from the retort are sawn up into pencils from one quarter to half an inch square, and from six to nine inches in length. Although very good results are obtained from carbon of this kind, it is a difficult material to work on account of its hardness, and it sometimes contains impurities which interfere with its conductivity. It is also liable to fracture when suddenly heated by the transmission of powerful electric currents. These defects have led to the introduction in electric lighting of artificial carbon, composed of powdered coke and lampblack, formed into a paste with molasses and gum. This material is pressed into cylindrical forms, and subjected for a given time to a high temperature in a special furnace. The manufacture of these carbon pencils has attained great perfection in the hands of Carré, of Paris, and they can be made into perfectly straight and cylindrical forms of from two to sixteen millimetres in diameter, and half a metre in length.

When the electric light is to be used for illumination, it is necessary that it should be as continuous as other modes of lighting. For this purpose not only should the current be regular in its action, but the distance between the carbon points must not alter, which necessitates the use of some arrangement for bringing them nearer together in proportion as they are consumed. Much ingenuity has been displayed by electricians in solving this problem, and the automatic contrivances invented by Staite, Duboscq, Foucault, Serrin, and others, leave little to be desired in regard to the steadiness of the light, when the regulators are in good order, and in the hands of intelligent operators. All automatic instruments, however, from the delicacy of their mechanism, are liable to derangement, and their action is not easily understood by persons not having a special knowledge of their construction. To obviate the objection to the use of such instruments by unskilled attendants, I devised, a few years since, a regulator for use on H.M.'s ships of war, to be actuated by hand. In this arrangement the carbons are made to approach and separate from each other by means of a right and left-handed screw connected with the carbon holders. Each of the screws, with its carbon holder, can be actuated independently of the other, for the purpose of adjusting the points of the carbons to the proper focus of the optical apparatus used in connection with it. The regulator, with its carbon points, is placed in the focus of a dioptric lens, which parallelises the divergent rays of light into a single beam of great intensity. The lens with the regulator is pivoted horizontally and vertically on the top of a short iron column, fixed on a raised platform above the deck, and the beam of light may be projected upon any distant object within its range. This special application of the electric light, however, as will be seen, requires the frequent adjustment of the carbons by the operator, but as he is always required to be in attendance to manipulate the projector, no inconvenience is experienced through the absence of the automatic arrangement. This method of regulating the electric light has now been in use in the Royal Navy for more than three years, and has proved very satisfactory.

Simultaneously with the progress of improvements in the mechanism for regulating the electric light, experiments have been made with the object of dispensing with the regulator altogether. The most recent, as well as the most successful, of these attempts has been made by M. Jablochkoff, a Russian inventor. In the specification of his letters patent of 1877 he proposes to place the carbons side by side (as had been previously proposed by Werdermann in 1874), and to separate them by an insulating substance to be consumed along with the carbon. The inventor states that the insulating substance for separating the carbons may be kaolin, glass of various kinds, alkaline earths, and silicates, which he prefers to apply in the form of powder rammed into an asbestos cartridge-case containing the carbons. A powder which the inventor found serviceable consists of one part lime, four parts sand, and two parts talc. These materials are rammed into the cartridge-case surrounding and separating two parallel sticks of carbon placed in the case, at a little distance apart. One of the carbons is made thicker than the other to allow for its more rapid waste. The lower ends of the carbons are inserted into pieces of copper tube or other good conductor, separated from one another by asbestos, and the ends of the tubes are pinched between two limbs of a screw vice, connected respectively to the conducting wires. This combination of carbons and insulating materials the inventor terms an electric candle, which, when mounted on a stand or candle-stick, has the appearance of the Roman candle of pyrotechnists. The inventor further states that the heat produced by the electricity fuses the material between the carbons and dissipates it; and the freedom of the passage afforded by the fused material to the electric current permits the subdivision of the light by placing several lamps in the course of one electric circuit. It is also stated that the construction of the candle may be varied; and, among the forms described, is one in which the carbons, instead of being contained in a cartridge case,

[1] Paper read by Mr. Henry Wilde at the Manchester Literary and Philosophical Society, October 29.

are separated by a partition of kaolin or other similar insulating material.

I have thought it well to describe, as nearly as possible in the words of the inventor, the electric candle, which is now the subject of so much attention in its application to electric lighting; so that its relation to what follows may be more clearly perceived. A remarkable peculiarity of the direct current in electric lighting is that of its consuming the positive carbon at twice the rate of the negative one, and while the negative carbon is a pointed cone, like that of a pencil, the positive pole takes the form of a hollow cavity or crater.

M. Jablochkoff's early experiments seem to have been made with the direct current, and hence his carbons are described as being of unequal thickness, in order that the positive and negative carbons of the candle might be evenly consumed. When the alternating current is used for producing electric light both carbons are of the same thickness, and are consumed at an equal rate, and both points terminate in regular cones. This property of the alternating current, besides other advantages, always maintains the luminous point in the focus of any optical apparatus used in connection with it, that is, when the carbons are placed end to end, as I had occasion to point out in a former paper read before the Society in 1873, on an electro-magnetic induction machine for producing alternating currents.

M. Jablochkoff, in the course of his experiments, would appear to have met with some difficulties in adapting the direct or continuous current to a system of lighting with his electric candles, and now uses the alternating current for this purpose. The candle has also been simplified by substituting a slip of plaster of Paris for the cartridge and partition of kaolin formerly employed.

To produce the alternating currents, however, to supply a number of lights, it was found necessary to employ powerful electro-magnetic induction machines, excited by the currents from other smaller machines, according to the principles laid down in my paper read before the Royal Society, and published in the *Philosophical Transactions* of 1867. From sixteen to twenty lights are produced from one of these electro-magnetic machines, each light absorbing about one-horse power.

The system of electric lighting above described would now seem to be definitely established in some places as a substitute for gas, and visitors to the French capital during the present summer will have been struck with the fine effects produced in the avenues and squares where the light is displayed.

My connection with the history of this system of lighting placed me in a position to make some experiments with the Jablochkoff candle, and led to the discovery of the following facts. One of the conditions necessary for producing a constant light from the candle, in its most recent form, was that the quantity and intensity of the alternating current should be such that the carbons consume at a rate of from four to five inches per hour. If the electric current were too powerful, the carbons became unduly heated, and presented additional resistance to the passage of the current; the points at the same time lost their regular conical form. If, on the other hand, the current were too weak, the electric arc played about the points of the carbons in an irregular manner, and the light was easily extinguished by currents of air.

In the course of these experiments I was struck with the apparently insignificant part which the insulating material played in the maintenance of the light between the carbon points; and it occurred to me to try the effect of covering each of the carbons with a thin coating of hydrate of lime, and mounting them parallel to each other in separate holders, and without any insulating material between them. The use of the lime covering was intended to prevent the light from travelling down the contiguous sides of the carbons. On completing the electric circuit the light was maintained between the two points, and the carbons were consumed in the same regular manner as when the insulating material had been placed between them.

Two plain cylindrical rods of carbon three-sixteenths of an inch in diameter and eight inches long, were now fixed in the holders parallel to each other, and one-eighth of an inch apart. The strength of the alternating current was such that it would fuse an iron wire 0·025 of an inch in diameter and eight feet in length. On establishing the electric current through the points of the carbons by means of a conducting paste composed of carbon and gum, the light was produced, and the carbons burnt steadily downwards as before.

Four pairs of naked carbons mounted in this manner were next placed in series in the circuit of a four-light machine, and the light was produced from these carbons simultaneously, as when the insulating material was used between them. The light from the naked carbons was also more regular than that from the insulated ones, as the plaster of Paris insulation did not always consume at the same rate as the carbons, and thereby obstructed the passage of the current. This was evident from the rosy tinge of the light produced by the volatilisation of the calcium simultaneously with the diminution of the brilliancy of the light from the carbons.

The only function, therefore, which the insulating material performs in the electric candle, as shown by these experiments, is that it conceals the singular and beautiful property of the alternating current to which I have directed attention.

As I have already said, the strength of the alternating current must bear a proper proportion to the diameter of the carbons used, and when a number of such lights are required to be produced in the same circuit, the quantity and property of the current will remain constant, while the tension will require to be increased with the number of lights.

This simple method of burning the carbons will, I believe, greatly further the development of the electric light, as the carbons can be used of much smaller diameter than has hitherto been possible. They may also be of any desired length, for as they are consumed they may be pushed up through the holders without interrupting the light. One of these developments will be a better method of lighting coal and other mines. In this application the alternating currents or waves from a powerful electro-magnetic induction machine may be used for generating, simultaneously, alternating secondary currents or waves in a number of small induction coils, placed in various parts of the mine. The light may be produced in the secondary circuits from pairs of small carbons inclosed in a glass vessel having a small aperture to permit the expansion of the heated air within. Diaphragms of wire gauze may be placed over the aperture to prevent the access of explosive gas. By generating secondary currents or waves without interrupting the continuity of the primary circuit, the contact-breaker is dispensed with, and the subdivision of the light may be carried to a very great extent.

A STUDY IN MAGNETISM

THE name of Faraday will go down to posterity foremost amongst the names of the scientific men of this century, for the simple comprehensiveness and original beauty of his researches in electricity and magnetism; chiefly, perhaps, for his discovery of magneto-electricity—the kind of electricity that can be induced in conductors which are caused to pass near magnets. Those who have carefully read Faraday's works know how he was led to this discovery by the conception he had formed of magnetic force. Until his time magnetic

attractions and repulsions had been explained as a kind of action-at-a-distance. Faraday explained them as the results of the action of the medium filling the intervening space; and he gave several indisputable proofs that the space surrounding a magnet was thrown into a peculiar condition by the presence of the magnetism. Two centuries previously another Englishman, as uniquely

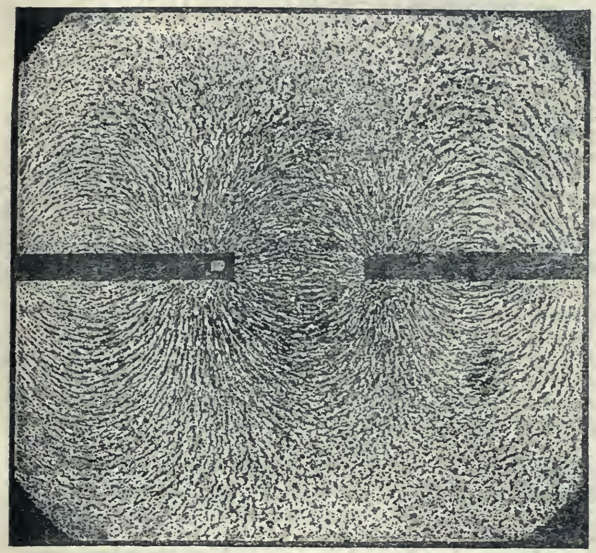


Fig. 1.

great if not greater, Dr. Gilbert, had in his famous treatise "De Magnete," told how iron filings sprinkled on a piece of card beneath which a magnet lay, assumed certain mysterious lines. To these lines Faraday gave the name of *lines of force*, and showed that they represented, wherever they went, the direction and strength of

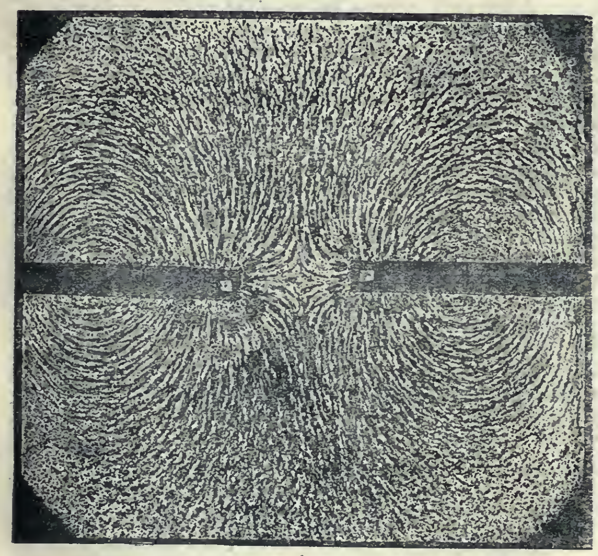


Fig. 2.

the magnetic force. His imagination seized upon these mysterious lines, and he saw all space, wherever a magnet had influence, traversed by them. He perceived that they were in some way bound up with that which was mysterious and unexplained in this seeing action-at-a-distance. He found them to react on one another, and to follow certain definite laws ascertainable by experiment. In the volumes of his researches he filled several entire plates with drawings of the figures assumed by the lines under various combinations. They had taught him to anticipate magneto-electricity and electro-magnetic rotation. He had diligently followed them up from the hint afforded by Dr. Gilbert's experiment with the iron filings. He had begun to apply the method to

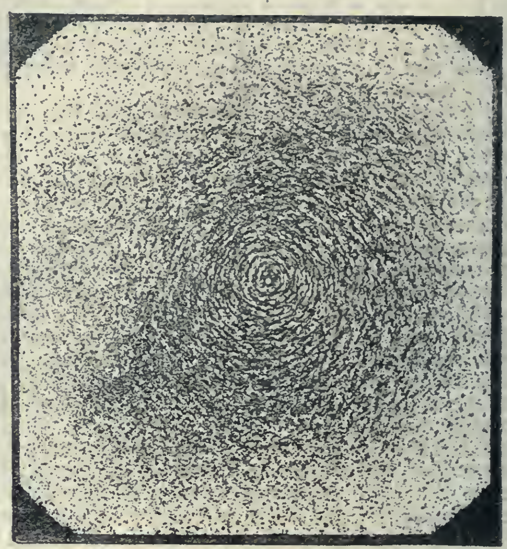


Fig. 3.

the investigation of the interaction of electric currents, when the decay of age overtook him, and the research dropped from his grasp. Had he lived the study which the writer of the present article is about to narrate would have been completed long ago.

The experiment of laying a card or a sheet of paper

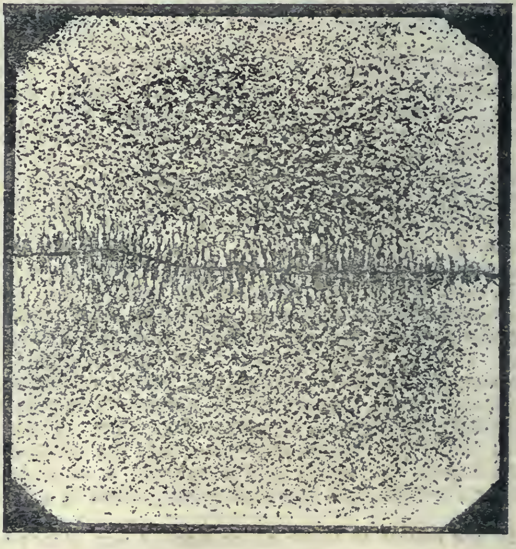


Fig. 4.

upon a magnet, sprinkling over it fine iron filings, and then tapping the card gently so as to allow the filings to take up their places in the "curving lines of force," is one which always possesses a peculiar interest and fascination for youthful electricians. Two other experiments, due originally to Musschenbroek, are not quite so familiar, though they are as simple; and since they have

a special bearing on that which follows, we will mention them in detail.

Let two bar-magnets of steel be placed on the table with the north-seeking pole of one towards the south-seeking pole of the other, but not touching. Over these lay a sheet of stiff writing-paper, or card, or a sheet of window-glass. Fill a pepper-box with fine iron-filings,

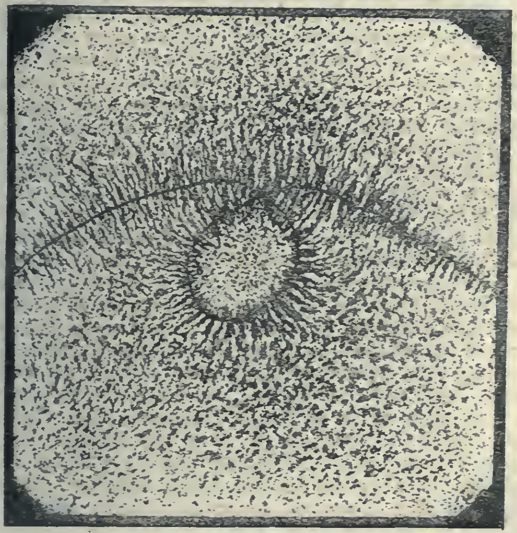


FIG. 5.

and sprinkle them evenly over the sheet; then tap the sheet gently, until the filings have arranged themselves. Observe (Fig. 1) that the lines of force run across from pole to pole. A line of force represents the direction in which the forces act. Suppose that the pole on the right is the north-seeking pole, and that on the left a south-seeking pole. The forces act across the space between them in

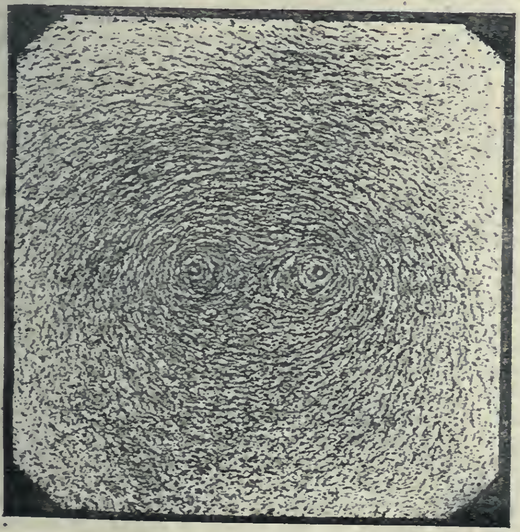


FIG. 6.

the continuous curves from pole to pole. Suppose you could obtain a piece of steel imbued with magnetism of one kind of polarity only—a magnetic "particle," in fact, of the same kind of magnetism as the north-seeking pole. If you were to put that magnetic particle down on one of these lines near the north-seeking pole on the right, it would, as we know, be repelled away, since similar poles repel one another. And it would move away along the line of force (for that line of force represents the direction in which the force acts), and would pass right over and be attracted to the south-seeking pole on the left. Similarly a magnetic particle of south-seeking polarity, if we could get one and place it down on a line of force, would be driven along the line in the opposite direction.

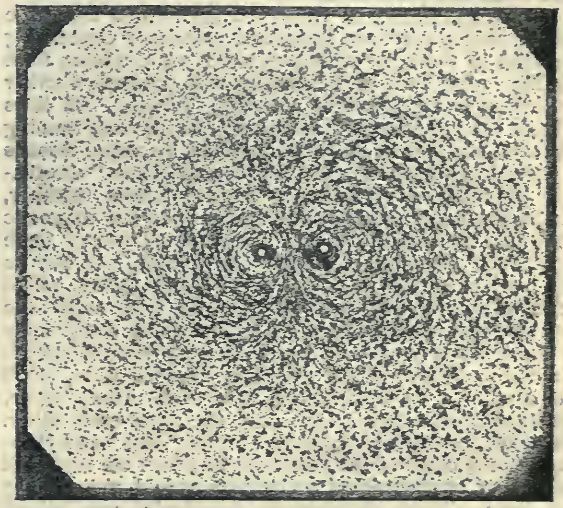


FIG. 7.

Notice, too, that a great many of the lines of force that run out of one pole run into the attracting pole opposite. This you will find always to be the case when two poles attract: their lines of force run into one another.

As a second experiment lay down the two magnets, but put their north-seeking poles towards one another, and then lay on them a sheet of card or glass, and sprinkle

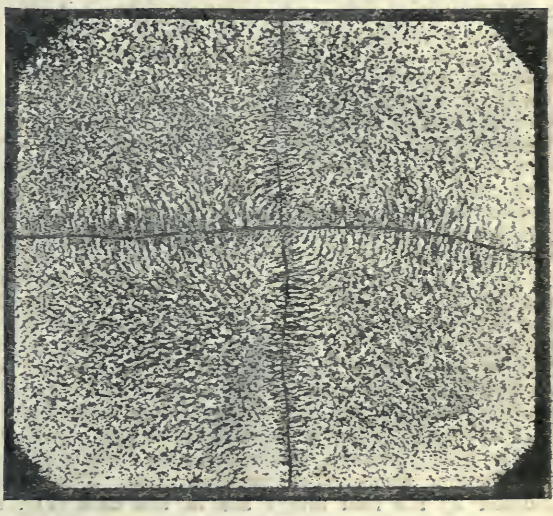


FIG. 8.

filings over, as before. The curves you obtain, which are like those of Fig. 2, are quite different from those obtained before. This time the lines do not run across from pole to pole. They start out, but instead of uniting and blending together, and bending over to run into one another, they turn away sharply where they encounter one another, and, without ever joining, swerve aside in

almost parallel paths. If our supposed particle of north-seeking magnetism were placed on a line near one pole it would not pass over from pole to pole, but would follow the line where it swerves away to the side. We know (by experiment) that two north-seeking poles repel one another, and we see here that the lines of force of two such poles never run into one another, but turn aside mutually repellant. You will find that this is always the case when two poles repel.

Such experiments as these led Faraday to enunciate several simple yet most important principles concerning the lines of force, by means of which we can learn from the lines what kind of action they will produce, whether attraction, or repulsion, or rotation. *Firstly.*—All lines of force tend to shorten themselves. If the lines running across in our first figure were replaced by actual threads of stretched elastic material, we see that any "shortening" of them would bring the poles nearer together, which, indeed, is precisely the tendency of the magnetic attraction between the poles. *Secondly.*—Lines of force repel each other when placed side by side. If this be the case then the lines in our second figure, which bend outwards, and run off side by side, repel one another, the two poles must be experiencing a tendency to move away from one another; and this we know is the case. *Thirdly*, Like magnetic lines of force, when "end on" to each other, run into each other; while, unlike magnetic lines, when end on, repel each other. Here, of course, we apply the terms "like" and "unlike" to the cases of the directions in which our supposed particle of north-seeking magnetism would move along the lines. These notions of Faraday's are full of meaning, and it is not many years since Prof. Clerk Maxwell showed how well they agreed with the most perfect mathematical expression of the forces that operate in the medium filling the surrounding space.

Keeping these simple principles in mind, let us apply them to some further cases of magnetic action, and see if they are equally applicable. We know that the wires carrying electric currents possess certain magnetic properties, and will deflect magnetic needles; that two electric currents may attract or repel each other; and that current may make a magnet pole rotate round it. Can we explain such electrodynamic actions also by applying the principles of Faraday to the magnetic lines of force existing in these various cases?

In the first place, what are the lines of force belonging to a wire through which an electric current is passing? To ascertain this we will bore a hole through a card or a piece of glass, and pass a wire up through the hole. Then, joining the ends of the wire to the poles of a powerful battery, we will, while the current is passing, sprinkle on iron filings, and, tapping lightly, will permit them to assume their places in the lines of force. Fig. 3 was thus obtained. It shows us a series of concentric circles. If a supposed north-seeking magnetic particle were put down on one of these circles it would move round and round in one direction; supposing the current to come up through the hole, this direction would be opposite to that of the hands of a watch. If the current went down through the hole, the movement would be the other way round. But we may examine the current in another way. Lay the conducting-wire down flat, and place over it the card or piece of glass. The forms assumed by the iron filings are in this case (Fig. 4) straight lines across the wire—are edge-views, so to speak, of the systems of circles we just now obtained.

These two figures were discovered by Faraday, and are given in his researches. They are also given by Dr. F. Guthrie in his book on "Magnetism and Electricity."

If we wind up our conducting-wire into a simple knot or loop, carefully preventing the overlapping parts from touching, the figure obtained with the iron filings is like that of Fig. 5. It is interesting to observe how in the middle of the loop there are no lines, only dots. The lines of force run through the loop, perpendicularly to its plane, and we only see them end-ways as points. It is clear that a magnetic particle such as we have imagined would be either attracted into the middle of the loop, or would be repelled out of it, according to its polarity.

Now what is the effect of carrying two parallel currents through two wires side by side? Take a piece of card or glass, as in Fig. 6, having two holes in it; through these pass a couple of wires joined to two batteries, so that the two currents are either both ascending or both descending through the flat surface. The magnetic field mapped out by the iron filings will then show a series of curves, the outermost of which are rough ovals inclosing both the currents, whilst the innermost are small ovals round each wire. The lines between the inner and outer systems present a sort of hour-glass shape or *lemniscate.* Had the two parallel currents, however, passed in opposite directions through the plate, one ascending and the other descending, the filings in the magnetic field would have taken the form given in Fig. 7. Here we find two separate systems of distorted and flattened circles surrounding the wires, each separate system of circles having displaced the other. The outer curves do not run into each other as in the preceding case. Let us apply Faraday's principles to these two figures. In the former (Fig. 6) any "shortening" of the exterior lines would tend to draw the centres nearer together. In the latter case (Fig. 7) no such consequence need result. A tendency of the successive lines to repel each other and to maintain equal distances from each other, would in the former case tend to reduce the entire figure to a system of concentric circles, which could not be accomplished unless the two centres approached each other and coalesced. In the latter case, since the systems of lines round the two centres never join across, this tendency would have the result of driving the two centres far apart to allow of the lines becoming perfect sets of circles. Now we know from Ampère's classical researches on parallel currents, that they attract one another when they run in the same direction, but are mutually repellant when they run in opposite directions. Our application of Faraday's principle enables us to foresee this electro-dynamical action as a consequence of the distribution of magnetic force in the field. In an exactly similar manner we may reason out the action of the forces in the field which is produced by two currents crossing one another at a right-angle (Fig. 8), the conducting wires attracting one another across those quadrants in which the currents flow both towards or both from the point of intersection.

We may apply our study further and investigate, with iron-filings, the action which currents exert on magnets. Let us conduct a current vertically through a hole in a plate, and fix near it a small magnetic needle, as in Fig. 9. The needle has been placed so as to point with one pole towards the current. The lines of force radiating from that pole run round and coalesce on one side with the circular lines of force of the current. On the other side of the pole they absolutely refuse to unite with the circles, and repel them away. Clearly, the "tendency to shorten," which Faraday predicated of the lines, would drag the pole of the magnet in one direction round the current. Looking at the other pole of the magnet we see that the tendency acts in the opposite direction, so that the total result would be a tendency to turn round the magnet about its middle point, and set it at right angles to its present position. This consequence, too, is, as we know from Oersted's famous experiments, the fact.

If, instead of laying the needle down flat, we had reared it up on end, as in our Fig. 10, where a square black

spot marks the place of the pole, we should perceive that the systems of circles round the current and of rays round the pole mutually disturbed each other, and that the figure was consequently unsymmetrical. Round one half of the figure the lines coalesce ; round the other they repel each other, and stream away. Applying the notions we have already obtained, we see that the result will be a

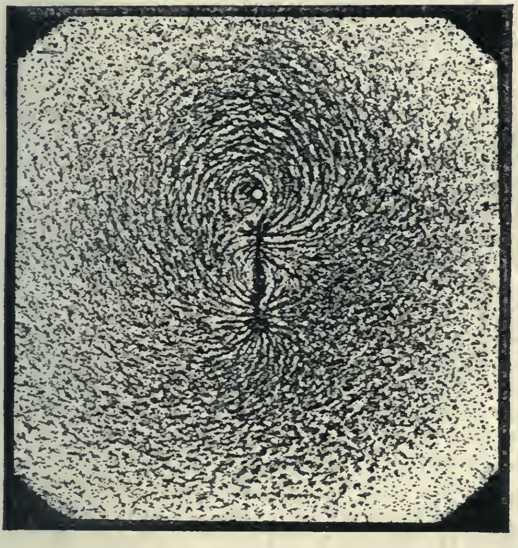


FIG. 9.

force tending to make the pole and the current rotate round each other. This, we know, was shown by Faraday himself to be the case, for when one was fixed and the other free to move, the one rotated round the other. Carry on the idea one stage further, and make the current run up through the plate at the precise point where the pole

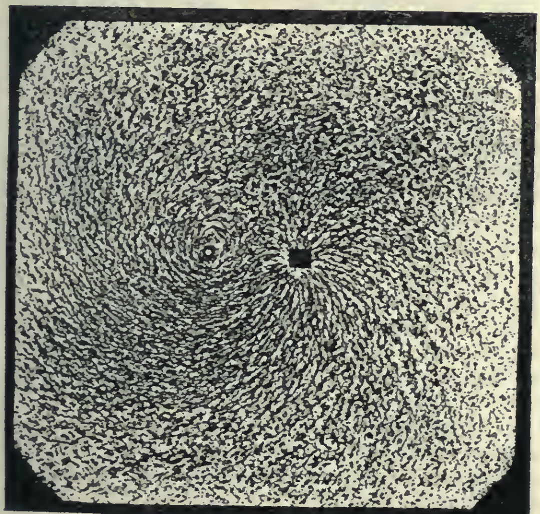


FIG. 10.

of the magnet is. Let it run up through the magnet. The "field" we obtain (Fig. 11) shows us neither the rays of the magnet nor the circles of the current, but a set of beautiful spirals unwinding from a common centre. What kind of motion can we deduce from this remarkable figure? If the branches of the spiral could shorten themselves they would certainly rotate the central region round on itself. The corresponding fact exists in another of Faraday's discoveries: that a magnet can be made to rotate round its own axis, under the influence of a current running up it through one of its poles.

One experiment more will close for the present our

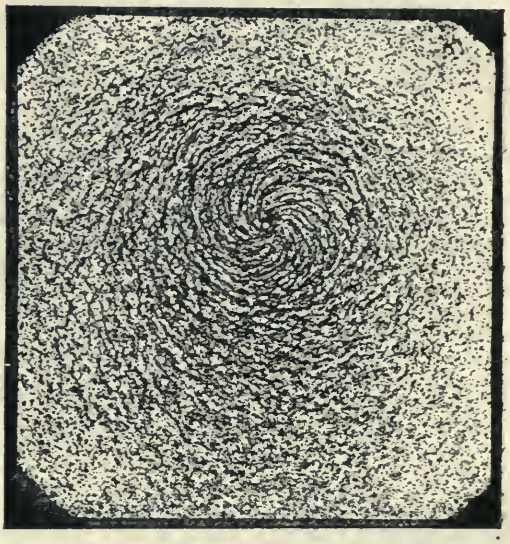


FIG. 11.

study in magnetism. We know that a rod of iron becomes a magnet when we wind a spiral of wire round it and send a current through the wire. There must be some relation between the iron bar and the coils of wire: what is it? Let us investigate this also by looking at the distribution of the lines of force within the coil.

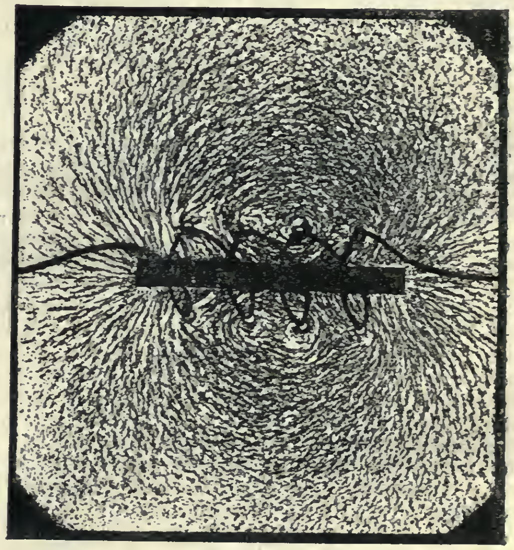


FIG. 12.

Take a plate of glass or a piece of card and bore eight holes through it, as in Fig. 12, and wind a corkscrew of wire in and out ; then lay a little bit of thin, soft iron down the middle. We see by the lines of force, when the current is passed, that the iron becomes a strong magnet, but that the wires at the same time are mag-

netic also. We shall discover also that the magnetism of the little iron bar is not distributed exactly in the same way as if it had been a permanent steel magnet, for the lines of force follow curves that fill surrounding space slightly differently. Still, on the whole, we should argue that the iron core possessed magnetic poles where the force was greatest, and that the two poles were of opposite kinds of polarity, one being a north-seeking, the other a south-seeking, pole.

SILVANUS P. THOMPSON

THE LATE MR. G. DAWSON ROWLEY

IT is with sincere regret that we have to announce the death, on the 21st inst., at his house in Brighton, of Mr. George Dawson Rowley, the projector of, and principal contributor to, the *Ornithological Miscellany*, which he published at his own very considerable cost, and author of several papers on ornithological and archæological subjects. Educated at Eton and Trinity College, Cambridge, where he graduated B.A. in 1846, he was the companion, both at school and at the University, of the late John Wolley, whose early passion for natural history he shared. In Mr. Rowley, however, the taste for a time gave way to antiquarian studies, and did not return, at any rate very strongly, until some years afterwards, when he had married and was settled at Brighton, where, notwithstanding the *dictum* of Mr. Ruskin that "no English gentleman has ever thought of birds except as flying targets or flavorous dishes," he became, so far as the opportunities of the place allowed, a very watchful observer of all that was passing in the feathered world, while in the spring he yearly repaired to his father's estate at St. Neot's in Huntingdonshire, the better to study the habits of birds in the breeding-season. He also began to form a collection of ornithological specimens of singular value, sparing no cost or trouble in the acquisition of objects of rarity or peculiar interest, and the treasures thus amassed finally became very numerous. The design of his *Ornithological Miscellany* seems to have chiefly been to illustrate this "Rarity Chamber"—for so, after the example set by old Rumphius, it might well be called—a considerable number if not most of the specimens therein figured or described being his own possessions. Yet he willingly accorded room in its pages to worthy contributors, among whom may be mentioned Mr. Dresser, Dr. Finsch, Messrs. Salvin, Sclater, Seebohm and Sharpe, and Lord Tweeddale, and his printing a translation of Prjevalsky's important work on the birds of Turkestan, published in Russian, with copies of the plates, was a real boon to those ignorant of that language. Besides this he often wandered into the by-ways of ornithology, which frequently possess a curious kind of interest, and he gave views of many places remarkable for the birds which frequent them. Never did the contents of a work better justify its title, for anything more miscellaneous than they are cannot well be imagined. Failing health, as he himself only a few months ago stated in his concluding remarks, brought it to an end far sooner than he had intended. Setting aside the scientific value of some of the papers, the beautiful plates by which nearly all are illustrated make its cessation a loss to ornithologists; and those who knew that Mr. Rowley had for a long time been gathering information bearing on the history of the extinct Gare-fowl (*Alca impennis*) had hoped that some result of his labours in this respect would one day make its appearance. But this was not to be. More than a year ago a violent hæmorrhage of the lungs gave warning of serious danger, and the attack was only too quickly followed by others of a like nature, under which he sank, in his fifty-seventh year, dying, by a singular coincidence, on the very same day as his father, who had long been an invalid.

NOTES

WE notice with regret the death, at the age of sixty-eight, of Mr. James M'Nab, the well-known curator of the Edinburgh Royal Botanic Garden. Mr. M'Nab's father was also curator of the Edinburgh Botanic Garden, where the son was trained. In 1834 Mr. M'Nab paid a visit to the United States and Canada, the fruits of which appeared in a variety of contributions, descriptive of the more interesting plants found during the journey, in the *Edinburgh Philosophical Journal* for 1835, and in the *Transactions* of that period of the Edinburgh Botanical Society. On the death of his father, in December, 1848, after thirty-eight years' superintendence of the Botanic Garden, Mr. M'Nab was promoted by the Regius Professor (Dr. Balfour) to the responsible post thus vacated. The extent of the Garden at that time was not more than fourteen imperial acres. Ten years later, however, two acres were added on the west side, which were laid out and planted by Mr. M'Nab, under the superintendence of Prof. Balfour. After the lapse of five more years the Experimental Garden, extending to ten acres, was thrown into the Botanic Garden, and planted with conifers and other kinds of evergreens. On a portion of the ground so acquired a Rock Garden was, on the suggestion of Mr. M'Nab, begun towards the end of 1860. The Rockery has now upwards of 5,442 "compartments" for the cultivation of Alpine and dwarf herbaceous plants, and is yearly being added to; while of late years portions of the southern slopes have been set apart for the rearing of bulbous and other plants, whose roots require to be well ripened before flowering. Mr. M'Nab was a frequent contributor to horticultural and other magazines, his writings including papers, not only on botanical subjects, but on vegetable climatology, landscape gardening, and arboriculture. One of the original members of the Edinburgh Botanical Society, founded in 1836, he was a voluminous writer in its *Transactions*; and in 1872 he was elected to the presidency of the society—a position rarely held by a practical gardener. In November of the following year Mr. M'Nab delivered his presidential address on "The Effects of Climate during the last Half-Century with Reference to the Cultivation of Plants in the Royal Botanic Garden of Edinburgh and elsewhere in Scotland," a paper which excited a good deal of discussion at the time, the writer having adduced facts with the view of showing that a change in our climate had taken place during the period in question. Mr. M'Nab also contributed to the Society a monthly report on thermometrical readings and progress of open-air vegetation in the Botanic Garden, which was highly valued, alike by horticulturists and meteorologists, for the practical information it conveyed. Prof. M'Nab, of the Royal College of Science, Dublin, is a son of the late Mr. M'Nab.

ON Friday a meeting of the local executive of the British Association was held at Sheffield to appoint committees to make the necessary preparations for the visit which commences on August 20 next year. The Master Cutler (Mr. W. H. Brittain) presided. It was stated that the guarantee fund now amounted to 3,338*l.*, and would eventually reach 5,000*l.* The Association, however, do not wish the expenses to exceed 1,500*l.*, or they fear that the expense of entertaining the Association will deter other towns from sending invitations. It is expected that at least 1,500 members and associates will attend the sittings. The necessary committees were appointed, and Mr. J. E. H. Gordon, who was present representing the Association, thanked the people of Sheffield for the splendid preparations they were making for the reception of the Association and for the hospitality which was already offered.

M. BARDOUX has appointed a great commission for the reorganisation of the Museum of Natural History of Paris. This

famous institution is almost entirely deserted by students. The report describes all the transformations the Jardin des Plantes has undergone since it was taken out of the hands of the king's physician and entrusted to the care of a special director.

ALTHOUGH we are aware that the columns of NATURE are studied by a considerable number of readers in Australia, New Zealand, and Japan, it is with some pleasure that we find an extract, however incorrect, from one of our articles ("On Cleopatra's Needle and the Wind Pressure"), inserted in the *Bolton Evening News*. Before "A Constant Reader" (we presume of the *Evening News*) took the trouble to multiply out the supposed 80 lbs. to the *square inch*, obtaining the somewhat alarming result of 11,520 lbs. to the square foot for a wind pressure, it might have allayed his fears concerning his own *integrity* and that of the Manchester houses, if he had consulted the original article.

THE Dundee Naturalists' Society have just obtained possession of a very fine specimen of *Pterygotus anglicus*, which was found at Carmyllie Quarries some months ago, and has now been presented to the Society by Messrs. Duncan, Falconer, and Co., the lessees of the quarries. Only fragments of this animal have hitherto been found, and its general appearance has been shown by what are known as restorations; but the correctness of these has been doubted by many palæontologists. This specimen, which is on a slab of the well-known Carmyllie pavement, has from head to tail the segmental plates all intact, with the exception of about an inch of the telson, which is wanting. The length of the fossil is 4 feet 2 inches, and its widest segment 15 inches. The carapace or head plate measures 10 inches by 8, and the telson or terminal plate 8 inches by 7. The abdominal aspect is presented, and the curious arrow-shaped plate, "epistoma," is distinctly shown attached to the middle of the second abdominal segment. In many attempted restorations this plate is figured on the under side of the anterior portion of the carapace.

THE rich collection of insects of Dr. Moritz Isenschmied is bequeathed to the Natural History Museum of Bern, together with a sum of 3,200*l.* for the entomological part of the museum.

A SCIENTIFIC society has been formed at Berlin under the title "Freie Akademie, wissenschaftlicher Centralverein," with the object of propagating scientific knowledge in wider circles by means of lectures. The new society will begin its work in January, 1879. Herr Eberty, the syndic of Berlin, and Dr. Max Hirsch are its directors, and the number of members is rapidly increasing.

AT the instigation of the "Society for the History of the Lake of Constance and its Surroundings," the King of Würtemberg has requested the Statistical and Topographical Office of Stuttgart to undertake a complete investigation of the lake in question. It appears that the various depths of the different parts of the lake have not been measured since 1826. The new investigation will doubtless yield a number of highly interesting data.

AN excellent geological map of Germany has just been published by the Photolithographic Institute of F. Graaz at Leipzig. It is drawn by Prof. Hirschwald, of Berlin, after the relief of C. Raoz, and is specially intended for use at geological lectures.

THE *Times* of yesterday contains an account of Dr. Schliemann's further excavations on the site of Troy, the account being evidently condensed from several letters of the enthusiastic explorer. All Dr. Schliemann's previous conclusions seem to be confirmed, and his already large collections have been greatly added to. Fortunately for his sceptical critics, some of his discoveries have been made in the presence of several British naval officers stationed at Besika Bay. Among other things he has found a steel dagger, the first iron object found on the site, but perhaps the most curious find are "the billions" of shells of cockles and mussels "found in all the strata of the prehistoric *débris*," and said to be no longer found on the shores of the Hellespont and Ægean.

AT some excavations recently made at Heidelberg a Roman well and several milestones were discovered, the inscriptions upon the latter giving interesting details regarding the old Roman colony on the banks of the Neckar.

MR. A. R. WALLACE has reprinted from the *Fortnightly Review* his valuable paper "On Epping Forest and how best to deal with it." It proves how well qualified Mr. Wallace is to have the care of what remains of the once extensive forest.

MR. DE WALL, the *Polytechnic Review* states, has observed that when two electric sparks are simultaneously produced at the extremities of a short tube smoked inside, the two discharges give figures in the form of a black ring at the middle of the tube. When the sparks are not quite simultaneous, the ring is slightly displaced. It is suggested by the author that this observation may afford a method of determining the velocity of sound, and of the speed of propagation of electricity in a conductor.

PROF. PERSIFOR FRAZER reports, we learn from the *Polytechnic Review*, the interesting observation that early in last June he tried a telephone with a diaphragm mounted so as to vibrate freely except in the circular line, where it was bound fast. With several other telephones in circuit, but muffled so that they could not take up the direct vibrations of the voice, he found that the over-tones produced in the diaphragm of one telephone, by a musical note sung into the mouth-piece, were reproduced in the others. This shows the extreme minuteness of the motion necessary to produce sound by fluctuations in the transmitting power of the line wire.

WE are glad to se that the *Princetown Review*, hitherto known as a high-class theological journal, is enlarging its programme so as to include subjects of scientific interest. In the November number, for example, we have excellent papers on "The Rights and Duties of Science," by Principal Dawson; "Man's Place in Nature," by Prof. Le Conte; "Eclipses of the Sun," by Prof. Simon Newcomb; "The Recent Solar Eclipse," by Prof. Young; and "Physiological Metaphysics," by President Ponter. Besides these there are other good papers on a variety of non-theological subjects.

L'Electricité of November 20 contains a first paper by Count du Moncel, on Lacour's Phonic Wheel, and several important papers on Electric Lighting.

THE *Lancet* announces the publication, next week, of a special article, from the pen of Dr. Richardson, on the remains of Harvey. The author has recently visited the church at Hempstead, in Essex, where Harvey lies, and has had photographed all the important historical mementoes, copies of some of which will be reproduced in the *Lancet*. The publication is intended as a further contribution to the literature of the tercentenary year of the birth of the greatest and most original of English anatomists.

AT 6 o'clock on the evening of October 2 a severe earthquake was experienced in the village of Jucuapa and neighbouring towns, in the department of Usulutan, in the southern portion of the Republic of Salvador. Nearly all the houses in Jucuapa were destroyed and many families buried in the ruins, particularly in the outskirts of the town, where the

means of escape were confined to narrow streets, and where the houses were not so solidly constructed as in the centre. The towns included in the disaster are Guadalupe, Nueva Guadalupe, Chinameca, Usulutan, the Caserio del Arenal, Santiago de Maria, which is entirely ruined and some lives lost, a condition in which are also found Tecapa, Triunfo, and San Buenaventura. The shock which produced the greatest damage was at first a kind of oscillatory movement which lasted over forty seconds, and terminated in what felt like a general upheaval of the earth, and was so violent that solid walls and arches and strongly braced roofs were broken and severed like pipe-stems. The movement proceeded from the south-west to the north-east. It was supposed to proceed from the volcano of Tecapa, which is reported as being in conflagration. The district which has been devastated is one of the most thickly settled portions of the country. The *Idea* of Santa Ana reports that apprehensions exist in the public mind that the volcano of Santa Ana is about to be in eruption, from the effects of which serious consequences are feared. The *Panama Star and Herald* is the authority for these statements.

A CORRESPONDENT of the *Pioneer*, writing from Mirzapore, calls attention to a phenomenon which he considers worth recording. Early one morning large quantities of fish of every description were seen coming to the surface along both banks of the river gasping and dying; all the crabs came out and hung in clusters to the clay, or lurked in the grass above the water level, and large eels, leaving the water, lay like snakes along the edge. The next day great numbers of fish, some of enormous size, floated past, and endeavours were made to induce the natives to bring them on shore for manure, but as their fathers had never employed fish for such a purpose, they declined to make such an innovation. The river was high, but not in full flood, and the water, probably on account of the long drought, was intensely and abnormally turbid. The death of the fish is attributed to this peculiarity, for the particles of earth held in suspense appear to have impregnated the gills and stopped breathing; it had not, however, been ascertained which of the affluents of the Ganges or Jumna had caused the mischief.

THE following method of measuring approximately the velocity of sound, devised by M. Bichat (*Journal de Physique*) is said to form a suitable lecture experiment. A white-iron tube, about 10 m. long is bent back on itself, so that the ends A and B are near each other. A is closed with a caoutchouc membrane; B has a stopper with glass tube which communicates, through a caoutchouc tube of a certain length, with one of M. Marey's manometric capsules. Near the end A is an aperture which, by means of another caoutchouc tube the same length as the former, communicates with another of Marey's capsules. These capsules are arranged before a blackened cylinder, the ends of their levers applied to it one over the other. A tuning-fork, giving 100 vibrations per second, inscribes these also in the same vertical line on the cylinder. All being ready a slight blow is given, with the hand or otherwise, to the membrane A, while an assistant turns the cylinder. The capsules register the point of departure and that of arrival, while the tuning-fork gives the time. Thus it is found that, between those two points, there are, *e.g.*, three vibrations of the tuning-fork, *i.e.*, about $\frac{3}{100}$ of a second have elapsed. The velocity of sound is inferred to be 333·3 m. per second. By means of two iron tubes placed one above the other the difference of the velocities of sound in air and hydrogen may be demonstrated, even though it may be difficult to keep the one tube filled with pure hydrogen.

AN historico-ethnographical exhibition has been opened at Winterthur (Switzerland), and the visitors are agreeably surprised by the large number and great variety of objects exhibited as well as by their artistic arrangement.

JAPANESE farmers appear to be determined not to be left behind by their fellow-countrymen in matters of progress, for we hear that in some parts they are growing wheat from foreign seed. On this subject a Nagasaki paper says:—"We have seen a magnificent specimen of wheat grown in Japan from American seed, than which nothing better could be desired, the flour produced from it being fully equal to any we have seen from America. Such a result speaks well for the prospects of Japan becoming, with proper cultivation, a profitable wheat-producing country."

A GERMAN engineer residing at Smyrna, Herr Karl Humann, who some time ago had undertaken some successful excavations at Berghama at his own expense (the ancient Pergamum in Asia Minor), has recently been commissioned by the German Government to continue these excavations, and has succeeded in bringing to light some highly interesting objects of art. The Porte has permitted the continuation of the excavations under the condition that only half the objects found shall become German property, while the other half will be retained at Constantinople.

IN the *Annual Report* of the Royal Botanic Garden, Calcutta, for the year 1877-78, Dr. King, the superintendent, draws attention to the want of proper accommodation for the herbarium, which now consists of ninety-three cabinets of dried plants, forming, as we are told, "the only large herbarium in India, containing authentic specimens from almost every Indian botanist from Heyne to Kurz, including excellent sets of Wallich's, and Hooker and Thomson's plants." Dr. King reports that of the seedlings of the Para rubber plant (*Hevea brasiliensis*) received at the beginning of last year, some were retained at Calcutta, while the others were sent to the Cinchona plantation in Sikkim. Several of the plants have died during the year, but those remaining at Calcutta are healthy, and have grown fairly well. Of the Ceara rubber plant (*Manihot glaziovi*), many of them were found to be dead on arrival; those that survived were divided between Calcutta and the Cinchona plantation, one of the plants is said to be ten feet high, and the others vary in height from two to five feet, but they all appear weak and lanky, as if the climate were too damp for them. Regarding the cultivation of vanilla in India, especially in the climate of Calcutta, Dr. King's experience confirms his first impression that "it is not worth while to go to any further expense in attempting to make a plantation of it, to be conducted on commercial principles." It is satisfactory to learn that ipecacuanha has been propagated largely; like vanilla, Dr. King is of opinion that it can never be grown successfully as a crop in any part of Bengal. The utilisation of new vegetable substances for paper-making, especially baobab bark and bamboo shoots, are fully considered by Dr. King, who expresses an opinion with regard to the former that, if the plant is "to be grown to a profit, it would be hardly feasible to give it cultivation, however rough, after the first year." Considering also the comparatively slow growth of the baobab, Dr. King says he is driven to the conclusion that it is not likely to afford in India a sufficiently cheap paper-fibre. He points out that a plant yielding an annual crop is much more likely to fulfil the financial conditions of success than any perennial like the baobab, which yields a crop only after many years. Regarding the use of the young shoots of bamboo for paper stock, which have been very favourably reported on by Mr. Routledge, Dr. King does not look upon it in any hopeful light. Of the so-called "Rain Tree," which has already been noticed in our columns, and referred to *Pithecolobium saman*, a number of good trees are growing in the Botanic Garden, Calcutta. One set consists of five trees, about eleven years old, and the other of eighty-four trees, planted in an avenue about four years ago. The tree is a very fast grower, and is said to be perfectly at home in the soil and climate of Lower Bengal. From its

umbrageous habit and wide-spreading branches it is extremely valuable as a shade tree. The wood is soft and of little value except as firewood, and the pod is sweet, like that of the carob (*Ceratonia siliqua*), and may probably prove valuable as a food for cattle, for which purpose, indeed, these pods are used in the West Indies. For this reason, and not for that of gathering and dispersing moisture (for which the tree became momentarily celebrated), it is probable the tree may be generally planted.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. Theodore Beck; a Black-crested Cardinal (*Gubernatrix cristatella*), two Red-crested Cardinals (*Paroaria cucullata*) from South America, purchased; a Macaque Monkey (*Macacus cynomolgus*) from India, deposited; a Baker's Antelope (*Hippotragus bakeri*) from Nubia, received in exchange.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE proposals of the Cambridge Mathematical Studies Syndicate for completing the new scheme for the mathematical tripos have been carried. The following summary of the whole scheme of the mathematical tripos which will come into operation in the year 1882 is given in the *Times*. It will consist of three parts, the examination for each part occupying three days. The subjects of the first part are to be confined to the more elementary parts of pure mathematics and natural philosophy, the subjects to be treated without the use of the differential calculus and the methods of analytical geometry. The examination in Part II. will only be open to those who have passed Part I. so as to deserve mathematical honours, and the subjects are algebra, trigonometry, plane and spherical, theory of equations, the easier parts of analytical geometry, plane and solid, including curvature of curves and surfaces, differential and integral calculus, easier parts of differential equations, statics, including elementary propositions on attractions and potentials; hydrostatics, dynamics of a particle, easier parts of rigid dynamics, easier parts of optics and spherical astronomy. Those who pass this second part will be arranged as wranglers, senior optimes, and junior optimes in order of merit. Both the examinations in Parts I. and II. will take place in June. The examination in Part III. will be held in January, and be open only to those who are classed as wranglers. It will last three days. On the tenth day after the end of the examination in Part III. the moderators and examiners, taking into account the examination in that part only, shall publish in three divisions, each division arranged alphabetically, those examined and approved. The moderators and examiners may place in the first division any candidate who has shown eminent proficiency in any one group of the subjects in Schedule III.

THE *University College of Wales Magazine*, the first number of which lies before us, is a neat little publication of fifty-two pages, doing credit to the Oswestry press from which it issues, as well as to the enterprise of the Aberystwith Institution, and the ability of its members. We do not suppose its promoters expect a large general circulation, though there is no reason why the magazine might not be so conducted as to meet with considerable favour in the principality. Curiously enough, the first paper after the introduction is on Persian literature, while one on Welsh literature occupies the sixth place. There is a paper on Cambria at Paris, showing what a good appearance she made at the recent exhibition; a Welsh story, an Oxford letter, college news, &c. We wish the magazine success; and it might do good service by devoting itself to research in various directions in regard to Wales. We should like to see the science professors in this college fill up some of its pages.

THE First Annual Report of the Dulwich College Science Society is, on the whole, satisfactory; the appended lists, forming the bulk of the volume, show that the Society has several diligent collectors, and we hope it will continue to do genuine work and nourish in the scho l a lasting love of real science.

PROF. H. G. SEELEY completed on Friday at the College for Men and Women, 29, Queen Square, Bloomsbury, a course of six lectures on some of the principal forms of extinct animals which resemble reptiles and birds, and have no representatives now living. The subjects have been as follows:—Lecture I.—On the Geological Distribution of Fossil Reptiles and Birds; and concerning points in which Extinct Reptiles differ from those which now inhabit the Earth. Lecture II.—The Ichthyosaurians and Animals of the Open Ocean. Lecture III.—The Plesiosaurians and Animals of the Sea Shore. Lecture IV.—The Dinosaurians and Allied Types of Land Animals. Lecture V.—The Ornithosaurians and other Flying Types of Life. Lecture VI.—The Classification of Reptiles and Allied Fossil Animals, as illustrating some Aspects of the Doctrine of Evolution.

PROF. WURTZ was charged some time since by the French Minister of Public Instruction, to make an inquiry into the organisation of the laboratories and practical instruction given in the several universities of Germany and Austro-Hungary. Prof. Wurtz accordingly made several journeys to the great seats of learning in these two countries, and the *Journal Officiel* of last Saturday publishes at full length his report. Prof. Wurtz insists strongly on the danger of creating large establishments, where students are taught something of everything, and on the necessity of creating special foci for every large section of experimental science. He shows the advantage of special institutes, and insists upon the organisation of chemical, physical, physiological, anatomical, and pathological institutions such as flourish on the other side of the Rhine, and may be established in Alsace-Lorraine. He ends his report by describing the Munich Hygienic Institute.

THE French budget of Public Instruction has been voted *au pas acceleré*. The resolutions proposed by the Commission were voted without any material alterations. The estimates reach about 2,000,000*l*.

The University of Bern celebrated, on November 15, the forty-fourth anniversary of its foundation. It numbers among its students, about twenty ladies, mostly Russians, who study medicine.

ACCORDING to a new law, all children who finish their education in any school of the Canton of Bern are submitted to an examination. This year 4,610 boys and 4,446 girls were examined (total population of the Canton 537,000), and the results proved unsatisfactory. The Canton continues to occupy the eighteenth and twenty-first places in the Cantons of the Swiss confederation.

A WEALTHY Serbian, Ilija Milosavljewitch Kolaraz, who died a month ago at the ripe age of eighty-two, has left the sum of 100,000 ducats for educational purposes, 10,000 ducats for the publishing of valuable works in the Serbian language, and 60,000 ducats for the foundation of a Serbian university at Belgrade, which is to be known as Kolaraz' University.

SCIENTIFIC SERIALS

Journal of Anatomy and Physiology, July, 1878.—Dr. Ogston, of Aberdeen, gives an account of the growth and maintenance of the articular ends of adult bones. He believes that the articular cartilage produces the osseous tissue beneath it, forms the epiphyses, supplies their waste, and maintains them in their proper size and bulk during adult life.—Prof. Cleland describes the brain in cyclopians or one-eyed monsters, including specimens of human kind, dogs, lambs, and pigs. He finds that there is no trace of a retina in the cyclopian eyeball, and that moreover there is an arrest of the development of the first cerebral vesicle.—Dr. Creighton gives an exhaustive account of the formation of the placenta in the guinea-pig, and refers very prominently to its early development in connection with the structure of the ovaries and supra-renal bodies.—Prof. Turner contributes notes on the fœtal membranes of the reindeer, and on the oviducts of the Greenland shark.—Mr. David Newman's paper on the functions of the kidney gives an account of the physical influences which promote secretion, so far as can be demonstrated by experiments with animal membranes and the kidneys of animals recently killed.—Dr. Dodds' historical and critical analysis of our knowledge upon the localisation of the functions of the brain deals with the anatomy of the brain in this number.

October.—Dr. Cunningham, of Edinburgh, gives his deductions on the intrinsic muscles of the mammalian foot, derived from a large number of dissections; and further describes the muscles of the foot of cuscus and thylacine.—Prof. Miall and Mr. Greenwood conclude their valuable memoir on the anatomy of the Indian elephant, dealing with the alimentary canal and

its appendages, and the other abdominal and thoracic viscera.—Dr. Creighton publishes his observations on the supra-renal bodies based on microscopical investigations of these organs when adult and during development, and shows how they present many features of analogy to the ovaries.—Prof. Humphry gives his reasons for dissent from Dr. Ogston's views on the important share taken by articular cartilage in the growth of bone, as expressed in the July number of the *Journal*.—Prof. Turner describes the placentation of the hog-deer (*Cervus porcinus*).—Dr. Urban Pritchard supplements his previous accounts of the development of the organ of Corti in the internal ear.—Dr. T. B. Henderson, of Glasgow, describes the physiological effects of the inhalation of phosphuretted hydrogen.

Journal de Physique, October.—In this number Prof. Dufet studies the variation of the indices of refraction in mixtures of isomorphous salts, arriving at the experimental law that the differences between the indices of a mixture of two such salts and those of the component salts are in inverse ratio of the numbers of equivalents of the two salts forming the mixture; in other terms the curve which has for ordinates the indices and for abscissæ the equivalents, is a straight line. This law is regarded as a consequence of Gladstone's, on the constancy of specific *refractive energy* in mixtures.—M. Terquem describes an improved way of realising Plateau's liquid laminar systems, giving larger systems with less liquid. Instead of using pieces all rigid, he uses a combination of rigid pieces with flexible threads (silk), *e.g.*, two horizontal rods joined together at the ends with such threads or two rings joined with threads. Many instructive effects are thus had. The liquid used is a solution of soap and sugar, prepared in a special way.—M. Bouty contributes a mathematical paper on the number of elements necessary for determining the exterior effect of an optical system, and M. Bichat gives a new method of measuring the velocity of sound.

SOCIETIES AND ACADEMIES

London

Royal Society, November 21.—"On Repulsion resulting from Radiation." Part VI., by William Crookes, F.R.S., V.P.C.S.

In this part, with which the research closes, the author first examines the action of thin mica screens fixed on the fly of an ordinary radiometer, in modifying the movements. It is found that when a disk of thin clear mica is attached 1 millim. in front of the blacked side of the vanes of an ordinary radiometer, the fly moves negatively, the black side approaching instead of retreating from the light. When a thin mica disk is fixed on each side of the vanes of a radiometer, the result is an almost total loss of sensitiveness.

In order to examine the action of screens still further an instrument is described having the screens movable, and working on a pivot independent of the one carrying the fly, so that the screens can move freely and come close either to the black or to the white surfaces of the disks. By gentle tapping the screens can be brought within 2 millims. of the black surfaces. A candle is now brought near, shaded so that the light has to pass through one of the clear disks and fall on the black surface. The black side immediately retreats, the clear disk remaining stationary for a moment, and then approaching the light. If the candle is allowed to shine on the plain side of the black disk, no immediate movement takes place. Very soon, however, both disks move in the same direction away from the candle, the speed of the clear disk gradually increasing over that of the blacked disk.

Instead of allowing the clear screens to freely move on a pivot, an instrument was made in which the screens could be fixed beforehand in any desired position in respect to the blacked disk. It was then found that with the screens close to the blacked sides of the vanes the fly rotates very slowly in the negative direction, stopping altogether when the candle is moved five or six inches off. With the screens 1 millim. from the black surface the direction is negative, and the speed at its maximum. When the screens and disks are 7 millims. apart a position of neutrality is attained, no movement taking place. When the distance is further increased, positive rotation commences, which gets stronger as the screens approach the bright sides of the disks, where the positive rotation is at its maximum. The author adduces reasons for considering that the negative rotations here observed are caused by the warming up of the black surface by radiation falling direct on it, through the clear mica screen, and the deflection backwards of the lines of molecular pressure thereby generated.

The action of these radiometers being complicated, owing to the surfaces of the vanes being different in absorptive power, another instrument was made in which the vanes were of polished aluminium, perfectly flat and symmetrical with the bulb. The screens were of clear mica movable in respect to the vanes, and at right angles to their surface. When exposed to the light of a candle it was found that with the screens brought up close to the disks, the rotation was as if the unscreened side were repelled; at an intermediate position there was neutrality. Explanations are given of these movements, but without the illustrative cuts they would be unintelligible.

Experiments on radiometers having movable screens interposed between the vanes and the bulb are next given, and these are followed by a long series of experiments on the influence of movable screens on radiometers with cup-shaped metallic vanes, the screens being varied in shape and position in respect to the plane of rotation, as well as in respect to the distance from the vanes.

A similar series is given with metallic cylinders as vanes, and from the behaviour of the latter kind of radiometer an explanation is given of the various movements previously obtained. It is found that when the screen touches the convex surface of the vanes the rotation under the influence of light is always positive. It commences at a low exhaustion, increases in speed till the rarefaction is so high that an ordinary radiometer would begin to lose sensitiveness, and afterwards remains at about the same speed up to the highest rarefaction yet obtained. At any rarefaction after 87 M (millionths of an atmosphere) there is a neutral position for the screen. When it is on the concave side of this neutral position the direction of rotation is positive, and when on the convex side of the neutral position it is negative; the speed of rotation is greater as the vanes are further removed from this neutral position on either side. The position of this neutral point varies with the degree of exhaustion; thus at 12 M the screens must be 3 millims. from the convex side; at ·18 M they must be 13 millims. from the convex side. The higher the exhaustion the greater the distance which must separate the convex side of the hemi-cylinders and the screens.

The author gives explanations of these phenomena based on the following already ascertained facts:—When thin aluminium vanes are exposed to light the metal rises in temperature and becomes equally warm throughout, and a layer of molecular pressure is generated on its surface. The thickness of this layer of pressure, or the length of the lines of force of repulsion varies with the degree of exhaustion, being longer as the exhaustion increases. The lines of force appear to radiate from the metal in a direction normal to its surface. The force of repulsion is also greater the closer the repelled body is to the generating or driving surface, and the force diminishes rapidly as the distance increases, according to a law which does not appear to be that of "inverse squares." Diagrams are given illustrating the author's explanation based on the above data.

An apparatus is next described not differing in principle from the last, but having, in addition to the aluminium hemi-cylinder and movable mica screen, a small rotating fly made of clear mica, mounted in such a way that it could be fixed by means of an exterior magnet in any desired position inside the bulb. The screen was also capable of adjustment by means of another magnet; the aluminium hemi-cylinder in this apparatus being fixed immovably. The adjustible indicator being very small in diameter in comparison to the other parts of the apparatus, and, being easily placed in any part of the bulb, was expected to afford information as to the intensity and direction of the lines of pressure when a candle was brought near the bulb. Experiments have been tried, *a*, with the screen in different positions in respect to the hemi-cylinder; *b*, with the indicator in different parts of the bulb; *c*, with the candle at different distances from the hemi-cylinder on one side or the other; *d*, with the degree of exhaustion varying between wide limits. It would be impossible to give an intelligible abstract of the results obtained with this apparatus without numerous diagrams It may, however, be briefly stated that they entirely corroborate the theories formed from a study of the behaviour of the instruments previously described.

The next part of the paper treats of the action of heat employed inside the radiometer. In a previous paper the author

showed that phenomena feeble and contradictory when caused by radiation external to the bulb, became vigorous and uniform when the radiation was applied internally by the agency of an electrically-heated wire. It was hoped that some of the more obscure phenomena shown by the deep cups with movable screens in front (referred to above) might be intensified if set in action by a hot wire. Several inds of apparatus and experiments with them are described, but the results are too complicated to be given in abstract. One experiment proves that the direction of pressure is not wholly normal to the surface on which it is generated, but that some of it is tangential.

The author then describes the turbine radiometer, early specimens of which were exhibited before the Royal Society on April 5, 1876. In the ordinary form of radiometer the number of disks constituting the fly is limited to six or eight, a greater number causing interference one with the other and obstruction of the incident light. In the turbine form of fly there is no such difficulty, the number of vanes may be considerably increased without overcrowding, and with corresponding advantage. In the earlier turbine radiometers the flies were made of mica blacked on both sides, and inclined at an angle like the sails of a windmill, instead of being in a vertical plane. This form of instrument is not sensitive to horizontal radiation, but moves readily in one or other direction to a candle held above or below. A vertical light falling on the fly gives the strongest action, but rotation takes place, whatever be the incident angle, provided the light is caught by one surface more than by the other. Ether dropped on the top of the bulb to chill it causes rapid negative rotation. If the turbine radiometer is floated in a vessel of ice-cold water, and the upper portion exposed to the air of a warm room, it rotates rapidly in the positive direction, acting as a heat engine, and continuing so to act until the rotating fly has equalised the temperature of the upper and lower portions of the bulb. By reversing the circle of operations—by floating the turbine radiometer in hot water and cooling the upper portion of the bulb—the fly instantly rotates in the negative direction.

After describing experiments in which the same fly was made to rotate first in a large bulb and then in a small one at the same degree of exhaustion, the author proceeds to discuss the influence exerted by the inner side of the glass case of the radiometer as a reacting surface. A flat metal band was put equatorially inside a radiometer, and lamp-blacked, so that the molecular pressure generated under the influence of light should react between the fly and the black band, instead of between the fly and the glass side of the bulb. It was found that the maximum speed with the band present was 40 revolutions a minute, against 8¼ revolutions when the band was absent.

The rotation of the case of a radiometer, the fly being held immovable by magnetism, is next described. A preliminary note on this subject having already appeared in the *Proceedings*,[1] it need not be again described in detail. Many different forms of instrument for effecting this rotation are described, and their mode of action explained.

The reacting inner surface of the envelope being thus proved to be essential to the rotation of the fly, other instruments were made in which this necessary reaction is obtained in a more direct manner. In one, the radiometer is furnished with a fly carrying four flat aluminium vanes, polished on both sides. Three vertical partitions of thin clear mica are fixed in the bulb, with their planes not passing through the axis of rotation, but inclined to it, thus throwing the obliquity off the fly on to the case, and giving three fixed planes for the reaction to take place against. Candles arranged symmetrically round the bulb make the fly rotate rapidly against the edges of the inclined planes. Breathing gently on the bulb gives negative rotation. A hot glass shade inverted over the instrument causes strong negative rotation, changing to positive on cooling. When the fly is furnished with clear mica or with silver flake mica vanes, the same results are obtained as when aluminium vanes are employed. The principal action is produced by dark heat warming the bulb, screens, and vanes.

The *otheoscope* is the next subject treated on in the paper. This has already been given in abstract,[2] and need not be again referred to. Many different varieties of otheoscope are figured and described.

It was suggested by Prof. Stokes that a disk might be made to revolve on its axis, and the author describes an instrument in which this suggestion is carried out. The disk is horizontal, mounted like the fly of a radiometer, and for lightness' sake is of mica, blacked above. Fixed to the bulb above the disk are four flat pieces of clear mica; each extends from the side of the bulb to near the centre, and ends below in a straight horizontal edge, leaving just space enough for the disk to revolve without risk of scraping. The edge is in a radial direction, and the plane of the plates is inclined about 45° to the horizon in the same direction for them all. Exposed to the light of a candle the rotation is against the edge. By slightly modifying this form the instrument becomes much more sensitive.

[1] *Proc.* Roy. Soc., No. 168, March 30, 1876.
[2] *Proc.* Roy. Soc., No. 180, April 26, 1877.

Whilst experimenting with the otheoscope it was found that, for a given exhaustion, the nearer the reacting surfaces were together the greater was the speed obtained. In the *Proceedings* of the Royal Society for November, 1876,[1] the author described an apparatus by which he was able to measure the thickness of the layer of molecular pressure generated when radiation impinged on a blackened surface inclosed in an atmosphere the rarefaction of which could be varied at will.

It was found that in this apparatus repulsion could be obtained at ordinary atmospheric pressures. Observations are given at normal pressure and at various degrees of rarefaction, with the driving and moving surfaces separated 1, 2, 3, 4, 6, 8, and 12 millims.; and diagrams of the resulting curves are shown when the atmospheric tension and the force of repulsion are used as abscissæ and ordinates. The tables and curves show that the law of increase of the force with the diminution of the distance between the disks does not remain uniform at all rarefactions. At the lowest exhaustions the mean path of the molecules of the attenuated gas is less than 1 millim., as rendered evident by the force of repulsion diminishing rapidly as the distance increases. At exhaustions higher than 9 millims. this condition alters, and as the gauge approaches barometric height the molecular pressure tends to become uniform through considerable distances, the mean path of the molecules now being comparable with the greatest distance separating the surfaces between which they act.

A similar apparatus to the one in which the last experiments were tried was used to measure the action at pressures at and approaching atmospheric. At pressures between atmospheric and 210 millims. the first action is very faint repulsion, immediately followed by strong attraction. The attraction then begins to decline, until, at 15 millims. pressure, it disappears. At the same time the repulsion, which begins to be apparent at 250 millims., increases as the attraction diminishes. The author considers that the attraction is the result of air-currents, caused by the permanent heating of the surface in front of the movable disk.

The paper concludes with experiments undertaken to measure the amount of repulsion, using a horizontal torsion balance,[2] on the principle of Ritchie's, in which the force of repulsion is balanced by the torsion of a fine glass fibre. The *pan* of the balance is a clear mica disk, and a similar disk is fastened to the tube in which the beam oscillates. This fixed disk is lamp-blacked on the upper side, and beneath is a spiral of platinum wire, connected with terminals sealed through the side of the tube. When the spiral is ignited by a constant electric current the blacked mica disk fixed above it becomes heated, and the molecular pressure thereby generated between it and the mica pan causes the latter to rise. The glass thread attached to the beam is thus twisted, and by means of a graduated circle the number of degrees through which the thread has to be turned in order to bring the beam back to equilibrium is noted. This gives a measurement of the pressure exerted, in torsional degrees, and these are converted into grains by ascertaining how many torsional degrees correspond to a known weight. A ray of light reflected from a mirror in the centre of the beam is used as an index, being brought back to zero at each experiment. The author gives in a table, and also shows in the form of a curve, the results obtained with this apparatus, giving the force of molecular pressure in grains weight at exhaustions varying between 2,237 and 0·7 millionths of an atmosphere.

Mathematical Society, November 14.—Lord Rayleigh, F.R.S., in the chair.—The Treasurer's and Secretaries' reports having been read and adopted, Prof. W. G. Adams, F.R.S., consented to act as auditor.—The scrutators declared the following gentlemen elected as the Council for the ensuing session, viz., Mr. C. W. Merrifield, F.R.S., President; Prof. Cayley,

[1] *Proc.* Roy. Soc., No. 175, vol. xxv. p. 310.
[2] For a description of this form of torsion balance, see the author's paper, *Phil. Trans.*, 1876, vol. clxvi. pt. 2, p. 371.

F.R.S., and Lord Rayleigh, F.R.S., Vice-Presidents; Mr. S. Roberts, F.R.S., Treasurer; Messrs. M. Jenkins and R. Tucker, Hon. Secretaries. Other Members: Mr. J. W. L. Glaisher, F.R.S., Mr. H. Hart, Dr. Henrici, F.R.S., Dr. Hirst, F.R.S., Dr. Hopkinson, F.R.S., Mr. A. B. Kempe, Dr. Spottiswoode, F.R.S., Prof. H. J. S. Smith, F.R.S., Mr. H. M. Taylor, and Mr. J. J. Walker.—Mr. Merrifield having taken the chair, Mr. J. D. H. Dickson was elected a Member, and Prof. W. S. Jevons, F.R.S., was proposed for election. The Rev. A. Freeman and Prof. Reinold were admitted into the Society.—The Chairman read a letter from Mr. Warren de la Rue, F.R.S., respecting a memorial to M. Leverrier.—The following communications were made to the Society:—On the instability of jets, by Lord Rayleigh.—On self-strained frames of six joints, by Prof. Crofton, F.R.S. (read by Mr. Hart).—On equivalent statements, iii., by Mr. H. McColl (abstract, read by Mr. Tucker).—The last paper contained a solution of a test problem to show the power of the author's method of elimination; then, an explanation, with illustrations and applications, of another allied method, called the "method of unit and zero substitution;" thirdly, a brief indication of the way in which this algebra of logic may render important service to scientific men in investigating the causes of natural phenomena; and lastly, a brief criticism of Prof. Jevons's method of solving logical problems.

Linnean Society, November 21.—Dr. Gwyn Jeffreys, F.R.S., vice-president, in the chair.—Dr. W. P. Kesteven exhibited, and a short note was read on some specimens of the so-called *Tête anglais* (*Melocactus communis*) from Vieuxfort, St. Lucia. There also was exhibited roots, tendrils, and tubers in different stages of *Vitis gongylodes* and *V. cuspidata*, illustrating the paper immediately thereafter read, viz.: On branch tubers and tendrils of *Vitis gongylodes*, by Mr. R. Irwin Lynch. Subterraneous tubers are by no means rare among plants, *e.g.*, the potato, but in contrast those of *V. gongylodes* present on the stem are aerial, at a height, and on dropping to the ground strike root. Cylindrical, of considerable size, and tenacious of life, they doubtless are a safeguard in propagation of the plant under circumstances prejudicial to the seed. The tendril possesses terminal adhesive disks, and these are formed without the stimulus of contact with any substance, therefore opposed to certain other climbers mentioned by Mr. Chas. Darwin. The aerial roots are of great length, eleven feet and more, they spring from each node, and are of a rich crimson colour in summer, so that they are attractive objects as seen in the Victoria House at Kew.—Report on the Mollusca of the *Challenger* expedition, by the Rev. R. Boog Watson. After introductory remarks, the author describes three genera of the Solenoconchia. Of these *Dentalium* has eighteen species, eleven being new. *Siphodentalium* has seven species, all new to science. Of *Cadulus* two only are already known, nine species and one variety being now recorded for the first time. In all, thirty-six species and four varieties, whereof twenty species were hitherto unknown. Some are of high interest, inasmuch as being remnants of genera now living which have existed since the cretaceous epoch.—The Secretary read the abstract of a paper by Mr. John Miers, on the Symplocaceæ. The author gives credit to Mr. Bentham for the earliest accurate knowledge of the group. The authors of the "Genera Plantarum" recently adopted the example of Prof. A. De Candolle, who regarded the Symplocaceæ as a mere tribe of the Styraceæ. This appears objectionable to Mr. Miers, who, with historical remarks, &c., gives grounds for his adverse opinion. Then follows a synopsis of, to him, eleven recognisable genera, with diagnoses of same, and lists of 125 species.—On the Algæ of Lake Nyassa, by Prof. Dickie, a brief communication, wherein he mentions being indebted to Dr. Laws, of the Livingstonia mission, for the collection. All the genera of the Algæ are known European forms, while the Diatomaceæ, with few exceptions, are likewise widely-diffused species, the only peculiar form being *Epithemia clavata*.—Messrs. Thos. Davidson, F.R.S., and Fred. Jas. Faraday, were elected Fellows of the Society.

Chemical Society, November 21.—R. Warington in the chair.—The following papers were read:—A chemical study of vegetable albinism, by Prof. Church. The author has made numerous analyses of white and green leaves of the same age from the same plant, in order to discover whether any difference in their composition could be detected. The leaves were gathered from the maple, the holly, the ivy, and three exotic plants. White leaves contain more water than corresponding green leaves, whilst the ash of white leaves contains more potash and phosphoric acid, but less lime, especially less oxalate and carbonate of calcium. Nearly sixty per cent. of nitrogen in the white leaves is non-albumenoid, while the green leaves contain thirty per cent. of nitrogen in that state. The author has also analysed a vegetable parasite, the dodder, and its host, the red clover; he finds that the white leaves resemble in composition the parasite, while the host represents the green leaves. The white leaf is therefore, in a sense, a parasite on the green leaf, and owes its existence to its connection with the normal portion of the plant.—Relation between the melting-points of the elements and their coefficients of expansion, by Dr. Carnelly. The author finds that, of thirty-one elements, twenty-six show that the coefficient of expansion increases as the melting-point diminishes; the five exceptions are arsenic, antimony, bismuth, tellurium, and tin.—A preliminary notice on a hydride of boron, by F. Jones. The author succeeded in preparing a grey friable mass of magnesium boride by strongly heating a mixture of magnesium dust and boron trioxide. On heating this mass with hydrochloric acid, a colourless gas was evolved, spontaneously inflammable, burning with a green flame, and of disagreeable odour.

Zoological Society, November 19.—Mr. A. Grote, vice-president, in the chair.—Mr. Sclater exhibited and made remarks on an adult specimen, in full plumage, of the black-throated stonechat (*Saxicola stapazina*), which had been obtained in Lancashire, and had been sent for exhibition by Mr. R. Davenport. The species had not been previously recorded as occurring in the British Isles, and was an interesting addition to the list of "Accidental Visitors."—The Secretary read two letters he had received from Dr. A. B. Meyer and Mr. A. D. Bartlett in reference to the communication read at the last meeting from Mr. Everett respecting the supposed existence of the anoa (*Anoa depressicornis*) in the Philippines.—Prof. Owen, C.B., read a memoir on the relative positions to their constructors of the chambered shells of cephalopods.—Sir Victor Brooke, Bart., read a paper on the classification of the Cervidæ, and gave a synoptical list of the existing species of this family.—A second paper by Sir V. Brooke contained the description of a new species of gazelle from Eastern Africa, which the author proposed to name *Gazella walleri*, after its discoverer, Mr. Gerald Waller.—Prof. A. H. Garrod, F.R.S., read a paper on the anatomy of *Indicator major*, and showed that, as regards its soft parts, as in its osteology, *Indicator* is not related to the cuckoos, but to the barbets and toucans.—A communication was read from the Marquis of Tweeddale, F.R.S., containing the eleventh of his contributions to the ornithology of the Philippines. The present paper gave an account of the collection made by Mr. A. H. Everett at Zamboanga, in the Island of Mindando. Ninety-eight species were obtained in this locality by Mr. Everett, of which eleven were new to the Philippine fauna and six were new to science.—Mr. E. R. Alston read some notes supplementary to his paper on the squirrels of the neotropical region.

Entomological Society, November 6.—Mr. H. W. Bates, F.L.S., F.Z.S., president, in the chair.—Mr. Waterhouse exhibited a specimen of *Chauliognathus excellens* (*Telephoridæ*), a new beetle from New Granada.—Mr. H. T. Stainton exhibited a new horn-feeding *Tinea* (*T. orientalis*), which had been reared by Mr. Simmons, of Poplar.—The Rev. H. L. Gorham exhibited some rare British beetles, taken in the neighbourhood of Horsham, Sussex.—Mr. Goss exhibited specimens of a rare dragon fly (*Cordulia curtisi*) from Christchurch, Hampshire.—Mr. Meldola exhibited a male specimen of the moth *Erebus odorus*, from Jamaica, possessing large tufts or brushes on the hind leg, which were considered as probably scent-secreting organs.—Prof. Wood Mason exhibited drawings and made remarks on the flower simulating *Mantidæ*.—Mrs. Randolph Clay exhibited a living specimen of a beetle (*Zopherus bremëi*), from Yucatan, worn as an ornament.—Sir Sydney Saunders exhibited specimens of *Blastophaga psenes* (Linn.), employed in the process of cuprification, received from Mr. J. Lichtenstein, of Montpellier.—Also specimens of *Sycophaga crassipes*, West., from the sycamore figs of Egypt, together with certain apterous associates.—The Secretary read a communication from the Board of Trade with reference to the damage done to the corn crops in the neighbourhood of Mariapol by swarms of the beetle *Anisoplia Austriaca*.—A sub-committee was appointed to draw up a report on the same.—Miss E. A. Ormerod read a paper on *Psila rosæ*, the well-known insect producing the so-called "rust" in carrot crops.

She advocated the use of a phenol preparation for the destruction of this pest.—Mr. C. O. Waterhouse read a paper containing descriptions of new *Telephoridæ* from Central and South America.—Sir Sydney Saunders communicated a paper on the habits and affinities of *Sycophaga* and *Apocrypta* from the sycamore figs of Egypt.—Mr. Distant communicated descriptions of new species of Hemiptera-Homoptera.

Geological Society, November 6.—Henry Clifton Sorby, F.R.S., president, in the chair.—Arthur Goodger, Rev. Walter Howchin, Lieut.-Col. C. A. McMahon, Oswald Milton Prouse, and M. G. Stuart, were elected Fellows of the Society.—The following communications were read:—On the range of the mammoth in space and time, by Prof. W. Boyd Dawkins, F.R.S. The author expressed his opinion that the result of the evidence collected since the death of Dr. Falconer has been to establish the view of that palæontologist as to the mammoth having appeared in Britain before the glacial epoch. The evidence as to the occurrence of the mammoth in the south of England was first examined. The remains found beneath the bed of erratics near Pagham belonged, not to *Elephas primigenius*, but to *E. antiquus*. But in 1858 remains belonging to the former were found by Prof. Prestwich under boulder-clay in Hertfordshire. In Scotland remains of *E. primigenius* have been found under boulder-clay, but whether under the oldest boulder-clay is uncertain. In 1878 a portion of a molar was brought up from a depth of sixty-five feet near Northwich. It was in a sand beneath boulder-clay, which the author considered to be undoubtedly the older boulder-clay. The author now assents to Dr. Falconer's opinion (which he formerly doubted) that *E. primigenius* was a member of the Cromer forest-bed fauna. It is also clear that it was living in the southern and central parts of England in post-glacial times. It has not been found north of Yorkshire on the east and Holyhead on the west, probably because Scotland and north-west England were long occupied by glaciers. Its remains have been found on the continent as far south as Naples and as far north as Hamburg, but not in Scandinavia. Its remains, as is well known, abound in Siberia, and it ranged over North America from Eschscholtz Bay to the Isthmus of Darien, *E. columbi*, *E. americanus*, and *E. Jacksoni* being only varieties. The author then discussed the relations of *E. primigenius* to *E. columbi*, *E. armeniacus*, and *E. indicus*, and came to the conclusion that it is the ancestor of the last.—The mammoth in Siberia, by H. H. Howorth, F.S.A. Communicated by J. Evans, LL.D., F.R.S. The author discussed the theories which account for their presence:—1. That the animals lived much further south, and were carried down by rivers to where they now lie; 2. That they lived on the spot. As there are physical difficulties in the way of the transport theory, as the mammoth was covered with dense hair and fed on plants growing on the spot, and as the remains are not confined to the vicinity of rivers, it is probable that the second view is the correct one. It seems probable that the climate of Siberia has become more severe. The author considered the cause of the mammoth's extinction. This he held to have been sudden. The remains must have been preserved after death. He therefore maintains that they were destroyed by a flood due to some sudden convulsion which also changed the climate.—On the association of dwarf crocodiles *Nannosuchus* and *Theriosuchus pusillus*, *e.g.*) with the diminutive mammals of the Purbeck series, by Prof. R. Owen, C.B., F.R.S. The author noticed an objection which had been raised to his view of the origin of the differences between the mesozoic and neozoic crocodiles by the adaptation of the latter to the destruction by drowning of large mammalia (*Q. J. G. S.*, xxxiv. p. 422), namely, that mammals were coexistent with the mesozoic forms, and remarked that from their small size they would hardly constitute a suitable prey for the crocodiles to which he then specially referred, but would be more likely to perform the same part as the ichneumons of the present day, which check the increase of crocodiles by destroying their eggs and newly-hatched young. He stated, however, that in waste slabs of "feather-bed" marl which accompanied the Becklesian Purbeck Collection to the British Museum, the remains of small crocodiles were detected in considerable abundance; and he gave a description of these, and especially of one which he named *Theriosuchus pusillus*. This reptile, which is estimated to have been about eighteen inches long, had scutes presenting the "peg and groove" character of those of *Goniopholis*, with which genus it further agreed by having the antorbital part of the skull of the broad-faced alligator type. In the dentition it resembled the triassic theriodonts more than any other crocodiles. The vertebræ are amphiplatyan. In conclusion, the author indicated the conditions which have to be fulfilled in the case of recent crocodiles to enable them to drown a large mammal without inconvenience to themselves, and showed that these conditions were realised also in the neozoic forms, whilst there was no reason to suppose that any mesozoic crocodiles possessed the adaptations in question.

Anthropological Institute, November 12.—Mr. John Evans, D.C.L., LL.D., F.R.S., president, in the chair.—The following new Members were announced:—Mr. M. J. Gabriel, and Mr. George H. Radford.—Mr. Robert Cust read a report on anthropological proceedings at the Oriental Congress, in which he gave a digest of all the papers and discussions at that Congress which appertained to the science of anthropology.—Mr. Park Harrison read a paper on some characters which are still in use as tattoo-marks by the Motu, a people located in the South-Eastern Peninsula of New Guinea, and described by the Rev. Dr. Turner as a superior race to the Papuans, from whom they differ both in colour and customs. About half of the more distinctive forms tattooed on a Motu girl, carefully copied by Dr. Turner, correspond with letters in the Asoka inscriptions in India, which are believed to be allied to Phœnician, whilst several others resemble letters admittedly derived from the same stock, but independently acquired. The marks are mostly arranged in groups of three; on the right arm, however, nine or ten are apparently connected by a line running above them all. The characters are twenty-three in number, and are formed of straight lines in the following combinations; viz., five of 2 lines, nine of 3 lines, five of 4 lines, and three of 5 lines, much in the same proportion as in the Rejang and Lampong alphabets of Sumatra, the letters of the former of which have been shown to be identical with Phœnician characters *reversed*. Archaic forms of letters have also been met with in several islands of the Indian Archipelago and Melanesia, but are now without meaning. The Motu characters are used simply for ornament or as charms. As an example of the use of letters for tattoo-marks, the case of the Austrian subject was cited, who, having been taken prisoners in Burmah, a few years ago, was there tattooed with letters and other patterns. Besides the characters on the Motu girl, there were various pictures, or hieroglyphics, consisting of eyes and eyebrows, a lunar crescent, and other forms.

Meteorological Society, November 20.—Mr. C. Greaves, F.G.S., president, in the chair.—Rev. T. L. Almond, Rev. T. C. Beasley, F. T. Bircham, H. F. Blanford, G. Chatterton, E. Easton, W. L. Fox, G. F. Lyster, Lieut.-Col. W. Stuart, R. Tennent, and H. Yool were elected Fellows of the Society.—The following papers were read:—Report on the phenological observations for 1878, by the Rev. T. A. Preston, M.A.—Up-bank thaws, by the Rev. Fenwick W. Stow, M.A.—Comparison of thermometric observations made on board ship, by Capt. H. Toynbee, F.R.A.S.

PARIS

Academy of Sciences, November 11.—M. Fizeau in the chair.—M. Loewy presented a memoir by M. Stephan and himself, on determination of the two differences of longitude, Paris-Marseilles and Algiers-Marseilles. He remarked, on the difference of velocity in transmission of signals through air and under water, that this velocity was found about 36,000 kilom. per second in the former case and 4,000 kilom. in the latter, numbers agreeing closely with those got lately by Dr. Albrecht, in Prussia, from shorter lines.—On the vision of colours, &c., second extract from work by M. Chevreul.—On the dilatation of heated bodies and the pressures they exercise, by M. de Saint Venant.—On the energy of a body and its specific heat, by M. Clausius.—Report on a memoir of M. Popoff, entitled, "New Researches relative to Expression of the Conditions of Motion of Water in Sewers." This shows the necessity of new formulæ, involving either change of known numerical coefficients or consideration of the movement as being generally varied. Several problems are enunciated as needing solution.—On measurement of the magnifying power in optical instruments, by M. Govi. It is inexact to say such and such a lens or microscope magnifies a certain number of times the image of objects, while it is not added at what distance the image must be for this magnification to take place. The distance of distinct vision is variable.—On the possibility of obtaining, with protoxide of nitrogen, an insensibility of long duration, and on the harmlessness of this anæsthetic, by M.

Bert. He recommends putting patients in an apparatus with the pressure raised to two atmospheres, and making them breathe a mixture of 50 per cent. protoxide of nitrogen and 50 per cent. air; thus long anæsthesia is had, while the normal quantity of oxygen is kept up in the blood.—Observations on M. Levy's memoir on a universal law relating to the dilatation of bodies, by M. Massieu.—On the transformation of linear forms of prime numbers into quadratic forms, by M. Oltramare.—Artificial crystallisation of orthose, by M. Meunier. The author obtained this (which MM. Fouqué and Levy are now seeking to effect) some years ago, by devitrification of the vitreous masses called retinites.—New process for application of galvanoplasty to conservation of nervous centres, by M. Oré. A hardened brain is dipped in fused gutta percha, and the gutta percha, on hardening, is divided and separated, forming a mould; this is lined with black lead and put in a nickelising bath; thus a hollow piece is had faithfully reproducing the brain.—Resistance of some wild types of American vines to phylloxera, by M. Millardet.—On the reduction of certain differential equations of the first order to linear form with reference to derivatives of the unknown function, by M. Halphen.—On the form of integrals of differential equations in the neighbourhood of certain critical points, by M. Picard.—On the theory of machines of the Gramme order, by M. Breguet. To obtain the best possible effect from a system formed by a movable circuit rotating in a magnetic field (1) if the motion is caused by a current of foreign source, the diameter of the points of contact should be displaced in the direction opposite to the rotation, and through a greater angle the greater the intensity of the current and the weaker the magnetic field; (2) if the motion has to generate a continuous current in the apparatus the same diameter should be displaced in the direction of the rotation.—Chemical researches on tungstates of earthy and metallic sesquioxides, by M. Lefort.—Analysis of different metallic fragments from the Peruvian burying-places of Ancon, near Lima, by M. Terreil. This reveals the presence of brass in these tombs belonging to the sixteenth century.—Synthesis of uric derivatives of the alloxane series, by M. Grimaux.—On some causes of inversion of cane sugar, and on the consecutive alterations of the glucoses formed, by M. Durin. The causes referred to are heat, water, and time (without pre-existent glucose), the phenomenon being purely chemical.—On the hatching of bees, by M. Girard.—Specific determination of fossil or ancient bones of Bovides, by M. Sanson. The bones of Bovides found in beds before the present geological epoch belong to the groups of bisons and bulls: the first, all to one species, now living (*B. americanus*), the second to four living species (specified).—On the presence of alcoholic ferment in air, by M. Miquel. Sterilised must exposed among the vineyards of the south of France always ferments in a few days; this is attributed to conveyance of wine-yeast by insects. He shows that the air really transports yeast. In the Montsouris Park, Paris, not a single case of spontaneous alcoholic fermentation was met with.—Organisation of *Hydrocrosis arsenicus*, Bret., by M. Marchand.

November 18.—M. Fizeau in the chair.—The following papers were read:—Meridian observations of small plants at the Greenwich and Paris Observatories, during the third quarter of 1878, communicated by M. Mouchez.—On a fresh discovery of Silurian terestrial plants in the slaty schists of Angers, by M. Crié; note by M. de Saporta. This is the frond of a fern resembling most nearly *Cardiopteris polymorpha*, Gœpp., which characterises the carboniferous limestone of Silesia; but it has also special features. (A figure is given.)—Means of measuring the manometric value of the pressure of the blood in man, by M. Marey. This consists in producing on a part of the body surface a known counter-pressure with water, capable of overcoming the blood-pressure in the vessels. The simple immersion of a finger in a suitable apparatus suffices; it has shown that in some adynamic fevers the blood pressure may fall to 3 ctm., while in interstitial nephritis it may rise above 20 ctm.—New remarks on M. Levy's communication, on a universal law relating to dilatation of bodies, by Prof. Boltzmann.—Observations on MM. Gruey and Hirn's notes regarding a gyroscopic apparatus, by M. Sire.—On an alterating gyroscopic tourniquet, by M. Gruey.—On a new system of electric lamp, by M. Werdermann.—Artificial reproduction of felspars and of a complex volcanic rock (pyroxenic labradorite) by the method of igneous fusion, and prolonged maintenance at a temperature near fusion, by MM. Fouqué and Levy.—Migration of pucerons of galls of lentiscus to the roots of Gramineæ, by M. Lichtenstein.—M. Oder presented (through M. du Moncel) an electrophone, with which words and notes can be heard 5 m. off. On one end of a sort of drum is fixed a diaphragm of parchment paper, having at the centre six small bars of white iron, fixed circularly, on which act six very small horseshoe electro-magnets connected together and actuated by a carbon microphone. The strong effects are attributed to the smallness of the magnets, giving more rapid magnetisation or demagnetisation.—Intra-Mercurial planets observed during the solar eclipse of July 29. Letter from Mr. Watson. A reply to questions.—On the development of surfaces whose linear element is expressible by a homogeneous function, by M. Levy.—Note on the determination of imaginary roots of algebraic equations, by M. Farkas.—Action of hydracids on sulphate of mercury; action of sulphuric acid on the haloid salts of this metal, by M. Debray. Sulphate of mercury gently heated in hydrochloric gas absorbs it with liberation of heat, and yields a matter fusible and volatile without decomposition, condensing in fine white needles; it has the formula $HgO,SO_3 + HCl$. It may be had directly by union of mercuric chloride and monohydrated sulphuric acid.—Peculiar action of platinum wire on hydrocarbons; modification of the grisoumeter, by M. Coquillion. Bicarbide of hydrogen mixed with air is more detonant than protocarbide; palladium gives a less detonation than platinum; and these two metals can equally burn at red-white small quantities of gas. Thus platinum may be substituted for palladium where there is no fear of detonations.—On the alkalinity of carbonates and silicates of magnesia, free, mixed, and combined, by M. Pichard.—Action of the cervical sympathetic on the pressure and velocity of the blood, by MM. Dastre and Morat. An unforeseen result is that the initial constriction on stimulation of the nerve is always followed by a dilatation greater than that which follows section of the nerve.—On the toxical power of the extract of seeds of hemlock, by MM. Bochefontaine and Mourrut. The common extract obtained from the whole plant is often almost without action; not so extract from the dry seeds; it is in them the active principle specially resides.—On a disease of lettuce (*Peronospora gangliiformis*, Berk.), by M. Cornu.—On the morphology of dicotyledonous stems, by M. Guinier. He applies the graphic method. *Inter alia*, at heights under 1,400 metres, stems bulge out about the middle; as you go higher, the swelling disappears, and about 1,700 m. height, it is replaced by a concavity. From the leafy head of trees may be deduced the form of the stem.—Observations on the orography of the chain of the Pyrenees, by . Schrader.

CONTENTS

THURSDAY, DECEMBER 5, 1878

BOTANICAL CHEMISTRY

The Organic Constituents of Plants and Vegetable Substances and their Chemical Analysis. By Dr. G. C. Wittstein. Authorised Translation from the German Original, Enlarged with numerous Additions, by Baron Ferd. von Mueller, F.R.S. (Melbourne: McCarron, Bird and Co., 1878.)

MANY who have been interested in botanical studies may have been struck with the varied nature and great number of chemical substances which are extracted from plants and which in many cases impart to the flowers their special and characteristic brilliant colours. Up to the present time we have had in English no work which has devoted itself specially to the systematic description of the nature and preparation of such substances; and the literature in connection with the subject, which is by no means meagre, with accounts of investigations in chemico-botanical research must be sought for in journals generally devoted to purely chemical matter, and consequently less likely to attract the attention or be available for the use of the more general reader.

With regard to other countries, however, this has not been the case, as we find a considerable portion of the work of Berzelius devoted to a consideration of the bodies found in plants, and the "Anleitung zur Analyse von Pflanzen und Pflanzentheilen" of Rochleder has long been known both in Germany and elsewhere as affording a good collection of the results of investigations in vegetable chemistry up to the date of its publication about the year 1858.

During the last year, however, Baron Ferd. von Mueller has brought forward a translation of Dr. G. Wittstein's "Anleitung zur chemischen Analyse von Pflanzentheilen auf ihre organischen Bestandtheile," published in 1868, the value of which he has found in his own researches, and which he has for some time wished to render available for English readers.

The present edition consists of two parts, each divided into three divisions, which form as it were the chapters. In the first part the author has placed the consideration of the proximate constituents of plants and vegetable substances as far as hitherto known, together with their properties and mode of extraction. His first intention in the arrangement of this part, which is naturally the largest portion of the work, was to adopt as far as possible a systematic classification. From the imperfect state of knowledge, however, as to the exact constitution of the bodies, and from the fact also that some of the better known substances possess properties which might cause them to appear in several groups if a classification depending on natural properties was taken, he has finally adopted an alphabetical order and has thus formed a dictionary of so-called phytochemical substances.

In the consideration of the individual substances Baron von Mueller has evidently confined himself almost entirely to their preparation from natural sources, and we have therefore no description of the very interesting and remarkable synthetical methods which are now employed for the production of certain of these bodies. This may of course have been beyond the limit originally intended by the present editor, but we should hope in the event of future editions to have some mention made of the more important recent investigations in this direction. The two latter divisions of the first part are occupied with a synopsis of the plants which yield the bodies previously described and a list of the plants indicated, systematically arranged in their different natural orders. The first of these lists is remarkably good, as it gives not only the names of the plants and those of the substances which they yield, but also the various parts of the plant from which these latter may be extracted. The want of such a list has been felt, and this part of the work might have been extended to rather wider limits; in its present form, however, it will still prove of considerable use. In assigning chemical formulæ to the substances described in the first part of the book the editor has retained the older forms of notation, but has introduced immediately after the alphabetical list of bodies a table containing the molecular weights of the compounds described according to the modern views adopted by chemists; this necessity for two lists concerning the same thing introduces confusion in the mind of the reader, and it would be well therefore in a future edition to dispense altogether with the older forms of molecular weights as they are little used at the present day.

The second part of the work is devoted to the apparatus and reagents necessary for phyto-chemical analysis and to the description of a systematic course embracing the different methods of procedure in conducting such researches. In these analyses one of the most important points is the proper extraction of the various ingredients of the plant; for this purpose solvents such as ether, alcohol, and water are employed. As at first sight it might appear immaterial in what order these solvents are to be used the author points out the importance of employing them in the following order: first ether, then alcohol, and finally water, and by this means preventing such bodies as wax or fat which are completely and entirely dissolved by ether, from passing also into the alcoholic extract in which they are only partially soluble. This also would apply to the extraction of certain of the alkaloids, as in their case partial separation may be carried out in their extraction by the different solvents.

It is to be regretted that the more modern names and atomic weights are not employed in this portion of the work, and also in the description of the preparation of reagents; thus, we find the molecular weight of calcium carbonate given as 625, and that of calcium oxalate as 1025: at the present time the use of such numbers tends greatly to confuse the student.

At the end of the work Baron von Mueller has arranged some useful tables, comprehending the comparison of Centigrade and Fahrenheit thermometric scales, the specific gravity of alcohol of different percentages by weight and by volume, the relation between cubic centimetres and cubic inches, between litres and fluid ounces, and a table of the atomic and molecular weights of the principal elementary bodies.

There can be little doubt that this work supplies a great want in chemical and botanical literature, but there is still room both for the farther elaboration of the matter discussed, and, in certain cases, for some

improvement in the rendering into English of the matter already employed. In the direction of chemico-botanical research there is great room for investigation, and a text-book embracing the knowledge already acquired, and information on points in connection with the chemistry of vegetable physiology, would render such a work of interest not only to the scientific chemist or botanist, but also to the general reader. Baron von Mueller's translation forms an excellent nucleus for such a work, and should a future edition of the book be required, we should hope to find it enlarged in such directions.

J. M. T.

GEOGRAPHICAL ASTRONOMY

Abriss der praktischen Astronomie, vorzüglich in ihrer Anwendung auf geographische Ortsbestimmung. Von Dr. A. Sawitsch, nach der zweiten russischen Original-Ausgabe. Neu herausgegeben von Dr. C. F. W. Peters. (Leipzig, 1879.)

As may be inferred from the title of this work, the astronomical reader will not find it to be a general treatise on the practical branches of the science, but one confined to the theory and uses of instruments, and explanation of methods employed at the present day in the determinations of geographical positions. As such the name of its author, Dr. A. Sawitsch, the well-known Professor of Astronomy in the Imperial University of St. Petersburg, will give the work high recommendation in the estimation of the student. The two volumes of the original edition are now incorporated in one, and such modifications as have been rendered necessary by the introduction of new or improved forms of instruments, and refinements of observation and reduction have been introduced in a great measure by the author himself. In the opening chapter we have explanations of the various methods of reckoning time, and the transformation of one into another; the reduction of mean into apparent places, the calculation of refraction and parallax, and the influence of the earth's compression upon the geocentric co-ordinates of points upon the surface, with remarks upon angular measures in general, and upon the astronomical telescope and its adjustment, the microscopes, verniers, level, &c. In the first section, the author treats of the transit instrument, and enters into the various adjustments to which it is subjected, and also describes in some detail the universal instrument of Piston and Martins, and the errors of division to which instruments for angular measures may be liable. The second section is devoted to the determination of latitude and time by measure of zenith distance, of time from corresponding altitudes, &c. The third section enters more fully into the uses and theory of the transit instrument, and likewise describes Bessel's method for the determination of latitude thereby, supplying practical rules and an example. The next section treats of the determination of azimuth, and of the influence of diurnal aberration on the polar co-ordinates of a star. The fifth section contains a valuable outline of the various methods applicable to the determination of terrestrial longitude, including the telegraphic method, the transportation of chronometers, and longitude by observations of eclipses, especially those of the sun, and by lunar occultations.

The reference to the utility of eclipses for longitude-determination leads to an important chapter on Hansen's method for the calculation of the general circumstances of these phenomena upon the earth's surface, and the methods followed by Dr. Zech, in his researches on the historical eclipses; and, as a numerical example, the formulæ are applied to the computation of the circumstances of the total solar eclipse of August 18, 1887, to which frequent reference has been made in astronomical treatises. The data are founded upon the lunar tables of Hansen and the solar tables of Leverrier. Further, we have a discussion on moon-culminators in their application to longitudes, with notices on the methods of Nicolai and Struve, and a fully-worked-out example. The sixth section relates to the reduction of the longitude, latitude, and azimuth of a place to another, both accurately and approximately, and the determination of the distance of points on the terrestrial spheroid, of which the geographical positions are given. There are two supplementary chapters: the one bearing upon reflection-instruments, and of course entering at length into the use of the sextant; the other treating of interpolation, with special reference to the formulæ of Bessel and Hansen.

In the language in which this work originally appeared it would be almost a sealed book in Western Europe. The excellent translation into a language of which every scientific student should, in these days, possess a knowledge, now placed in our hands by Dr. Peters, will be, without doubt, a welcome addition to his means of instruction on an important branch of practical astronomy.

OUR BOOK SHELF

A Treatise on Dynamics of a Particle, with numerous Examples. By P. G. Tait and the late W. J. Steele. Fourth Edition. (London: Macmillan and Co., 1878.)

THE bibliography of this revised text-book is—a first edition in 1856, 304 pages; a second edition in 1865, 363 pages; a third edition in 1871, 428 pages; and the present edition of 407 pages. There are slight alterations in the disposition and amount of the matter in this edition, caps. x. and xi. of the third are put into cap. ix., caps. v. and vi. are contained in cap. v. of the fourth. The position of some of the exercises has been changed. The main features remain unaltered. The revision has had the advantage of Prof. Greenhill's supervision, who has verified (and corrected where necessary) the Examples and has freely introduced the use of Elliptic Functions. There is no need of any commendation for a text-book so well-known. We are, however, very much disposed to think that had Prof. Tait composed the work at a later date than he did, it would have differed somewhat from its present form and have approximated more closely to the Natural Philosophy brought out under the joint editorship of Sir William Thomson and himself. The author justly complains that "several sections in which some novelties appear have been translated almost *letter* for *letter* and transferred, without the slightest allusion to their source, to the pages of a German work. Several other books have obviously been similarly treated. It is well that this should be known, as the English authors might otherwise come to be supposed to have adopted these passages *simpliciter* from the German."

Familiar Wild Flowers. Figured and Described by F. Edward Hulme, F.L.S., F.S.A. First Series. With Coloured Plates. (Cassell, Petter, and Galpin.)

SCIENTIFIC books are of three kinds: to inform the

scientific world of some fresh discovery or advance—works of research; to offer a digest, for the information of students, of results already attained—text-books; and to attract to the paths of science the outside public—popular works. The pretty and attractive book before us belongs to the last of these categories, and is, we think, well calculated to gain the end in view. It consists of chromo-lithographs of nearly fifty of our better-known native wild flowers, with two or three pages of gossipy talk about each. Of the letter-press not much more can be said than that it is fairly accurate from a botanical point of view, and pleasantly written. The illustrations strike us as unusually good of their kind. They have of course the inherent defects of this mode of illustration, in the absence of half-tones and delicate shades; but the general aspect of the plant is in nearly all cases well and faithfully given, and the drawing is good. The book is a very good one to put in the hands of a child to interest him or her in the wealth of wild flowers which is such a source of delight to all dwellers in the country who have eyes educated to see their beauty.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Telephone

I HAVE just read an article in NATURE, vol. xviii. p. 698, on the history of the speaking telephone, which contains an erroneous statement of facts which happen to be within my own knowledge; so that, in the interest of a truthful history of this discovery, it is incumbent upon me to make a brief statement in regard to it.

I had the honour to be one of the judges at the International Exhibition at Philadelphia, and of the group to whom was confided the examination of instruments of research and precision. In the performance of my official duty I took part in the experiments which first brought the speaking telephone to the notice of the scientific world. Prof. Bell and Mr. Elisha Gray were both exhibitors at that Exhibition. Mr. Gray's apparatus was conspicuously shown near one of the main aisles, with the exhibit of the Western Electric Company, while Prof. Bell's was in a side room in one of the galleries, as a part of the Massachusetts' educational exhibit. About the middle of June, 1876, Prof. Bell came to Philadelphia to give personal explanations in reference to his apparatus, and before any public exhibition was made he stated to me in detail the character of his inventions. He was working at two independent things, the one the multiple telegraph by means of transmitted musical notes, the other the transmission of articulate speech over long distances. I told him that I was present in May, 1874, at the rooms of Prof. Henry, in the Smithsonian Institution, when Mr. Elisha Gray exhibited to us an apparatus for the electric transmission of musical sounds, and I asked him whether his first invention was similar. He said there was some similarity, although each had worked independently, and that there was a dispute as to the priority of invention. While sanguine as to practical results from his multiple telegraph, his great invention was the speaking telephone, which he believed he had discovered, and in respect to which there was no rival claimant. He said the idea came to him from some of the suggestions in respect to sound vibrations made by Helmholtz, and that he had succeeded, after patient research, in constructing an instrument which would transmit articulate speech. To this invention he desired to direct the attention of the judges.

The experiments with the telephones had to be made when the Exhibition was closed to the public, and the first experiments were made by Sir William Thomson and others on Sunday, June 18 or 25—I do not now remember upon which of these two dates. Their Majesties the Emperor and Empress of Brazil were present at these experiments. Attention was first given to Mr. Gray, and he gave a lengthy account of his experiments, which had resulted in the perfected apparatus which he then exhibited. He gave an explanation of his various instruments in chronological order, and conducted some very entertaining experiments as he proceeded in his discourse. The object which he had in view was to send many messages simultaneously over the same wire by using sending and receiving instruments of different musical notes.

The greater part of the day was given to Mr. Gray, so that insufficient time remained for satisfactory trial of Prof. Bell's apparatus. The judges and the distinguished visitors present did, however, proceed to the Massachusetts gallery, and Prof. Bell explained briefly his two inventions, and some experiments were made with his speaking telephone, enough to excite the curiosity of those present in the highest degree. The results were so at variance with the views hitherto received that it was determined by my distinguished colleague, Sir William Thomson, to make other experiments, in which I took part. These experiments were made two or three days later, in the building known as the Judges' Pavilion, in the evening, after the visitors had left the grounds. Prof. Bell had returned to Boston, and was not present at this trial of his apparatus. It was brought over to the judges' pavilion, at my request, by Mr. Hubbard, one of the officers in charge of the Massachusetts exhibit, and the experiments were made by Sir William Thomson and myself. Every precaution was taken to make an impartial test. I was at the transmitting instrument which was placed out of doors at a distant part of the building, and Sir William Thomson was at the receiving instrument in a distant room in the building. After some experiments to find the pitch of voice which would suit the vibrating membrane then used, I received word by messenger from Sir William that he could then hear distinctly, and accordingly the pitch of voice then used was maintained in the subsequent trials. I held in my hand a copy of the *New York Daily Tribune*, and I began to read to him items from its news summary, and soon the messenger came to tell me that the messages were heard distinctly at the other end. The longest message which I sent was the following from that paper:—"The Americans of London have made arrangements to celebrate the coming Fourth of July," and the messenger brought me back from Sir William Thomson the exact repetition of the message. Thereupon we exchanged places, and I could not only hear distinctly the utterances of my colleague, but I could even distinguish the ictus of his voice. The results convinced both of us that Prof. Bell had made a wonderful discovery, and that its complete development would follow in the near future.

The news of these successful experiments soon circulated freely, and the day following, or possibly two days afterwards, Mr. Gray came to me and inquired whether the reports of our success with Bell's telephone were correct; and upon receiving from me an affirmative reply, he said that it was impossible, that we had been deceived in some way, that the transmission was by actual metallic contact through the wire, and that it was, to use his own words, "nothing more than the old lover's telegraph." In reply I said to him that we had taken every possible precaution against error, that we were both convinced of the reality of Bell's claims, and that Sir William Thomson would report to that effect. He persisted in his statement that the result was impossible, and that we must have been deceived in some way or other.

After having had direct knowledge of Mr. Gray's views at that time, I must confess to some astonishment at his claim now made that he anticipated Mr. Bell in the invention of the speaking telephone. Several months ago I saw an article in *Scribner's Magazine*, by Mr. Prescott, in which, while no direct assertion was made that Mr. Gray was the first inventor, there were illustrations given to show the development of the invention in chronological order, and Mr. Gray's instrument was there given priority. I had it in mind then to write a note to Mr. Prescott upon this subject, but I feared that there might be unpleasant controversies over the patents, and, the claim of Mr. Gray being rather indefinitely stated, I held my peace. But now that the error appears to be taking root, I have felt it to be my duty to make the statements above given. I have before me a letter from Mr. Bell, dated at Boston, Wednesday, June 28, 1876, and directed to me at Philadelphia, in which he gives diagrams showing how we might arrange the apparatus to transmit articulate speech, as he believed, from Boston to Philadelphia, and proposing experiments to that end if the judges should so desire.

In conclusion I ought to state further, that after Sir William Thomson's address at Glasgow had brought the telephone into

notoriety, Mr. Gray, whose instruments had also been called telephones, gave a public exhibition, in Chicago, I think, and in the report of his lecture which I read, he never once alluded to Bell's invention. His discourse was then, as at Philadelphia before the judges, solely in reference to the musical telephone. In fact, the newspapers had to take pains to inform the public that Mr. Gray's invention must not be confounded with Mr. Bell's, to which Sir William Thomson had referred. You will imagine, then, the surprise of the judges who examined these inventions particularly at Philadelphia in 1876, and heard the personal explanations made by the inventors, to be told now that Gray had already invented the speaking telephone, when all his statements then made show directly to the contrary.

Ann Arbor, November 18 JAMES C. WATSON

The Intra-Mercurial Planets

NATURE (vol. xviii. p. 569), in commenting upon my letter published the previous week, regarding the discovery of Vulcan, accused me of being not only "indefinite," but "contradictory." The number containing my letter (p. 539) has, from some unknown cause, not yet reached me, though I am in receipt of four numbers published later.

In the several articles written by me on that subject—to the Chicago Astronomical Society, to the Naval Observatory at Washington, to the Astronomer-Royal, to Admiral Muochez of the Paris Observatory, and to others—I have invariably stated the facts as they occurred under my observation, and as they impressed themselves upon my mind, and have invariably adhered to these statements, viz., that the two stars seen by me were of about the fifth magnitude, about 7′ or 8′ apart, with large red disks, and pointing towards the sun's centre. It is true my letter did contain an error, but not of observation, nor of estimation. In reducing the 8′ of arc (the estimated distance between the stars) to time, I somehow called it 2′, when, in reality, it is but 32s., thus not only changing its position in R.A., but also increasing, in this element, the discordance between Prof. Watson and myself. The detection of this error has changed, to me, the whole aspect of the Vulcan question. I had previously written to Prof. Watson that I could not reconcile his observations with my own either in R.A., or in Dec., but did not tell him what changes were necessary in order that they might harmonise. He gave me his corrected positions, which helped matters considerably, but still his R.A. was too great, and Dec. too little, for, from three estimations, the two stars ranged with the sun's centre. Recently I have been experimenting with α^1 and α^2 Capricorni (two stars which, in respect to distance from each other, resemble those I saw during the eclipse), my object being to test the accuracy of estimations made of the directions towards which two stars will range when hastily brought into the estimated centre of the field of a telescope having a diameter of one and a half degrees. I find that unless the objects are brought exactly to the centre, they do not point to the same place. During totality time was, of course, too precious to waste in being precise in this, and yet I endeavoured to be so, and as at each of the three comparisons they seemed to range with the sun's-centre, I feel convinced that I was not far out in my estimated Dec.

In order to meet Prof. Watson's excessive R.A., I published (contrary, however, to my better judgment), that the distance between the stars was about 8′ instead of 7′ (as previously announced). On the assumption, therefore, that (a) one of the objects was θ Cancri, and (b) that they were 8′ apart, and (c) that the one nearest the sun was the planet, as Watson says, the position of the planet was as follows:—

Washington M. T.	
1878, July 29, 5h. 22 m. R.A., θ Cancri ...	8h. 24m. 40s.
Add 8′ =	32s.
Planet's R.A., Swift ...	8h. 25m. 12s.
,, ,, Watson ...	8h. 27m. 35s.
Difference	2m. 23s.
Dec. Swift	18° 30′
,, Watson	18° 16′
Difference	14′

It will be seen that there is a discrepancy between us of over a half degree of arc in R.A. If we saw the same objects how can we differ so widely? Could I be in error to the amount of 34′ between two stars in the same field? Can two stars be three and one half times the distance of Mizar from Alcor and an observer of experience estimate them at only 7′ or 8′? It will be remembered that I recorded in my note-book at the time the distance as 12′, but knowing how liable I might be to error in the valuation of so large a distance (for though, from practice, I can estimate quite closely double stars whose distances are from 2″ to 20″, I have had no experience in the estimation of those of several minutes separation), I chose to carry it in my mind until I should reach home, when it would be the work of only a few minutes to find two stars of the same apparent distance.

I said to Prof. Hough on our homeward journey, that, from memory, I thought their distance was about equal to that separating α^1 and α^2 Capricorni, and that I could decide when I should observe them. My memory of Mizar and Alcor was quite distinct, and as soon as I thought of those (which I did before my arrival at Kansas City) I mentally said, "A little over half the distance between them equals that between θ Cancri and the new object," which I did not doubt was Vulcan. Upon my arrival at home I immediately consulted "Webb's Celestial Objects," and was not a little surprised to find their whole distance to be less than 12′. Thus I know they were not over 8′ apart, I believe they were but 7′. I know they pointed to the sun's disk, I believe they did to his centre. I know they did not differ one-fourth of a magnitude in brightness, I believe they were exactly equal. I see them, in my mind's eye, as I then saw them, and, while consciousness endures, their image can never fade from the retina of my memory!

I consider the estimated distance in arc, made in such great haste, as valueless compared with the distance as impressed upon the mind from three comparisons, and verified by observations of a reliable character since arriving at home.

Can any error, then, be ascribed to the measurements of Watson, a skilful observer, with telescope well mounted, and with appliances for measuring, and who not only did measure the position of the new planet, but that of the sun and θ and δ Cancri (three objects in its immediate neigbourhood) as well.

Have we any right to call in question the accuracy of his circles in giving the position of the new object when they correctly gave the positions of the others?

Wherein, then, lies the discrepancy, and how can it be reconciled? Again, Watson says the planet was much brighter than θ, while the stars which I saw were of equal magnitude.

Several times since my return from the eclipse expedition I have, both in darkness and in strong twilight, examined θ, and I find no star near it, nor no two stars in its vicinity answering, in any particular, to those seen by me at Denver.

The above facts I submit to the world, and astronomers must deduce therefrom their own conclusions as to what the objects were. My own are reached, and, briefly stated, are as follows: —That the two objects seen by me were both intra-Mercurial planets, and that I did not—as was for a time supposed—see θ Cancri. Prof. Watson saw θ, and, some 42′ of arc south-east of it, another planet, and determined its position, and near to ζ Cancri still another, whose position also he fortunately ascertained, making four in all. It will not do to say, as some have intimated, that Watson saw θ Cancri, and 42′ from it a planet which I did not see, and that I, also, saw θ, and, 7′ or 8′ from it, another planet which he did not see. This reasoning appears to me untenable, for how could he have failed to see mine, when the diameter of his field was over 40′, and had θ in its centre?

If the above conclusions are true, and that four planets were discovered instead of two (as at first supposed), the question naturally arises, Which, if any one, is Lescarbault's Vulcan?

I estimated, at the time, the objects as being of the fifth magnitude, that is, as bright as a fifth magnitude star would appear in a clear, dark night. How much allowance ought to be made for diminution from atmospheric illumination I know not. I was then of the opinion that it would make a difference of at least one magnitude, but, having examined the region around θ, and finding many stars there, and several which are quite bright, not one of which I saw during the eclipse, I think that fully two magnitudes should be allowed.

In what way can these intra-Mercurial planets (of which there are probably many) be detected?

I would suggest that, on July 29 next, a determined and systematic effort be made, with large telescopes equatorially mounted, to observe θ Cancri, and, if then successful, there is hope that these planets, or some of the larger ones, may be discovered in the absence of a total eclipse, or while in transit. If

θ cannot be thus seen, then it appears to me that all time spent in their search in the sun's vicinity, except during a total or very large partial eclipse, would be time lost.

Rochester, N.Y., November 8 LEWIS SWIFT

Colour-Variation in Lizards.—Corsican Herpetology

IN a communication sent to you by my friend Mr. Wallace, under the title, "Remarkable Local Colour-variation in Lizards," published in NATURE, vol. xix. p. 4, mention is made of the well-known case of *Lacerta* (Podarcis) *muralis*, var. *faraglionensis*, only found on the Outer Faraglione of Capri, but there are many similar cases to my knowledge, and I add a note of them, for the fact, although unexplained, is one of great interest. During the last two years, while engaged in forming a complete series of the Italian vertebrate animals, I have visited and explored most of the Mediterranean islands included in the Italian sub-region, and I have invariably found that our common lizard (*Podarcis muralis*) constantly presents dark varieties *on islets adjoining small islands:* this is the case on the Scuola, near Pianosa, on the Scoglio di Mezzogiorno, off Palmarola (Ponza), on S. Stefano, off Ventotene, on the Toro, off Vacca (Sardinia), on Lisca nera, Lisca bianca, and Bottaro, off Panaria (Lipari), on Filfla, off Malta, and on Linosa, near Lampedusa. The extreme cases are those of the Faraglione off Capri and Filfla, where a nearly intense black is obtained; next comes Toro, and next Linosa; only the latter case might be explained by the "struggle for existence" theory, for the lava rocks of Linosa *are* black; but such is certainly not the case with the other islets, and, *pace* Dr. Eimer, the Faraglione is gray, while Filfla—on which I spent a pleasant day in October last—is painfully white in the glaring Maltese sun, so that its black lizards are most conspicuous. I may add that few creatures I know vary more in colour than *Podarcis muralis*, even in the same locality; two most distinct varieties occur promiscuously on the small flat islet Formica di Grosseto.

Going over my Mediterranean herpetological notes reminds me of an interesting discovery I made last summer in Corsica, an island of great interest, which, strange to say, is rarely trodden by naturalists. Most of your zoological readers will be aware that, in 1839, Prof. Savi, of Pisa, described two new species of Italian Urodela, both from Corsica, viz., *Salamandra corsica* and *Megapterna montana*. The former has been quite neglected by modern herpetologists, or else placed among the synonyma of *S. maculosa*, simply because no one had Corsican specimens to compare. Now it is evidently nearly allied to the Continental form, but *quite distinct*, as the specimens I collected testify, all of them presenting the distinctive characters pointed out by Savi forty years ago. A nearly similar lot befell *Megapterna montana*, which Savi described nearly contemporaneously with Genè's description of *Euproctus Rusconii*, from Sardinia. Buonaparte, in his "Fauna Italica," united the two under the name of *Euproctus platycephalus*, given by Gravenhorst in 1829 to a newt, *sine patria*, preserved in the Breslau Museum; and most naturalists have followed Buonaparte, especially later writers on the subject, as Strauch, De Betta, and Schreiber, whilst others, acting more wisely, stuck to Genè's name. I believe that since Savi's day no one has studied the Corsican form, whose essential characters pointed out by the Pisan naturalist, who had only two specimens to work on, were overlooked even by his contemporary, the Prince of Canino; this explains all. Last year I rambled and collected all over Corsica, and found Savi's newt quite common in all the mountainous districts; I secured about 150 specimens of both sexes and all ages, even larvæ, and on my return to Florence was much surprised to find them *quite distinct* from the Sardinian *Euproctus* I possess; this made me refer to the original descriptions, and thus I found that Savi and Genè had described two very distinct species, and described them well. The two Italian species of *Euproctus* may be thus defined:—

E. Rusconii, Genè: Parotids wanting. Skin smooth, with small whitish tubercles scattered, especially about the sides of the head and neck. Female with a small conical pointed fibular tubercle, very like a rudimentary finger. Hind fingers slender and cylindrical. Irregular dark blotches on the throat. Size somewhat larger than the succeeding species. Hab. Sardinia.

E. montanus, Savi: Parotids small but distinct. Skin rough and granular. Female with a large, obtuse, compressed fibular tubercle, more like a ridge or crest, than anything else. Hind fingers stout, broad, and flattened. Throat uniform, rusty, without blotches; often a red or yellow dorsal stripe. Hab. Corsica.

As to *Euproctus platycephalus*, Gravenh., only a careful examination of the type-specimens, if yet existing in the Breslau Museum, can settle to which form it ought to be referred, but if their locality is unknown, I believe it better to suppress the name. *Euproctus platycephalus* is said to be found in Spain, but as I have no Spanish specimens, I cannot give any opinion on that form. In conclusion, I may add that Buonaparte was perfectly right in separating from the former the North African species *T. Poireti*, which is very distinct from our Italian *Euproctus*, in the shape of the head and body, and in the complete absence of any fibular tubercle in the female; it ought to be called *Glossoliga Poireti*. HENRY HILLYER GIGLIOLI

Florence, November 16

Commercial Crises and Sun-Spots

REFERRING to Prof. Stanley Jevons's article upon "Commercial Crises and Sun-spots" in NATURE, vol. xix. p. 33, I beg to draw your attention to the inclosed circular which I issued to my subscribers in April last.

The figures relating to the "Failures in England and Wales," were compiled by my clerks, under my own direction; those relating to the failures in the United States and Canada were supplied by Messrs. R. G. Dun and Co., of New York and London, and it may be observed how nearly they agree (*i.e.*, the failures in England and Wales, and those in the United States and Canada) in their fluctuations, and that there is an agreement between both sets of figures and the sun-spot period.

I have not been able to obtain similar figures for continental states, but I have observed that the complaints of depression in trade there agree, in substance and in time, with those in this country and North America. I have also noticed similar complaints from the southern hemisphere, especially New Zealand.

I refer to Dr. Hunter's suggestion of an Indian famine period in my circular, but I do not find that the famine period in India agrees, in point of time, with the depressions in the temperate zones; it is very probable that the excess of sunshine which produces drought and famine in India has an opposite effect on the prosperity of England and all other countries lying between the same isothermal lines, and that the more moderate degree of sunshine which may suit the Indian cultivator is insufficient to properly ripen English wheat and other produce (oats excepted).

Since April last I have taken several opportunities of ascertaining from agriculturists the effect of the variations in the sun-spots upon their yield of wheat, &c., and I find an agreement between them that during these years of minimum sun-spots the yield has proved bad when thrashed out, in consequence of the kernels being much smaller than in other years. I do not know whether the test has ever been tried or not; if not, I would suggest that some scientific observer should weigh an ounce, or a few ounces, of the kernels of each kind of grain grown in England every year, and count the number of them. I think it would be found that in years of maximum sun-spots wheat and barley kernels weigh their heaviest and oats their lightest, and that these proportions would be reversed in the years of minimum sun-spots. The difference in each kernel or in an ounce of them may, taken alone, appear trifling; but if it is an indication of the difference in the yield of the harvest throughout the whole kingdom, it may be a fact of the greatest importance as showing the cause of the cyclical variations in the prosperity of the country, and it may be of great value to land-owners and agriculturists generally as a guide in the rotation of crops and in allowing fields to lie fallow.

It is in this direction that I look for the causes of commercial depression. The whole of our "home" trade is dependent upon internal prosperity, and likewise a large proportion of our "foreign" trade. Other causes may have some effect upon either or both, such as peace or war, trade-unionism, bank-management, and the like; but the influence of the sun is too far-reaching and too powerful to be checked thereby. Man, by studying the working of its influence and power upon his daily life, may learn how to guard against much of the distress which periodically recurs. JOHN KEMP

Aspley Guise, November 16

"*London, April*, 1878

"*Failures in England and Wales*

"We append a Summary of the failures in England and

Wales, which it has been our duty to publish in *Kemp's Mercantile Gazette* during the past eleven years:—

Year.	1st quarter.	2nd quarter.	3rd quarter.	4th quarter.	Totals.
1867	3,981	4,081	3,555	4,233	15,850
1868	4,091	4,131	4,139	3,501	15,862
1869	3,819	3,997	3,495	5,207	16,518
1870	2,804	1,589	1,773	1,985	8,151
1871	2,142	2,191	1,837	1,994	8,164
1872	2,192	1,980	1,795	2,145	8,112
1873	2,354	2,299	2,054	2,357	9,064
1874	2,193	2,428	2,339	2,290	9,250
1875	2,331	2,277	2,133	2,453	9,194
1876	2,744	2,573	2,670	2,861	10,848
1877	2,829	2,856	2,610	2,952	11,247
Total for 11 years					122,260

. . . .

"The question occurs: Does the number of failures in a year depend upon natural causes? that is to say, Would the number rise and fall periodically according to the state of trade (or national prosperity) if the Bankruptcy Law remained constant? Whenever failures have become frequent, complaints have been made against the Law, and not without reason, but many who complain ignore the existence of any other cause. We compared the foregoing figures with the scientific tables recently published in NATURE, from the pen of Prof. Balfour Stewart,[1] and, being struck with the coincidence in their fluctuations, we further compared them with the statistics published by Messrs. Dun and Co., of New York, of the failures in the United States during the past eight years,[2] which period, being that of the existence of our present Bankruptcy Law, affords us a fair opportunity for making a comparison. Messrs. Dun and Co. report the following as the total failures in the United States during this period:—

1870, Number of Failures,	3,551
1871, ,, ,,	2,915
1872, ,, ,,	4,069
1873, ,, ,,	5,183
1874, ,, ,,	5,830
1875, ,, ,,	7,740
1876, ,, ,,	9,092
1877, ,, ,,	8,822

"Evidently the same causes which were at work in England to depress trade and overwhelm the struggling and improvident classes, were equally effective in other countries—similar complaints of depression come to us from every part of the globe.

"The discussion which has arisen out of Dr. Hunter's suggestion of a 'famine period' in India, has brought to the public some knowledge of the existence of natural periods or cycles, of an average duration of 11·9 years each. The suggestion that England is affected with the same regularity is but reasonable, and although fortunately for us as a nation the effects do not produce famine, it appears evident that some degree of suffering is caused, and that the number of failures is thereby materially increased—the commercial panics which have occurred with about the same regularity furnish further evidence that this is the case.

"If we make due allowance for the excessive number in the last quarter of 1869, caused by the change in the Law, we find that the maximum number of failures in the last cycle occurred in the year 1868, which was the year succeeding the natural minimum; hence we may conclude that about a year is required for the full effect of the natural depression to be reproduced in commerce. The twelve months from October 1, 1867, to September 30, 1868, appear to have been more serious to commercial men than either of the complete years, according to the number of failures:—

"In the 4th Quarter of	1867	there were 4,233	failures.
,, 1st ,,	1868	,, 4,091	,,
,, 2nd ,,	1868	,, 4,131	,,
,, 3rd ,,	1868	,, 4,139	,,
Total		16,594	

"These data indicate that we have not yet reached the worst of the present period—assuming that it runs an average length, we have to endure an increasing number of failures which will not reach its maximum until the fourth quarter of 1879."

JOHN KEMP AND CO.

Since this was written I have had counted the number of failures gazetted since January 1, 1878, and I find that they are *2,042* in *excess* of the corresponding period (January 1 to November 19) in the preceding year. J. K.

[1] *Vide* NATURE, vol. xvi. pp. 9, 26, 45.

[2] Messrs. Dun and Co.'s Annual Circular, January, 1878.

Strange Properties of Matter

THE following are two experiments which will, perhaps interest some of your readers:—

Experiment No. 1.—The "Welding" of Metals at Low Temperatures

Some time ago, in order to estimate the amount of hydrocyanic acid in a solution, I precipitated it with silver nitrate. After having filtered and washed the precipitate, I reduced it to the metallic state by heating to the required temperature. Just as I was about to allow it to cool, I noticed a small piece of dirt among the reduced silver. In order to separate them, I took a thin platinum wire, and pushed the silver to one side, but on attempting to take the wire away the silver remained in contact with it. As I thought this curious, I tried the following experiment. I took a piece of silver foil about one centimetre square, placed it in an inverted porcelain crucible lid, and heated it to about 500° C.; then I brought into contact with it the extremity of a thin platinum wire, and to my astonishment the wire raised the silver from the lid, and it remained in contact when cold, as the silver was so very much below its melting-point; the above fact caused me some surprise, and I could not satisfactorily account for it.

I wrote to Sir W. Thomson, F.R.S., giving him a description of the above experiment, and in return I received a reply asking me to come and show him the experiment at his laboratory. I accordingly went up to the Glasgow University, and repeated it before him. He was very much interested, and advised me to write to NATURE, giving a description of the experiments. Sir W. Thomson gave the following explanation—That it was a remarkable case of "cohesion," the two metals, in fact, "welding," although the temperature was far below the melting-point of silver. The above experiment can be performed successfully at lower temperatures than 500° C., if smaller pieces of foil are taken. Other metals, for instance, copper and aluminium, cohere to silver in the same manner as platinum, but less strikingly.

Experiment No. 2.—A Curious Resonator

Some months ago I made the following experiment:—I took a small tuning-fork and struck it on the table. After the note had died away, so that it was no longer audible, I held the fork in the tip of the flame of a Bunsen burner, when the note was given out, so that it could be heard at some distance. I showed Sir W. Thomson this experiment, who gave the following explanation—That owing to the difference in density of the gases in the flame, the flame acted as a resonator, and so the note was emitted.

It seems to me that experiment No. 1 could be made the subject of an interesting research, but as I am wholly engaged in commercial pursuits, I am unable to take it up.

CHARLES A. FAWSITT

Glasgow, November 12

Galvanometer for Strong Currents

I MUST confess that I was surprised by Mr. R. E. Baynes' communication, in NATURE, vol. xix. p. 33, that the galvanometer I have proposed in NATURE, vol. xviii. p. 707, has already been described. Before writing my article I have searched a good many books and journals relating to the subject without finding an allusion to any such instrument. Since Mr. Baynes drew my attention to "The Elements of Physical Manipulation," by Prof. Pickering, of the Massachusetts Institute of Technology, U.S., I have procured this book and find that it certainly does contain the theory of a galvanometer like mine, with the coil moving round a horizontal axis. As far as I know, such an instrument has, however, not been practically employed either in this country or on the Continent before I introduced it, though its want must have been much felt for some time past. This seems to show that Prof. Pickering's description of the instrument has not been brought to the general

knowledge of electricians. If, therefore, my article in NATURE, at a time when electric currents of great strength are being so widely introduced into practical working, has contributed to make electricians acquainted with this form of galvanometer, I shall not regret the time I have spent in theoretically and experimentally investigating this subject. Prof. Pickering alludes in his book to the improper *dip motions* of the needle as a defect in his form of galvanometer; I think I have obviated this by pivoting the axis of the needle at both ends. Since communicating the results obtained with my *experimental* instrument, more elaborate instruments have been constructed and found very useful in many instances where the ordinary forms of galvanometers would not have answered the purpose. EUGEN OBACH

Woolwich, November 19

Utilisation of the African Elephant

I have just read some remarks in NATURE (vol. xix. p. 54) on the utilisation of the African elephant, which I think are worth considering. Judging from the specimens in the Regent's Park Gardens, which I suppose have not been selected in any way, and are therefore only average samples of the African species, I should say that the African elephant would prove hardier and capable of more work than the general run of Indian elephants. I was surprised to find that the two African elephants I saw in the Regent's Park were what the mahouts call "Dohara Band," which I would translate "double constitution." I do not think that more than five per cent. of Indian elephants are placed in this class. It is very rare indeed to see an elephant of this class in a Government Feelkanah, or in the hands of any European, because wealthy natives value them so highly and give such enormous prices for them, that even when caught by a Government keddah officer they are often sold on account of the fancy prices they fetch. The "Dohara Band" elephant will do a wonderful amount of work on a small quantity of food, and stands fatigue and exposure to the sun far better than any other sort.

To commence elephant-catching operations in Africa, six "koonkies," *i.e.*, elephants trained for catching purposes, would be required, and about twenty men from Assam or some other jungle district of Bengal would be sufficient. With this establishment it would be possible to catch and train at least two hundred elephants in the course of twelve months.

H. L. JENKINS

Clanacombe, Kingsbridge, South Devon, November 24

OUR ASTRONOMICAL COLUMN

ORBITS OF BINARY STARS.—In a communication to *The Observatory*, Dr. Doberck, of Col. Cooper's Observatory, Markree Castle, has summarised the results of the investigations on the orbits of the revolving double stars which have occupied him between three and four years, and which he has conducted with so much skill and laborious application. In a climate where the skies are too irregularly favourable to allow of an astronomer occupying himself wholly upon observations, it would be difficult to name any more interesting work to which he could devote his leisure, than such a revision of the elements of the binary systems. Dr. Doberck's account of his methods in different cases will be of much service to any one who may engage upon similar researches. He has found no reason to detract from the value of the early, graphical method of Sir John Herschel; on the contrary, instances are mentioned where it has been of the greatest service in tracing out the general form of the orbit, for correction by more refined processes, as in the difficult case of Σ 1768: indeed, Dr. Doberck considers it superior to the second method given by Herschel from its admitting of the weights being approximately taken into account with ease. In a provisional orbit for the close double star Σ 3121, the period assigned is thirty-seven years; at present we know of only two more rapid binaries. Some of [the orbits included in Dr. Doberck's paper, have been made the subject of communications to the Royal Irish Academy, and have been published in the *Transactions*.

LALANDE'S STARS, NOS. 5,499 AND 45,400.—Mr. J. E. Gore writes suggesting variation in both these objects. In examining the question of variability of any of Lalande's stars, Mr. Gore will find it necessary, in the first instance, to have recourse to the original observations as printed in the "Histoire Céleste," there being many errors in the reduced catalogue. Thus, No. 5,499 appears through a misprint at p. 246, where the transit at the third wire is given as 2h. 47m. 45·2s., instead of 2h. 42m. 45·2s. The star is really No. 788 of Weisse's Bessel. The following star is also thrown out by a similar error in the time of transit, so that Lalande, 5,520, requires a correction of – 5m. in R.A. With regard to No. 45,400, the suspicion of variability is probably occasioned by a misprint in the "Histoire Céleste," since Piazzi, Bessel, and others estimate the magnitude the same as at present.

THE ANNULAR ECLIPSE OF JANUARY 22, 1879.—The first of the annular eclipses of the ensuing year, a return of that of January 10, 1861, which was central in Australia, commences in Uruguay, whence the belt of annular phase traverses the South Atlantic, passing over Tristan d'Acunha, the few inhabitants of which islands may probably be startled by seeing the sun transformed into a narrow luminous ring while he is high in their heavens. The central line crosses the African continent in the direction of Pemba Island, north of Zanzibar, where the annularity will continue nearly three minutes: the middle of the eclipse at 4h. 6m. P.M., local mean time.

GEOGRAPHICAL NOTES

THE Council of the Royal Geographical Society have determined to commence, on January 1, 1879, the monthly issue of a new series of their *Proceedings*, under the title of *The Proceedings of the Royal Geographical Society and Monthly Record of Geography*. The latter part of this title will, we believe, fairly indicate the nature of the contents, which will include the papers read at the evening meetings, original articles, geographical notes, obituary notices, proceedings of geographical societies, and brief analytical notes on new books and maps. Each monthly number will be illustrated with one or more maps, and no doubt in this respect endeavours will be made to meet the wants of a public larger than that contained within the ranks of this popular society, which now numbers some 3,400 members.

IN connection with this we regret to announce that this month's number of the *Geographical Magazine* brings the career of that journal to a close. This regret, however, is considerably mitigated by the fact that the new form of the Geographical Society's organ is intended to take the place of the journal which for so many years has been so ably conducted by Mr. C. R. Markham. Mr. Markham deserves great credit for his disinterestedness in continuing to carry on a journal which aimed so successfully to be the organ of scientific geography in this country. The volumes will form a valuable record of the progress of geography for the period over which they extend. In the December number Mr. Markham gives an account of the career of the journal since its first start as *Ocean Highways* in 1870. We trust that the new organ will prove a worthy successor of its predecessor, and that while giving due prominence to geographical news, the conductors will aim at bringing geography under the guidance of sound scientific principles.

THE Church Missionary Society have recently received from Mr. A. M. Mackay, of their Nyanza Expedition, the journal of his experiences, extending from December 31, 1877, to May 16, 1878, in which occur some useful suggestions to African travellers, as well as information of considerable interest to geographers. On April 30 Mr. Mackay reached Uyui, after a hurried and tedious tramp

of 350 miles, undertaken with the view of aiding the Rev. C. T. Wilson, and he promises a detailed account of his troubles then on another occasion. In the course of this march he had to cross the extensive wilderness of Mgunda Mkali, which he describes as being for the most part not merely a swamp, but this year more under water than above it. Day after day the party waded and splashed through mud and water, now over the ankles, sometimes up to the knee, and here and there up to the waist or higher. Hopeless and still more hopeless, Mr. Mackay remarks, the wading seemed to become, and he found that they were crossing what was neither more nor less than the source at once of the Nile, the Congo, and the Rufiji. This gigantic boggy plain or moss Cameron calls the Nya Kun Swamp, and, where Mr. Mackay crossed it, in S. lat. 5° 20′, his aneroids recorded an average elevation of 4,000 feet exactly. To the north, in Usukuma, the swamp narrows itself into the sluggish Lewumberi River, the most southerly of the sources of the Nile. From this swamp, too, the rapid Mdaburu River takes its rise, and flows southward into the Ruaha, in Unyoro, and Uhehe, and thence to the Indian Ocean, where it is known as the Rufiji. A day's march east of Tura Mr. Mackay found the swamp contract to a breadth of 300 yards, with increased depth, and slight indications of flow to the southward; it then rounds to the west, steering clear of Iiwe-la-Singa, and, after two more days, it was crossed—a flooded, five-armed river, flowing rapidly north at an altitude of 3,700 feet. Here it is called the Nghwàla River by the Wanyamwezi, from the number of partridges on its banks. In Speke's map alone it would seem that the true course of the river is indicated; it flows north-east to Mirambo's country, where it is known as the Ngombe (*i.e.*, ox) River, and finds its way into the Malagarasi, thus aiding that river in bringing Lake Tanganyika nearer to permanent overflowing, when the Lukuga—which has been such a bone of contention to two great travellers—will no longer be a swamp but a decided stream, and the waterline of the Congo will run from Loango on the West Coast to the confines of Ugogo.

THE London Missionary Society have received a letter from the Rev. J. B. Thomson, dated from Ujiji, announcing the safe arrival on August 23 of the main body of the Society's expedition at its destination on Lake Tanganyika. Though, as will be remembered, this expedition met with a long series of disasters and delays in the coast region, they have now been successful in performing one of the quickest and most prosperous journeys from Mpwapwa to Ujiji, having been but seventy-three days on the road. Messrs. Thomson and Hore have already found an apparently healthy site for their station close to Kinegoma Bay, and about three miles from Ujiji.

WE learn from a Japan paper that an American gentleman has been engaged for some time past in surveying the Island of Yezo, as well as in making geological investigations. The result is said to be that there are 7,000 square miles of land fit for agricultural purposes, and 6,000 suitable for pasture, while there are 5,000 square miles of forests and 9,000 of volcanic mountains and mineral country. An impression appears to prevail that the Government wish to encourage emigration to this thinly-populated part of the Japanese empire.

AS an erroneous impression prevails that nothing is known of the scientific work done by the Portuguese African expedition previous to the date mentioned in last week's NATURE, it may be interesting to give, as instances of the service which they are rendering to geography, the positions of some of the places determined by Senhor Serpa Pinto and his colleagues:—Benguela, long. 13° 25′ 20″ 45‴, lat. 12° 34′ 17″, alt. 7 metres; Dombe Grande, long. 13° 7′ 45″, lat. 12° 55″ 12′, alt. 98 metres; Quillenques, long. 14° 5′ 3″, lat. 14° 3′ 10″, alt. 900 metres; Caconda, long. 15° 1′ 51″, lat. 13° 0′ 44″, alt. 1,678 metres; Bihé, long. 16° 49′ 24″, lat. 12° 22′ 40″, alt. 1,670 metres. The longitudes are stated to be chronometrical.

DR. EDWIN R. HEATH, of Wisconsin, is about undertaking the exploration of the Beni and Madre di Dios Rivers of Brazil, his sojourn in South America some years as secretary of legation in Chili giving him excellent advantages for this purpose. Dr. Heath had arranged to visit South America with Prof. Orton, but was detained, and he now desires to carry out some of the work that the untimely death of that well-known explorer has left uncompleted.

THE latest advices from Mr. Frederick A. Ober, of whose explorations in the West Indies on account of the Smithsonian Institution we have given notice from time to time, were from Point à Pitre, Guadeloupe, on September 23. He was about to leave for the United States, expecting to arrive some time between the middle and end of October. Since his last report he has obtained quite a number of additional collections, and hoped to complete the material for the proposed catalogue of the birds of the West Indies. So far the collections sent forward by him to the Smithsonian Institution have been found to contain some seventeen undescribed species of birds, as determined by Mr. George N. Lawrence, of New York.

AN interesting account of a recent visit to Pitcairn Island by Admiral De Horsey in the *Shah*, forwarded to the Admiralty, will be found in yesterday's *Daily News*. The people are evidently as primitive and well-conducted, and on the whole as comfortable as ever.

IN a previous number we referred to a work of great geographical interest—"Die Sahara, von Oase zu Oase," by Dr. Joseph Chavanne, published by Hartleben, of Vienna. At that time the work was in course of publication, and we refer to it now to announce its completion in twenty parts. The last parts are in every way equal to the earlier ones, and if anything the interest is rather increased than otherwise. The work contains numerous woodcuts, besides seven coloured plates and a map of the great desert; its perusal will be found extremely attractive by any one taking interest in geographical science. The exact route which Dr. Chavanne describes is the following:—The travellers start from Tripolis through the Fezzan to Mursuk, then westward to Rhat, the land of the Tuareg or Imoshag. From Rhat they turn northward to Rhadames, thence to Biskra in Algeria. Here the travellers again turn their backs to the Mediterranean and proceed in a south-westerly direction by way of El Aruat and El Golea to Insalah. From Insalah they go to Tafilet, in the extreme north-west of the desert, and thence many thousand miles to the south to Timbuctoo. The Oasis of Air or Asben is the next station, situated due east from Timbuctoo, then Tibesti, the land of the Tebbu. Thence they turn to the northeast to the Jupiter Ammon Oasis, which is the furthest point to the east reached. The travellers then turn westward again and return to Tripolis by way of Audschila.

ON THE DEVELOPMENT OF THE GARPIKE

THE gar or bony pike of North America is one of the most interesting of living fishes. The best known species of the genus to which it belongs is the *Lepidosteus osseus*. This species owes the grammatical form of its scientific name, and, indeed, its first scientific description to the elder Agassiz, and we have now to record the filling up of the last details of its life-history to the younger Agassiz. Known for over three-quarters of a century, it has been only within the last few months that the young fish as they escape from the egg have been

seen, and it has been the good fortune of Alexander Agassiz to succeed in hatching the eggs and raising the young until they showed at least the principal structural peculiarities of the adult. A short account of the chief facts in connection with this stage of the bony pike's history will appear in the forthcoming number of the *Proceedings* of the American Academy of Arts and Sciences; from an advance copy we cull the following details:—The spawning-ground selected for observation was the Black Lake, at Ogdensburgh, N.Y. Mr. Garman, who describes the scene, and Mr. Blodgett, who rendered most essential assistance, deserve the thanks of every naturalist. The eggs collected were carried by the hand in pails from Ogdensburgh to Cambridge, where their progress was watched by Prof. A. Agassiz.

The fish began to spawn about May 18. Little projections of granite stand out here and there into the lake. The frosts from time to time have broken off from these, small angular blocks, which lie piled together under the water at depths varying from two to fourteen inches. Into these shallows the female fish would come, each of them attended by two males. While very timid when in deep water, they seemed to be courageous to recklessness when they approached the shallows. On they would come in threes, when rising to the surface of the water, and thrusting their bill out of it they would open this widely, then take in air, and close it with a snap. In some few cases three or four males would be in attendance on one female, but much more often there would be but two, and these would swim resting on either side of the female fish, their bills reaching up toward the back of her head. At times the water would be lashed into all directions with their conjoined convulsive movements. The eggs when laid were excessively sticky; to whatever they happened to touch they stuck, and so tenaciously, that it was next to impossible to release them without tearing away a portion of their envelopes. It is remarkable that, as far as could be seen, there was, on and about the spawning ground, a complete absence of anything that might serve as food for the young fish.

Of the quantity of eggs brought to Cambridge, only thirty hatched, and not one of those artificially fecundated was hatched. In Prof. A. Agassiz' anxiety not to spoil this interesting experiment he did not venture to examine any of the fresh eggs; so that the history of their segmentation and very early development remains to be worked out. The envelope of the eggs is very opaque and of a yellowish green, like that of toads. Of the thirty hatched out by the end of May, twenty-eight were alive in the middle of July last. When first hatched the young fish possesses a gigantic yolk-bag, and the posterior part of the body presented nothing specially different from the general appearance of any ordinary bony (teleostean) fish of the same age; but the anterior part was most extraordinary: it looked like a huge mouth cavity, extending nearly to the gill opening, and crowned by a depression like a horse's hoof in outline, along the margin of which were a row of protuberances acting as suckers. The moment the young fish was hatched it attached itself to the sides of the vessel by means of these, and would hang immovable. The eye was not very advanced, the body was transparent, the gill covers were pressed against the sides of the body; the tail was slightly rounded, the embryonic fin is narrow, and there were no traces of embryonic fin rays; the olfactory lobes were greatly developed and elongated as in sharks and skates; the chorda was straight. On the third day the body became covered with minute black pigment cells, and then was noted the first traces of the pectoral fins, and the snout became more elongated; the great yolk-bag was greatly reduced in size. About the fifth day were seen traces of the caudal, dorsal, and anal fins. Gradually the snout became elongated, the suckers concentrated, and the disproportionate size of the sucking disc became reduced, so that when about three weeks old it became altogether more fish-like. The sucking disc was now reduced to a swelling at the top of the upper jaw, the yolk-bag had disappeared, the gill covers extended well up to the base of the pectorals—these latter were in constant motion, and the tail exhibited the same rapid vibratile movements. The young fish now begins to swim about, and is not so dependent upon its sucking disc, and at last this only remains as a fleshy globular termination on the snout. At this stage, too, the young have the peculiar habit of the adult fish of coming to the surface to swallow air. When they go through the process under water of expiring this air they open their jaws wide and spread their gill-cover, and swallow as if they were choking, making violent efforts, until a minute bubble of air has become liberated, when they become quiet again. Their growth is rapid. Within a month the teeth made their appearance, and some of the fin-rays on the fringe of the pectorals were to be seen.

Prof. A. Agassiz draws the following conclusions from these observations:—"That notwithstanding its similarity in certain stages of its growth to the sturgeon, notwithstanding its affinity with sharks by the formation of its pectorals from a lateral fold, as well as by the mode of growth of the gill openings and gill arches, the Lepidosteus is not at all so far removed as is generally supposed from the bony fishes." The memoir is illustrated by five plates containing some forty-five figures, and is only to be regarded as a preliminary account, but it is a preliminary account of such exactness, importance, and interest, that no apology is necessary for bringing it at once under the notice of our readers. This memoir was presented to the American Academy as recently as October 8 last. E. PERCEVAL WRIGHT

THE MUSIC OF COLOUR AND MOTION

AT the Physical Society, on November 23, 1878, Prof. W. E. Ayrton, late of the Imperial Engineering College, Tokio, Japan, read a paper, written by himself and Prof. J. Perry, of the same college, on "The Music of Colour and of Visible Motion." The authors began by pointing out the well-known fact that emotion is excited by moving bodies, and they believed that, upon this basis, a new emotional art would be created which would receive a high development in the far distant future. All methods of exciting emotion could be cultivated; but of these, music, by reason of the facility with which its effects could be produced, had alone been highly perfected by the bulk of mankind. Sculpture and painting are not purely emotional arts, like music, inasmuch as they involve thought. It would take a long time and much culture for the eye to behold moving figures with similar emotional results to those of the ear on hearing sweet sounds; but time and culture only might be necessary. It might be due to their neglect of this emotional tendency that the Western nations felt little emotion at moving visual displays. For among the Eastern nations they had entertainments consisting of motions and dumb show, which, although incomprehensible and even ludicrous to the European, powerfully affected the feelings of a native audience. In Japan the authors had seen whole operas of "melodious motion" performed in the theatres, the emotions being expressed by movements of the body, affecting to the audience, which were quite strange to them. The accompanying orchestral music was, withal, displeasing to the authors, while, on the other hand, Western music is mostly displeasing to the Japanese.

The emotions produced by rapidly-moving masses, such as a train bowling up to a bridge, or by changing colours, as in sunsets, have been felt by all, and those excited when the moving bodies are very large do not

seem to be producible by anything else in nature. Harmonic instruments have been constructed to exhibit the combination of two or more pairs of harmonic motions to the eye; for example, Blackburn's pendulum, Lissajous' forks, Wheatstone's kaleidophone, Yeates' vibrating prisms, Donkin's and Tisley's harmonographs, and Hopkins' electric diapason. Prof. Ayrton illustrated his remarks by exhibiting these instruments in action. The pendulum traced out the complex path of the combined motions by a jet of falling sand, the forks or prisms by a moving beam of light thrown on a screen, the kaleidophone by a bright bead, and the harmonograph by the involutions of an aniline pen. With none of these and such like instruments, however, is the production of mere emotion the end in view; and in some of them no change can be made in the periods of the pairs of harmonic or periodic motions combined without arresting the instrument, a proceeding which in music would be analogous to stopping the tune at the end of every chord. There is no provision either for changing the amplitude or phase, equivalent in music to an inability to render, at will, a note forte or piano, or rather as it is not only the strength of the entire note, but even the amplitude of the various component harmonics that these instruments cannot regulate, it would be as if in music there was the probability of a note marked in the score as piano for the flute being rendered by a loud blast from a trumpet. A successful instrument in the new kinematical art must at least visibly render changes in period, amplitude, and phase of the harmonic motions represented. Profs. Perry and Ayrton had designed an instrument, which is now in Japan, for effecting these required changes in a combination of harmonic motions given to a moving body, and which they claimed to be the first musical instrument of the visual art in question. They had not given it a name yet, because the nomenclature of the subject was uninvented. Photographs and diagrams of this instrument were exhibited to the meeting. It consists of a mechanical arrangement of sliders, pulleys, and cords, whereby two motions, one along a vertical, and the other along a horizontal line, and *each* consisting of the *sum* of a number of harmonic motions the *period*, *amplitude*, or *phase* of *any one* of which can be varied at will, are compounded in the resultant motion of a suspended pane of glass. A black circle painted on the pane is intended to represent the moving body as projected against a wall or screen behind. The sliders controlling the motion of the pane are actuated by a revolving barrel, the periphery of which is carved according to mathematical principles, so as to give the different harmonic motions to the sliders in one revolution. The motion is further regulated by shifting the sliders either parallel to the axis or at right angles to the radius of the revolving barrel; and by the angular velocity of the barrel. In this way the period, amplitude, and phase of the component motions of the glass either in a vertical or horizontal direction, may be changed at will, and almost immediately. Other kinds of periodic motions may be compounded in a similar way. Prof. Ayrton also suggested other forms of apparatus for this purpose. Numberless combinations of graceful motions producing emotional effects on the beholder can by its means be given to a visible body. It is the intention of the authors to construct an improved form of the apparatus, and to arrange for the blending of colour with the moving body to heighten the emotional influence; for example, they purpose having changing mosaics of different hues, thrown upon the screen for a background to the black spot. This can be done by means of an instrument similar to the chromotrope with its revolving sheets of parti-coloured glass. In conclusion Prof. Ayrton said that there might yet be invented many different ways of producing these spectacles, and there was no reason why a whole city full of people should not enjoy these displays projected upon the clouds overhead.

THE SWEDISH NORTH-EAST PASSAGE EXPEDITION

FROM letters despatched from the mouth of the Lena by Prof. Nordenskjöld on August 27, which have just been published in the Gothenburg *Handels Tidning*, we learn that the *Vega* accompanied by the *Lena* left Dickson Harbour, at the mouth of the Yenissej, on August 10, the weather being fine. On the 11th ice was seen, but it consisted almost exclusively of bay ice which did not obstruct navigation, which, however, was rendered difficult by a thick fog. The salinity of the water began gradually to increase and its temperature to fall. Organic life at the bottom grew richer at the same time, so that Dr. Stuxberg on the night between August 13 and 14, while the vessel lay anchored to a drift-ice floe, collected with the swab a large number of beautiful pure marine types; for example, large specimens of the remarkable crinoid, *Alecto eschrichtii*, numerous asterids (*Asterias linckii* and *panopla*), pycnogonids, &c. The dredgings near the land now too began to yield to Dr. Kjellman several large marine algæ. On the other hand the higher plant and animal life on land was still so poor that the coast here forms a complete desert in comparison with the rocky shores of Spitzbergen or West Novaya Zemlya. Auks, rotges, loons, and terns, which are met with on Spitzbergen in thousands upon thousands, are here almost completely absent. Gulls and *Lestris* which there fill the air with continual sound occur here only sparingly, each with two species, and it appears as if they quarrelled less with one another. Only the snow-bunting, six or seven species of waders, and a few varieties of geese are found on land in any great numbers. If we add a ptarmigan or two, a snowy owl, and a species of falcon, we have enumerated the whole bird fauna of the region, at least so far as the Swedish expedition have been able to ascertain it. Of warm-blooded animals in the neighbouring sea, only two walruses and some seals, *Phoca barbata* and *hispida*, were met with. There is probably great abundance of fish. Cosmic dust was sought for on the ice without success, but there was found upon it some yellow specks which, on examination, were found to be a coarse-grained sand, consisting exclusively of very beautifully-formed crystals up to two millimetres in diameter. The nature of these crystals was not ascertained, but it was evident that they are not formed of any ordinary terrestrial mineral, but possibly of some substance crystallised out of the sea-water during the severe cold of winter.

The *Vega* lay at anchor from August 14 to 18 in a harbour named Actinia Harbour, from the number of these animals brought up by the dredge from the sea-bottom. This harbour is situated in a sound between Taimyr Island and the mainland. The land was free of snow, and covered with a greyish-green turf formed of a close mixture of grasses, mosses, and lichens, forming a reindeer pasture much superior to that of the valleys in Spitzbergen which abound in reindeer. Only a few reindeer, however, were seen here, probably owing to the presence of wolves. The number of phanerogamous plants is exceedingly small; the moss and especially the lichen vegetation, on the other hand, abundant enough. Actinia Harbour is an excellent position for a meteorological station. The fog still continuing, the *Vega* and the *Lena* sailed again on the 18th, and reached Cape Chelyuskin on the 19th, anchoring in a little bay which indents the low promontory, dividing it into two parts. The western point was found to be situated in 77° 36′ 37″ N. L., and 103° 25½′ E. from Greenwich, and the eastern in 77° 41′ N. L., and 104° 1′ E. L. Inland the mountains appeared to rise by degrees to a height of 1,000 feet. These mountains, as well as the plains, were free of snow. Only here and there were to be seen large white patches of snow in hollows on the mountain sides or in some small depression on the plains. At the beach, however, the ice-foot still remained at most places.

The soil of the plains is clayey, partly bare, and cracked into more or less regular hexagonal figures, partly covered by a turf of grass, mosses, and lichens resembling that at the last landing-place. The rock here, however, was not granite, but upright unfossiliferous schistose strata rich in crystals of sulphide of iron, and crossed at the extremity of the cape by thick quartz veins. Dr. Kjellman could not find here more than twenty-four species of phanerogamous plants, most of them distinguished by a disposition to form compact half-globular tufts. The lichen vegetation was also, according to Dr. Almquist, monotonous, though luxuriantly developed. It almost appeared as if the plants of the Chelyuskin peninsula had attempted to wander farther toward the north, but halted when they met the sea, at the very outermost point. For here were found within a very limited space nearly all the plants, both phanerogams and cryptogams, which the land had to offer, and many of them were sought for in vain farther up on the plains.

Animal life on land rivalled the higher plant life in poverty. Of birds there were seen only a number of *Phalaropus*, some species of *Tringa*, a *Colymbus arcticus*, a very numerous flock of *Anser bernicla*, a few eiders, and the remains of a snowy owl. In the neighbouring sea, which was almost free of ice, were seen a single walrus, two shoals of white whales and some few seals (*Phoca hispida*). It, too, was here evidently very poor in warm-blooded animals. On the other hand the dredge brought up from the sea-bottom various large algæ (*Laminaria agardhi*, &c.), and a large number of lower animals, among them very large specimens of *Idothea entomon*, an isopod, which also occurs in the Baltic and the large Swedish lakes, and is looked upon as an evidence that during the ice age they were connected with the Polar Sea. The algæ obtained were interesting as affording further proof of the incorrectness of the view which long prevailed, that the Siberian Polar Sea was quite devoid of the higher algæ.

On August 20 the voyage was resumed, the course being set east by south, in the hope of falling in with a continuation of the new Siberian Islands. Drift-ice was soon met with, and by the morning of the 23rd it was found impossible to proceed further in that direction. An attempt was now made by sailing in a northerly and north-westerly direction to get out of the ice-field, and in about twenty-four hours the *Vega* was again in open water, and the same day land was sighted. The land was found to be the north-eastern extremity of the eastern Taimyr Peninsula, lying in about 76° 30′ N. L., and about 113° E. of Greenwich. The sea was completely free of ice for a distance of 15′ or 16′. Fine mountains 2,000 to 3,000 feet high were seen some distance inland. These, like the plains below, were free of snow to their highest summits. Some small glaciers were believed to be seen, but they ended at a height of about 800 to 1,000 feet above the sea.

Animal life now began to be very abundant. Dr. Stuxberg had, while the vessel lay anchored to a floe in the drift-ice-field, brought up from a depth of 35 fathoms an unexpected variety of fine marine animal types, among them three specimens of a crinoid supported on a stalk, probably young individuals of *Alecto eschrichtii*, which also was found in innumerable full-grown specimens, masses of asterids (for instance *Solaster papposus*, *endeca*, *furcifer*, *Pteraster militaris*, *Asterophyton eucnemis*), and of the otherwise exceedingly rare *Molpadia borealis*, a colossal pycnogonid of 180 millimetres diameter, &c. Not less abundant was the lower animal life at a smaller depth though the forms were partly different. The animals occurring here were evidently pure Polar Sea types without any immigration whatever from southern seas, such as has doubtless taken place in the case of the fauna of Spitzbergen.

On August 24 land was sighted, which was found to be Preobraschenski Island, near the mouth of the river Katanga. From this point to the mouth of the Lena the depth was only from 5 to 8 fathoms.

To judge from the experience of the voyage there is no more ice on the Siberian coast during the latter part of summer than in the White Sea during midsummer. Besides the ordinary observations of the temperature of the sea-water at the surface in connection with the common meteorological observations made six times in the twenty-four hours, the temperature and salinity of the water at different depths were determined two or three times a day. When the depth amounts to at least 30 metres the temperature at the bottom is found to vary between $-1{\cdot}0°$ and $-1{\cdot}4°$ C. The specific gravity of the water amounts there to 1·026—1·027, the salinity being little less than that of the water of the Atlantic Ocean. On the surface the temperature is exceedingly variable:—At Dickson Harbour $+10°$, a little south of Taimyr Sound $+5{\cdot}4°$, among the drift ice off that sound $+0{\cdot}8°$, off Taimyr Bay $+3{\cdot}0°$, at Cape Chelyuskin $+0{\cdot}1°$, off Katanga Bay $+4{\cdot}0°$, between Katanga and Lena $+1{\cdot}2°$ to $+5{\cdot}8°$. The specific gravity of the surface-water in a broad channel along the coast never exceeded 1·023, in general only amounted to 1·01 or under. The latter figure corresponds to a mixture of about one part sea water with two parts river water. This shows indisputably that a warm surface current of little salinity from the mouths of the Obi and the Yenissei runs first along the coast towards the north-east, and then under the influence of the rotation of the earth in an easterly direction. Other similar currents originate from the Katanga, Anabor, Olonek, Lena, Jana, Indigirka, and Kolyma, all which pour their waters, more or less heated during the hot summer of Siberia, into the Polar Sea and render it, during a short period of the year, almost free of ice.

On the night between August 27 and 28 the *Vega* parted from the *Lena* off the mouth of the River Lena. There is scarcely any hope now that the voyage will be completed before next summer. No doubt the *Vega* has got into a safe winter-harbour, and that during the detention of the expedition a rich harvest of scientific results will be gathered.

THE FORMATION OF MOUNTAINS

PROF. ALPHONSE FAVRE, of Geneva, has been making an interesting series of experiments to illustrate the formation of the great inequalities of the earth's surface by means of lateral thrust or crushing. These he describes and illustrates in a recent number of *La Nature*, to which we are indebted for the illustrations which accompany this article. Prof. Favre refers to the early experiments of Sir James Hall with various kinds of cloth, which he made to assume a variety of shapes by means of weights. He speaks of the various theories of the elevation of mountains, and especially of that of H. B. de Saussure, whose term *refoulement* seems to have meant much the same as that used by M. Favre, *écrasement latéral*.

The three systems, M. Favre says, which account for the origin of mountains by forces which push the great mineral masses from below upwards, from above downwards, or laterally, do not differ so much from each other as at first sight appears. Those geologists who have admitted the system of elevations as the principal cause of modification of the surface of the globe, would probably enough admit the formation of depressions as a secondary modification; and so those who have accounted for these modifications mainly by depression, would probably enough also admit elevation as a secondary factor. Again, in the system of lateral crushing, there is a general depression of the surface of the earth, since there is a diminu-

tion in the length of the radius of our globe, and yet there result elevations of the ground in the midst of this general depression.

The cause of lateral crushing, M. Favre goes on to say, is owing to the cooling of the earth. It is, in fact, very probable that our globe is at the stage when, according to Élie de Beaumont, "the mean annual cooling of the mass exceeds that of the surface, and exceeds it more and more." It must follow that the external strata of the globe, tending always to rest on the internal parts, are wrinkled, folded, dislocated, depressed at certain points, and elevated at others.

"The experiments," M. Favre continues, "which I have made at the works of the Geneva Society for the manufacture of physical instruments, resemble much those of Sir James Hall; they differ notably, however, in two points:—1. The celebrated Scotchman caused the matter which he wished to compress to rest on a body which itself could not be compressed, while I placed the layer of clay employed in these experiments on a sheet of caoutchouc, tightly stretched, to which I made it adhere as much as possible; then I allowed the caoutchouc to resume its original dimensions. By its contraction the caoutchouc would act equally on all points of the lower part of the clay, and more or less on all the mass in the direction of the lateral thrust. 2. Hall compressed, by a weight, the upper surface of the body which he wished to wrinkle, which prevented any deformation, while by leaving that surface free, I have seen, during the experiment, forms appear similar to those of hills and mountains which may be observed in various countries."

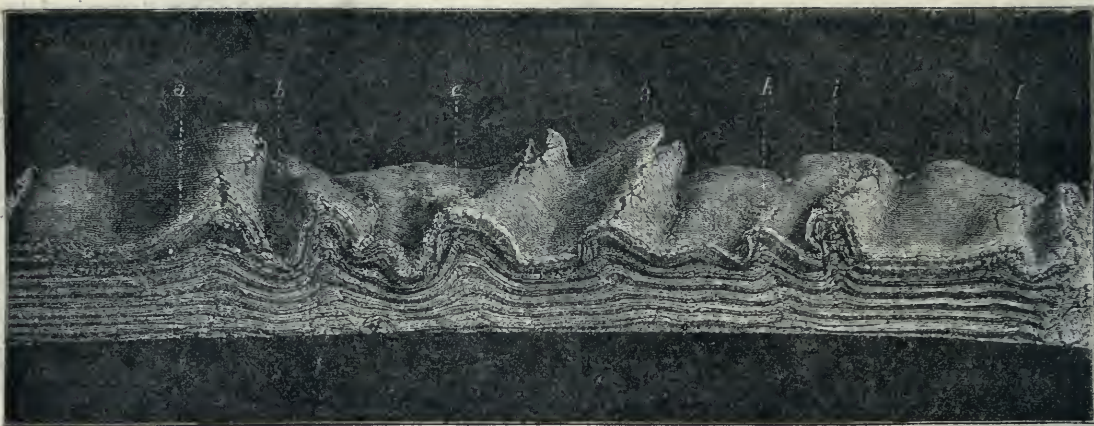


FIG. 1.

"The arrangement of the apparatus is very simple. A sheet of india-rubber 16 mm. in thickness, 12 cm. broad, and 40 cm. long, was stretched, in most of the experiments, to a length of 60 cm. This was covered with a layer of potter's clay in a pasty condition, the thickness of which varied, according to the experiments, from 25 to 60 mm. It will be seen from the dimensions indicated that pressure would diminish the length of the band of clay by one-third. This pressure has been exerted on certain mountains of Savoy. For example, the section which I have given [1] of the mountains situated between the Pointe-Percée and the neighbourhood of Bonneville enables it to be seen that those folded and contorted strata which are shown between Dessy and the Cal du Grand Barnaud cover a length which is two-thirds of that which they had before compression. These mountains, then, have been subjected, like the potter's clay, to a compression indicated by the ratio of 60 to 40. Contortions are not, perhaps, observed over all the surface of the globe; it has not been equally folded in all its extent, but they are found in a great number of countries, and even beneath strata almost horizontal. Sometimes the folds approach

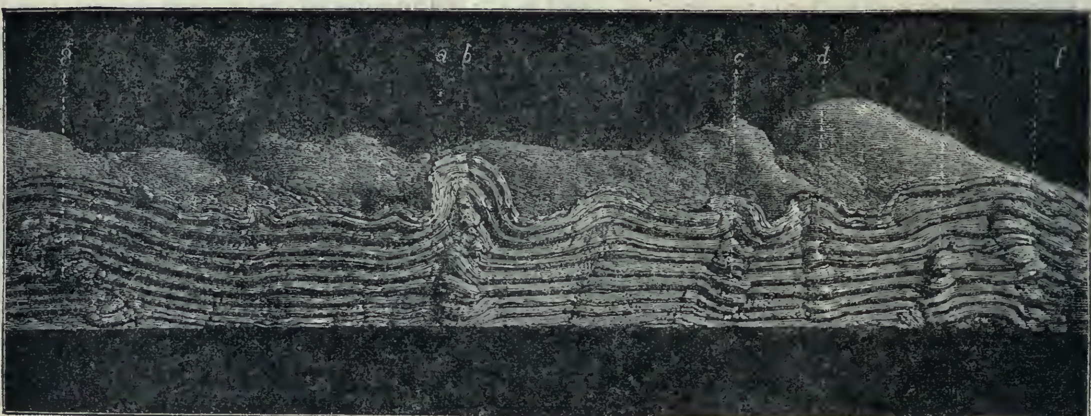


FIG. 2.

[1] *Bullet. Société Géologique de France*, 1875, t. iii. pl. xxii. A. Favre, *Recherches Géologiques*, Atlas, pl. ix.

the vertical, and are close against each other; this structure indicates that pressure has been exercised in a stronger manner than I have indicated.

"These powerful lateral thrusts of the external and solid parts of the globe appear to result from a diminution which the radius of the interior pasty or fluid nucleus has undergone during millions of ages. It may have been sufficiently great to cause the solid crust (which must always have been supported on the interior nucleus, whose volume continually diminishes) to assume the forms which we know, with a slowness equal to that of the contraction of the radius.

"To return to my experiments. At the extremities of the band of clay are pieces of wood or supports, which accompany it in its movement of contraction. The clay is thus compressed at once by its adhesion to the caoutchouc and by lateral pressure of the supports. By the influence of the caoutchouc alone, without the presence

Fig. 3.

of the supports, there are formed only slight wrinkles on the surface of a sheet of clay 3 or 4 ctm. in thickness; and if the supports alone compressed the clay placed on a material which is not compressed (a very smooth oiled plate), the clay scarcely wrinkles near the centre of its surface; it increases a little in thickness and forms swellings (*bourrelets*) against the supports. The strata which appear to divide the masses of clay, and which are represented in the figures, are not really strata, but simply horizontal lines at the surface of the clay.

Such pressure as has been applied in these experiments produces contortions of strata which elevate the surface of the matter compressed, as well in the plane parts or plains, as in those which take the forms of valleys, hills, or mountains. These latter have the appearance of vaults or folds, sometimes perpendicular, sometimes warped (*déjétes*); the ridges are complete.

Fig. 4.

or broken at the summit by a longitudinal fracture, narrow below and wide above; next, another fracture, narrow above and wide below, is produced at the base of the mountain or vault. The sides of valleys are sometimes almost vertical, sometimes present gentle slopes. The strata are less strongly contorted in the lower parts than in the neighbourhood of the upper surface. They are disjoined in certain parts by fissures or caverns; they are traversed by clefts or faults inclined or vertical. All these deformations are the more varied in that they are not similar on the opposite sides of the same band of clay.

Most of these phenomena are seen in Fig. 1, which represents the result of an experiment made on a band of clay, whose thickness, before compression, was about 25 mm., while after that it attained 62 mm. at the culminating point. At *a* is seen a vault a little broken at the summit, covering a cavern similar to that figured in

the memoir of Sir J. Hall (*Trans.* R. S. E., vol. vii. 1813), and to that of the Petit Bornand in Savoy (Favre, *Recherches*, pl. x.); at *b* is a valley open at one of its ends and almost closed at the other; at *e* is a vault almost straight, the prolongation of which is very level; at *g, h*, and *l* are vaults twisted and a little broken, while at *i* is a broken fold, the curves of which are almost vertical. All these accidents of the ground recall those which have been so often observed in the Jura, the Alps, and the Appalachians.

Fig. 2 represents a band of clay whose thickness was about 40 mm. before compression, and 65 after. We remark contortions similar to those of the preceding figure, among others a vault *a*, very exactly formed. At distances are seen vertical slices, on which the pressure appears to have acted in a particularly energetic fashion, and which may be called "zones de refoulement;" the strata are there broken in an exceptional manner, often separated from each other. One of these vaults is replaced by a single fault on the opposite side of the band of clay.

Before compression, in the band of clay in Fig. 3, were seen the two divisions which are seen there now—that in the right was 33 cm. long, and 25 mm. thick at *a*, and 35 at *b*; the left division was 25 cm. long, and 65 mm. thick. A gentle slope united the part *c* to the part *b*. After compression, the mean height of *ab* was 45, and that of *c* 75 mm. All the layers were spread horizontally.

In this experiment I have sought to imitate the effect of crushing at the limit of a mountain and a plain. The height of the mountain *c* has been notably increased, the five or six upper layers have advanced on the side of the plain; they encroach on it. The plain has, however, offered a resistance sufficiently great to cause the strata of the mountain to be strongly inflected at the bottom. From this struggle between the plain and the mountain, there resulted a cushion, *d*, which is the first hill at the foot of the height. It also resulted that the strata of the plain assumed an appearance of depression at contact with the mountain in consequence of the vault which is formed at *b*; they plunge underneath the mountain. This resembles what is often seen in the Alps at the junction of the first calcareous chain and the hills of "mollasse;" in fact, the strata of the latter rock seem to plunge under those of the neighbouring heights. In consequence of the pressure, there are formed several ranges of hills in the plain between *b* and *a*.

"In Fig. 4 the band of clay had, before compression, a thickness of 45 mm.; after that the culminating point was more than 10 cm. I have here sought to represent what must happen when terrestrial pressure is exerted on horizontal strata still moist, deposited at the bottom of a sea where are two mountains already solidified. For this purpose I placed in the caoutchouc and under the clay two bare cylinders of wood, *a* and *b*, of about 35 mm. radius, at 20 cm. from the ends of the band of clay, and at the same distance from each other. Before compression, the surface of the clay and the strata were completely horizontal. Pressure gave rise at the top of the half-cylinder, *a*, to a valley, *c*, formed by a twisting of the beds to the right, and by a little mountain, *d*, to the left. But I do not believe that it has ever been thought to assign to a valley an origin of this nature.

"On the other semi-cylinder, *b*, is produced an enormous elevation which has carried the ground to *e*, with such a rupture that the left lip, *f,g*, has suffered a complete reversal by turning, as on a hinge, around the horizontal line which passes by the point *h*. It follows that the four upper strata of clay designated by the figures 1, 2, 3, 4, being in a normal position before compression, are, after that, so arranged as to show the succession represented by the following arrangement of figures:—1, 2, 3, 4, 4, 3, 2, 1, 1, 2, 3, 4, making the section of this formation by a line drawn from *x* to *z*. If the left lip should disappear we should then have between the points *x* and *z* the section 1, 2, 3, 4, 5, 1, 2, 3, 4, 5. Sections analogous to these, presenting inversions in the order of strata, are known to geologists.

"The forms assumed by the clay depend on several circumstances which it is difficult to describe, such as the strength and the rate of compression, the thickness and the greater or less plasticity of the clay, &c. Why have accidents of the upper surface of the clay, which are intimately connected with those of the interior of the mass, so small an extension that they are not even similar in the two sides of a band of clay? This small continuity is owing to causes which we can neither foresee nor appreciate. Is it not the same in nature? Why is the chain of the Alps not a true chain, but a succession of masses often oblique with respect to each other? Why, in the Jura, do we see chains which have for their prolongation plains and valleys? It is always the case that the forms and structures obtained in these experiments have an incredible resemblance to those which are found on the surface of the globe. But it must be admitted that many of the latter have not been reproduced by these artificial crushings.

"It appears probable that, by pressures more powerful and more variedly employed, we might obtain again very different structures. But I have not thought it necessary to multiply these experiments, thinking that the varied forms which have resulted show sufficiently the effects of crushing."

GEORGE HENRY LEWES

THIS is a name which has been long before the reading public of England, and the announcement of Mr. Lewes's death, on Saturday last, at the age of sixty-one years, will be received by very many with genuine regret. This will be especially the case with those who have reached or passed middle life, for latterly Mr. Lewes's name has come little before the public, and what work he has done appeals to a comparatively small circle. Of Mr. Lewes's many-sidedness every one knows; he commenced his career as a novelist, and ended as a physiological psychologist—perhaps in some respects no very great leap, after all; indeed the two functions may be said to be combined in that greatest of philosophical novelists, if not of novelists absolutely, "George Eliot," Mr. Lewes's widow. Science owes a good deal to Mr. Lewes; for, though he made little or no pretension to be an original investigator in physical science, he did very much by his writings to give the general public an idea of what real science is, and to help forward the good work of carrying it into every-day life. His "Physiology of Common Life" had a long and deserved popularity, and even yet, we believe, is often "asked for" at libraries and book-shops. His "Biographical History of Philosophy" is thoroughly readable and full of information, which is more than can be said of philosophical works generally. Of his "Life of Goethe," one of the very few masterly biographies, we leave it to others to speak, though he did much there to bring out the real importance of Goethe's botanical and other scientific researches. Of his latest work, "Problems of Life and Mind," we spoke at length on the appearance of the volumes that have been published; in these volumes and in one or two letters and articles contributed to our pages, Mr. Lewes was perhaps at his best as an investigator in a department of science with which we are cautious of interfering, but which has a strange fascination for many thinkers. Altogether Mr. Lewes filled an important and many-sided place in the intellectual life of this country during his long career. It is easy to say that a man of his unusual keenness of mind might have achieved permanent greatness by concentrating his great store of energy in one

particular direction; but then he would not have been the innately versatile George Henry Lewes. His influence has been spread over a wide field, and has been largely beneficial to progress and enlightenment; he never aimed, we believe, at piling up an enduring monument to himself.

NOTES

MR. E. J. STONE, F.R.S., Astronomer-Royal at the Cape of Good Hope, has been appointed to the Radcliffe Observatory, Oxford, in place of the late Rev. R. Main.

MR. WILLIAM SPOTTISWOODE, having been elected President of the Royal Society on Saturday last, has resigned the office of Secretary to the Royal Institution. At the meeting, on Monday last, it was proposed that the Members of the Royal Institution subscribe to present a bust of Mr. Spottiswoode to the Institution as a recognition of his valuable services as Treasurer and Secretary successively.

WE have already referred to the new Parkes Museum of Hygiene, at University College, London, and we now earnestly draw our readers' attention to the appeal made by the Executive Committee for subscriptions towards an endowment, which is absolutely necessary for the efficiency of the institution, in diffusing the much needed knowledge of sanitary appliances and their uses. Although quite in its infancy, the Parkes Museum contains objects relating to life-protection, dietetics, clothing, furnishing, engineering, and architecture—in fact, every branch of hygiene. The library already consists of between 300 and 400 volumes, exclusive of pamphlets. "It cannot be too widely known," the Executive Committee state, "that it is intended to extend the benefits of the Museum to all classes, so that not only professional men, but owners of property, employers of labour, artisans, and others, both men and women, may be able to study at their leisure the subjects in which they are most interested." The Executive Committee, therefore, confidently appeal for pecuniary support to all those who, while being interested in technical education and sanitary science, have the inclination and the means to give such assistance. The Committee will not only be glad to receive subscriptions of money, but also books and pamphlets in any language, statistical tables, maps, plans, and other drawings, models, apparatus, or specimens illustrating any branch of hygiene. Subscriptions may be paid to the Treasurer, Mr. Berkeley Hill, 55, Wimpole Street, W. All communications relating to the presentation of articles to the Museum should be addressed to the Curator, Mr. Mark H. Judge, Parkes Museum, University College, Gower Street, W.C.

THE *Daily News'* New York correspondent telegraphs that Mr. Edison announces that he has perfected a machine for measuring the current used in the electric light. It consists of an apparatus placed in every house lighted by electricity, which registers the quantity of electricity consumed, and uses for the purpose the one-thousandth part of the quantity consumed in the building. Mr. Edison declares that his invention of the light, including the arrangement for counteracting loss in subdivision, is now completed. His experiments at present are directed to reducing the cost. He has, he says, already brought this decidedly below the cost of gas, and as soon as the minimum is reached, will make the results public. The *New York World* contains the following interesting details of Mr. Edison's doings:—"Dozens of workmen and machinists are hard at work at Menlo Park on the new buildings, the workshop being now almost ready for the roof. Mr. Edison said to a reporter for the *World*, 'I don't know when I am going to stop making improvements on the electric light. I've just got another one that I found by accident. I was experimenting with one of my burners when I dropped a screw-driver on to it. Instantly the light was almost doubled and continued to burn with the increased power. I examined the burner and found it had been knocked out of shape. I restored it to its original form, and the light was decreased. Now, I make all my burners in the form accidentally given to that one by the screw-driver. The result is that I can produce the amount of light given out by the first burner with little more than half the power. It is almost impossible to calculate with certainty the resources of my light, but I have engaged a mathematician to work out the problem from my data.' On the whole, Mr. Edison states he is confident of success, however much any one may be puzzled by his methods or claims."

THE Corporation of Liverpool have given notice that they intend to apply in the ensuing Session for an Act authorising the lighting of the public streets, places, and buildings within the borough by means of the electric light, "or otherwise than by means of gas." The Corporations of Warrington, Derby, South Shields, Leicester, Blackburn, Over Darwen, and Stratford-upon-Avon, in conjunction, in the latter borough, with the Local Board of Health, ask for similar powers in the Bills which they intend to promote.

THE sixth part of the illustrated work of C. J. Maynard on the "Birds of Florida, and the Water and Game Birds of Eastern North America," has just been published, and contains three quarto plates, one of them representing sixty-six pecies of eggs.

MESSRS. MACMILLAN AND CO. will shortly publish "Notes of a Naturalist on Board the *Challenger*," by Mr. H. N. Moseley, F.R.S., who was on the scientific staff of the expedition. The work will be illustrated.

THE zoological station of the Zoological Society of the Netherlands has published its third report. During the summer of 1878 the station was erected on the Island of Terschelling, and in the course of two months it was visited by ten zoologists. This year the investigation of the Zuider-Zee was the principal object kept in view, and for that purpose some fourteen dredging excursions with the boat stationed in West-Terschelling for laying buoys were organised. The station underwent no small improvement, a nicely-organised aquarium-room being added to the main building. Here a small hot-air engine of about $\frac{1}{10}$-horse power (construction of D. W. van Rennes, Utrecht) drives an air-forcing pump; the compressed air gathered in a white-iron box is distributed through numerous aquarium-vessels by means of gum-elastic tubes and small glass-canules. By means of this arrangement even on hot days numerous animals were kept alive. The investigation of the Zuider-Zee not being brought to a close, the Island of Terschelling will probably next year see the station again erected in one of its picturesque valleys.

AT last week's meeting of the Paris Academy M. Pasteur read a critical examination of the posthumous papers of Claude Bernard, in which statements were made opposed to the conclusions reached by M. Pasteur. He regards the manuscript of Bernard as a sterile attempt to substitute for well-established facts the deductions of an ephemeral system. "The errors, however," M. Pasteur says, "of those who in the sciences have accomplished a valiant career have only the philosophical interest which attaches to the knowledge of our human frailty."

THE following are the probable arrangements of the Royal Institution for the Friday Evening Meetings before Easter, 1879:—January 17, Prof. Tyndall, "The Electric Light;" January 24, Prof. W. E. Ayrton, "The Mirror of Japan and its Magic Quality;" January 31, Mr. H. Heathcote Statham,

"The Logic of Architectural Design;" February 7, Rev. H. R. Haweis, "Bells;" February 14, Prof. G. Johnstone Stoney, "The Story of the November Meteors;" February 21, Prof. Roscoe, "A New Chemical Industry;" February 28, Sir Wm. Thomson, "The Sorting Demon of Maxwell;" March 7, Prof. Huxley, LL.D., F.R.S.; March 14, Mr. E. B. Tylor, "The History of Games;" March 21, Prof. Abel, C.B., F.R.S., "Recent Contributions to the History of Detonating Agents;" March 28, Sir Henry C. Rawlinson, K.C.B., D.C.L., "The Geography of the Oxus, and the Changes of its Course at different Periods of History;" April 4, Mr. Warren de la Rue, D.C.L., F.R.S.

The Smithsonian Institution of Washington, U.S., has issued a catalogue of their valuable publications, and a list of various societies, journals, &c., with which they exchange publications. The Institution is anxious to add to this list any societies with which they have not hitherto been in communication, and also the names of specialists in all parts of the world. Communications should be addressed to Dr. Spencer F. Baird, at the Institution.

The Rev. S. J. Whitmee writes us with reference to reports which have lately reached this country of violent volcanic eruptions in the Society Islands, and also in the neighbourhood of New Britain. "These," he says, "have not, however, been confirmed, and from private information received from Tahiti, I hear that nothing has occurred in the Society Islands like what has been reported. From an Australian paper I saw a month ago, that the master of the brigantine *Matautu* had found a vast quantity of pumice-stone about the Ellice Islands. This is now confirmed by the captain of the missionary barque *John Williams*, who visited these islands in May and June last. From his account, a brief notice of which appeared in the *Times* last Friday, we learn that the pumice has reached the Ellice group from another locality, and that there has been no eruption in those islands. I give the following quotation from a notice of the *John Williams*' voyage which appears in the *Samoa Times* for July 27:—'In the Gilbert group, although the currents were somewhat irregular, the old strong easterly set seems to prevail again. In fact, sometimes it was running at the rate of thirty-six to forty-eight knots in the twenty-four hours. . . . On the outward voyage (from Samoa), when about 120 miles to the east of Nukulaelae, vast quantities of pumice-stone were passed, and the shores of all the Ellice Islands from Nukulaelae to Nintao and Nanumea are thickly covered with it. It is no exaggeration to say that hundreds of tons have been thrown up on every island. Stumps of trees and thick bamboos with roots attached have been thrown up on some of the islands. Early in May the brig *Isabella*, Everts, master, from Sydney, called at Vaitupu, and there left a report that there had been some volcanic eruption in the Society group. One report even says that Raiatea and Borabora have been destroyed, and 2,000 lives lost. There were also passed while beating back (to Samoa) between Nukulaelae and Fakaofo, one *mālili* tree about eighty feet long, one cocoa-nut tree, and four other gigantic forest trees, all evidently proofs that the story of Capt. Evers is probably only too true. The pumice-stone began to arrive on the Ellice Islands about the middle of April, and continued till the middle of June. When first noticed there was no seaweed growth on the pieces, and no barnacles, but by the beginning of June this began to be plainly noticeable. There is no trace of the pumice-stone in the Union or Gilbert Groups, and the time of its arrival in the Ellice group will be interesting to those who study the question of ocean currents.' From the above remarks it would appear that the pumice is supposed to have arrived from the east, and it is taken for granted that it has come from the Society Islands. But it is at least doubtful whether any eruption has taken place there. And, at a date later than that given as the time when the pumice ceased to reach Vaitupu, the *John Williams* met with trees far to the east of that island. If it had come from the east, surely some would have been seen also about the Union or Tokelau Islands. We must wait for further information as to where the eruption took place; but I am inclined to think it must have been to the west of the Ellice Islands."

We hear that M. Mannheim is engaged upon the preparation of a work entitled "Géométrie Cinématique." Hitherto our acquaintance with this important subject has been derived from Italian and German works, or these translated into French. M. Mannheim's ability as a geometer leads us to expect he will produce an elegant treatise on this branch of geometry.

According to the report for 1877 recently issued, made by Dr. Corfield, the Medical Officer of Health for St. George's, the death-rate for the parish in that year was as low as 17·46. Of the three sub-districts into which the parish is divided Hanover Square had a rate of 16·58, Mayfair of 13·04, and Belgravia of 18·70. The rate for the whole parish (17·46), with a calculated population of 91,037 is compared with some of the twenty-two "other large towns of the United Kingdom." Portsmouth has the lowest rate (17·4), which is therefore just under, Brighton with 18·7 coming next. No foreign town has so low a death rate as St. George's parish, the nearest known approach being Philadelphia (18·8). Dr. Corfield has calculated the mean duration of life in the parish and in each of the sub-districts for 1877. For the whole parish it was 49·52 years, for Hanover Square sub-district 56·63, for Mayfair 62·66, and for Belgravia 45·53. The mean duration in the 22 towns on which the Registrar-General reports was (including London) 34·2.

Prof. E. Morren's useful "Correspondance Botanique" is continually increasing in size. This year it occupies 150 pages, against 92 last year. The principal increase is in the American department, which is much fuller and more complete than it has been before. But in Europe also there is an increase in the number of names, partly due no doubt to more accurate information, but partly also, we may hope, to an actual increase in the number of workers in botanical science. It is a significant fact that while second and third class university towns in Germany or Italy number their six or eight working botanists, mostly attached to the university, the number of names given under the heads of Oxford and Cambridge together is five.

The *Golos* publishes the following telegram from Tiflis, dated November 27:—"Telegraphic intelligence received to-day states that this morning at two o'clock a severe earthquake was experienced at Suram and Borjom. A frightful rumbling noise was heard during the earthquake, accompanied by a shock so severe that people were thrown from their beds. Nothing similar has occurred in the district before."

Yesterday M. Bardoux, Minister of Public Instruction, laid the first stone of the new practical school of the Faculty of Medicine in Paris.

The Commission for the reception of the great reflector at the Paris Observatory have assented to its reception, although the mirror cannot be used in its full breadth without being diaphragmised at the circumference. It is stated, moreover, that the instrument can be used only on a very few days in the year in the atmosphere of Paris, and that reflectors are decidedly unmanageable except under special circumstances. It remains to be explained, then, how Lord Rosse and the great Herschel could manage to make so many interesting discoveries with instruments declared so unsuitable for celestial observations.

The Paris Observatory being no more intrusted with the care of publishing the *International Bulletin of Meteorology*, which has been taken over to the rue de Grenelle by the Central

Bureau, the Paris Press are deprived of all the documents for current meteorology which were put at their disposition by M. Leverrier.

We are informed that Admiral Mouchez has signed with M. Martin the contract for the polishing of the great lens of the great refracting telescope prepared by M. Leverrier. The lamp, of 75 centimetres diameter, has been placed in the hands of M. Feil, the glass founder, to repair a few defects which have been detected. This operation is done by cutting out the defective parts and heating the glass to a state of liquidity. This process is sometimes used for central parts with success. Guinault, the originator of the process used now for glass-founding, is said to have so mended eighteen times one of the most celebrated glasses produced by him at the end of the last century.

We regret to announce the death of Dr. Eugen von Gorup Besanez, Professor of Chemistry at Erlangen University, and author of an excellent chemical handbook in three volumes. He had attained the age of sixty-two years.

Bentley and Trimen's "Medicinal Plants" has now reached its thirty-fifth number, and maintains in every way the excellent promise with which it started. Both the letterpress and the illustrations are of sterling quality, and the work, when completed, will be a complete repertorium for the botanico-medical student.

Near the Rhinefall at Schaffhausen a cave has been discovered which was evidently used as a dwelling-place in prehistoric times. Flints, broken jars, and bone rests were found in it. The jars were partly of Celtic and Roman origin.

We have on our table the following works:—"Six Months in Ascension," by Mrs. Gill, John Murray; "Robert Dick, Geologist and Botanist," by Dr. Smiles, John Murray; "Pocket Book for Chemists," by Thomas Bayley, E. and F. H. Spon; "The Mollusca of the Firth of Clyde," by Alf. Brown, Hugh Hopkins; "A Visit to South America," by Edwin Clark, Dean and Son; "Coal: its History and Uses," Edited by Prof. Thorpe, F.R.S., Macmillan and Co.; "Bau des Eozoon Canadense," by Karl Möbius, Theodore Fischer; "Flowers, and their Unbidden Guests," Translated by W. Ogle, Kegan Paul; "Vogelbilder aus fernen Zonen," by Dr. Ant. Reichenon, Theodore Fischer; "Dictionary of Scientific Terms," by William Rossiter, W. Collins and Sons; "On Foot in Spain," by J. S. Campion, Chapman and Hall; "Elementary Geometry Books," i.–v., Fourth Edition, by J. M. Wilson, Macmillan and Co.; "Treatise on Chemistry," vol. ii., part 1, by Professors Roscoe and Schorlemmer, Macmillan and Co.; "The Magic Lantern Manual," by W. J. Chadwick, F. Warne and Co.; "The Localisation of Cerebral Disease," by Dr. Ferrier, Smith, Elder, and Co.; "Cassell's Natural History," vol. ii., Edited by P. Martin Duncan, F.R.S., Cassell, Petter, and Galpin.

The additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Dr. Whately, R.N.; a Brazilian Tree Porcupine (*Sphingurus prehensilis*) from Trinidad, presented by Dr. J. F. Chittenden, jun.; a Common Peafowl (*Pavo cristatus*) from India, presented by Mrs. Russ; two Common Cormorants (*Phalacrocorax carbo*), British, presented by Mr. Frank Buckland, F.Z.S.; a Water Rail (*Rallus aquaticus*), British, presented by Mr. W. Thompson; a Black Lemur (*Lemur macaco*) from Madagascar, a Rufous-vented Guan (*Penelope cristata*) from Central America, purchased; a Red Kangaroo (*Macropus rufus*), born in the Gardens.

ROYAL SOCIETY—THE PRESIDENT'S ANNIVERSARY ADDRESS[1]

Gentlemen,

At the conclusion of this, the fifth and last year during which I shall have held the most honourable office of your president, I have the gratifying assurance that the communications made to the Society and its publications have in no respect fallen off in scientific interest and value. We have not, indeed, been called upon to undertake during the past year such responsible and time-absorbing duties in behalf of the Government as the Polar, Circumnavigation, Transit of Venus, and other Committees demanded of us during the previous four years; but some of the results already achieved by those expeditions have been contributed to our publications, and we are in expectation of more. It is also with satisfaction that I can refer to the good attendance at our evening meetings, *soirées*, and *réunions*, as evidence of the interest taken in our proceedings by the Fellows generally and their friends.

Before proceeding to touch upon some of the advances made in science during the last few years, I have, as heretofore, to inform you of the Society's condition and prospects, and of those duties undertaken by its Council, for information as to which non-resident Fellows look to the annual address.

The loss by death of Fellows, twenty-one in number, is but little short of last year's rate, while that of Foreign Fellows (six) is twice as great as last year. On the home list is Sir George Back, the last, with the exception of our late venerable President, Sir E. Sabine, of that celebrated band of Arctic voyagers which during the early part of the century added so much to our renown as navigators and discoverers. Sir George was further the companion of Franklin and Richardson in that overland journey to the American Polar Sea, in which human endurance was tried to the uttermost compatible with human existence, and the modest but thrilling narrative of which, by the first-named officers, will ever hold a unique place in the annals of scientific discovery. Of Indian explorers no less than four have been taken away, namely, Col. Sir Andrew Waugh, for many years Director of the Great Trigonometrical Survey of India; and shortly afterwards his successor, Col. Montgomerie; Dr. Oldham, for a quarter of a century the Director of the Geological Society of India; and Dr. Thomas Thomson, my fellow-traveller in the Himalaya, the narrative of whose explorations in Western Tibet contains the first connected account of the physical and natural features of that remote and difficult country. Lieut.-Gen. Cameron survived but for one year our late Fellow, Sir Henry James, his predecessor in the Direction of the Ordnance Survey of Great Britain. In the Rev. James Booth we have lost a mathematician of high attainments, and the author of many contributions to our own and other scientific journals. The Rev. W. B. Clark, of New South Wales, wrote many papers abounding in excellent observations on Meteorology and Geology, especially made in England, the Cape of Good Hope, Australia, and the Pacific. The Rev. R. Main, the Director of the Radcliffe Observatory, at Oxford, was a very eminent astronomer, and for nearly half a century an indefatigable author. Lastly, Earl Russell, the distinguished statesman and the earnest advocate, whether in the Government or in Parliament, of every measure for the promotion of scientific inquiry.

Of Foreign Fellows our losses are a great chemist in Becquerel, of Paris, whose election took place upwards of forty years ago; a great physiologist in Claude Bernard, also of Paris; the father of mycology, and for long the patriarch of Scandinavian botanists, Elias Fries; a most distinguished physicist and the recipient of both a Rumford and Copley medal in Regnault; a veteran anatomist in Weber; and in Secchi, of Rome, an astronomer of astonishing activity, the author of more than three hundred separate contributions to the science of which he was so great an ornament.

In matters of finance I may with satisfaction refer you to our treasurer's balance-sheets.

It will be in your recollection that Mr. T. J. Phillips Jodrell placed in 1874 a sum of 6,000*l.* at the disposal of the Society, with the view of its being devoted to the encouragement of scientific research by periodical grants to investigators whom your Council might think it expedient thus to aid. Shortly after the receipt of this munificent gift, the Government announced its

[1] Address of Sir Joseph Hooker, C.B., K.C.S.I., the President, delivered at the Anniversary Meeting of the Royal Society, on Saturday, November 30, 1878.

intention of devoting annually for five years 4,000*l.* to the same object, thus anticipating the special purpose which Mr. Jodrell had in view. Thereupon, with that gentleman's consent, his donation was temporarily funded, and the proceeds applied to the general purposes of the Society until some other scheme for its appropriation shall be approved. In April last I received a further communication from Mr. Jodrell, declaring it to be his wish and intention that, subject to any appropriation of the sum which we might, with the approval of the Society, make during his lifetime, it should immediately on his death be incorporated with the Donation Fund, the annual income in the meantime going to the general revenue of the Society. Upon this subject I have now to state that since the receipt of that letter Mr. Jodrell has approved of 1,000*l.* of the sum being contributed to a fund presently to be mentioned.

I have also to inform you of a check for 1,000*l.* having been placed in my hands by our Fellow, Mr. James Young, of Kelly, to be expended in the interests of the Society in such manner as I should approve.

Mr. De La Rue, to whose beautiful experiments I shall have occasion to refer, has presented to the Society both the letterpress and the exquisitely engraved facsimiles of the electric discharges described in his and Dr. Hugo Müller's paper, and which have appeared with that paper in the *Transactions*.

Our Fellow, Dr. Bigsby, has presented seven copies of his "Thesaurus Devonico-carboniferus" for distribution, and they have been distributed accordingly.

A very valuable addition to our gallery of deceased eminent Fellows has been the gift, by Mr. Leonard Lyell, of a copy in marble, by Theed, of the bust of his uncle, the late Sir Charles Lyell, F.R.S., together with a pedestal. This is the best likeness of the late eminent geologist that has been executed, and is in every respect a satisfactory one.

I have the gratification of announcing to you, that through the munificence of a limited number of Fellows, means have been advanced for reducing the fees to which all ordinary Fellows in future elected will be liable. That these fees, though not higher than the most economical expenditure on the part of the Society for its special purposes demanded, were higher than it was expedient to maintain if any possible means for reducing them could be obtained, was not only my own opinion but that of many Fellows. They exceed considerably those of any other scientific society in England or abroad; their amount has occasionally prevented men of great merit from having their names brought forward as candidates, and they press heavily, especially upon those who, with limited incomes, have other scientific societies to subscribe to. Nor does it appear to me as otherwise than regrettable that so high an honour as Fellowship of the Royal Society, the only one of the kind in England that is limited as to the number annually elected, and selective in principle, should be attainable only at a heavy pecuniary expenditure. It is true that our Fellows receive in return annually volumes of publications of great value to science generally; but these treat of so many branches of knowledge that it is but a fraction of each that can materially benefit the recipient, while their bulk entails an additional expenditure; and now that the individual papers published in the *Transactions* are separately obtainable, the advantages of Fellowship are less than they were when to obtain a treatise on his own subject a specialist had either to join the Society, or to purchase a whole volume or a large part of it annually.

It was not, however, till I had satisfied myself that the annual income of the Society, though not ample, was sufficient for its ordinary purposes, that its prospects in other points of view were good, and that the expenditure upon publication was the main, if not the sole, obstacle to a reduction of fees, that I consulted your treasurer on the subject of taking steps to attain this object.

My first idea was to create, by contributions of small amount, a fund, the interest of which should be allowed to accumulate; and when the income of the accumulated capital reached a sufficient amount to enable the Society to take the step without loss of income, to reduce either the entrance fee or annual contribution; and to which fund Mr. Young's gift should be regarded as the first donation.

This proposal was in so far entertained by your Council that they resolved to establish a Publication Fund, and to place Mr. Young's gift to the credit thereof; and further, appointed a committee to consider and report upon the Statutes of the Society concerning the fees.

The movement, once set on foot, met with an unexpectedly enthusiastic reception; several Fellows, with the best means of forming a judgment, not only approved of it, but offered liberal aid, urging that the reduction of fees should be the first and immediate object, and that, if such a course were thought desirable, the means of carrying it out would surely be forthcoming. On this your Treasurer prepared for my consideration a plan for raising 10,000*l.*, the sum required for effecting any material reduction; and we resolved to ascertain by private inquiry whether so large an amount could be obtained.

Here again our inquiries were responded to in a spirit of, I may say, unexampled liberality: in a few weeks upwards of 8,000*l.* was given or promised by twenty Fellows of the Society, and I need hardly add that the remaining 2,000*l.* was contributed very shortly afterwards.

At a subsequent meeting of the Council it was resolved:—

1.—That the sums referred to as the Publication Fund, as well as those received or that may be hereafter received, for the purpose of relieving future ordinary Fellows from the Entrance Fee, and for reducing their Annual Contribution, be formed into one fund.

2.—That the Entrance Fee for ordinary Fellows be henceforth abolished; and that the Annual Contribution for ordinary Fellows hereafter elected be three pounds (3*l.*). Also, that the income of the Fund above-mentioned be applied, so far as is requisite, to make up the loss to the Society arising from these remissions and reductions.

3.—That the account of this Fund be kept separate; and that the annual surplus of income, after providing for the remission and reduction above recommended, be re-invested, until the income from the Fund reaches 600*l.* So soon as the annual income reaches this amount, any surplus of income in any year, after providing for the remission and reduction above-mentioned, shall be available, in the first instance, in aid of publication and for the promotion of research.

A list of subscribers to this Fund will be placed in the hands or every Fellow, with the information that it will be kept open for future contributions, in the interests of research and of the Society's publications. I hope that it will be largely and speedily augmented, and that it may eventually reach an amount which will provide us with the means of accomplishing as much as is effected by the Government Fund, upon our own sole and undivided responsibility. I must not conclude my notice of this movement without a mention of those whose encouragement and liberality have most largely promoted it; and first of all, Mr. Spottiswoode, to whose counsel and active co-operation throughout, its success is mainly due; Messrs. Young's and Jodrell's contributions have already been alluded to, they have been supported by others:—2,000*l.* from Sir Joseph Whitworth, 1,000*l.* from Sir W. Armstrong, and 500*l.* each from His Grace the Duke of Devonshire, Mr. De La Rue, Messrs. Spottiswoode and Eyre, Dr. Siemens, and the Earl of Derby, and 250*l.* from Dr. Gladstone. The balance is the joint contributions of thirty-two Fellows.

I have to mention your obligations to Dr. W. Farr for the labour he has bestowed on ascertaining these vital and other statistics of the Society, upon an accurate knowledge of which the calculations for the reduction of fees had to be based; and to Mr. Bramwell for constructing a table showing to what extent the above changes will affect the Society's present and future income. It may interest you to know that the contributions of ordinary Fellows in future to be elected, is but little over that which was required of all Fellows from the very commencement of the Society's existence, namely, 1*s.* per week, and that the last Fellow who paid that sum died in 1869. So recent (1823) has been the augmented scale of payment in force up to the present date.

Looking back over the five years during which I have occupied this chair I recognise advances in scientific discovery and research at home and abroad far greater than any previous semi-decade can show. I do not here allude to such inventions as the telephone, phonograph, and microphone, wonderful as they are, and promising immediate results of great importance to the community, nor even to those outcomes of great research and high attainments, the harmonic analyser of Sir W. Thomson, the radiometer and otheoscope of Crookes, the bathometer and gravitation meter of Siemens, but to those discoveries and advances which appeal to the seeker of knowledge for its own sake, whether as developing principles, suggesting new fields of

research, or awakening attention to hitherto unseen or unrecognised, or unexplained phenomena of nature.

In the foremost rank as regards the magnitude of the undertakings and the combination of means to carry them out, nothing in the history of physical science can compare with the Transit of Venus Expeditions. To observe the transit of Venus various nations of Europe and the United States competed as to the completeness of the expeditions they severally equipped. The value[1] of the solar parallax cannot be ascertained until the results of all the expeditions are taken into account, when it will have an international claim to acceptance. But advances in this direction will not have ended here, the very difficulties attending the observation of the transit of Venus, having directed attention to the method originally suggested by the Astronomer-Royal in 1857, of obtaining the solar parallax from the diurnal parallax of Mars at its opposition.

Mr. Gill, by the skilful employment at Ascension Island of the heliometer lent by Lord Lindsay, has greatly increased the accuracy of the method by which the necessary star comparisons with Mars are made, and there is every reason to believe that the results of his observations, which are now in course of reduction, will be very satisfactory.

Within the last two years a remarkable addition has been made to the number of members of the solar system by Prof. Asaph Hall's discovery of the satellites of Mars; and more recently, during the solar eclipse which was visible in America, by Prof. Watson's detection of planetary bodies within the orbit of mercury.

In 1876 Schmidt recorded an outburst of light in a star in Cygnus, which showed a continuous spectrum containing bright lines similar to those of the remarkable star of 1866. As the star waned the continuous spectrum and bright lines faded, all but one bright line in the green, giving the object the spectroscopic appearance of a small gaseous nebula.

Great progress has been made during the last five years at Greenwich in the method of determining the motions of the heavenly bodies by the displacement of the lines in their spectra, as first successfully accomplished by Mr. Huggins in 1868. Not only do the results obtained by the stars observed at Greenwich agree with those of Mr. Huggins, as satisfactorily as can be expected in so delicate an investigation, but the motions of seventeen more have been determined; whilst the trustworthiness of the method has been shown by the agreement of the values for the rotation of the sun and the motions of Venus, with the known movements of these bodies. Mr. Huggins has also obtained photographs of the spectra of some of the brighter stars, which give well-defined lines in the violet and ultra-violet parts of the spectrum. These spectra have already shown alterations in the lines common to them and the sun, which are of much interest.

In solar physics, which afford remarkable evidence of Mr. Lockyer's energetic labours in this country and Mr. Janssen's in France, I must mention our Foreign Member's wonderful photographs of the sun, wherein the minutest of the constant changes in the granulations exhibited on its surface (and which vary in size from $\frac{1}{10}$ of a second to 3 or 4 seconds) can be studied in the future from hour to hour and day to day; as can also their different behaviour at different periods of frequency of sunspots.

Before dismissing this fruitful field of research, I must allude to Mr. Lockyer's discovery of carbon in the sun; and to his announced but not yet published observations on the changes and modifications of spectra under different conditions, some of which he even regards as indicating the breaking up of the atoms of bodies hitherto regarded as elementary.

Some important investigations on the electric discharge have been communicated to the Society by Messrs. De La Rue and Müller, and by Mr. Spottiswoode. These, prosecuted by different means, tend to limit the possible causes of the stratification observed in discharges through vacuum tubes. They also point to the conclusion that this phenomenon is in a great measure due to motions among the molecules of the residual gas, which themselves become vehicles for the transmission of electricity through the tube. It is well known that gases at atmospheric pressure offer great resistance to the passage of electricity; and that this resistance diminishes (to a certain limit, different for different gases) with the pressure. And the researches in question appear to show that the discharge, manifestly disruptive at the higher pressures, is really also disruptive even at pressures when stratification takes place. The period of these discontinuous discharges has not yet been the subject of measurement, but it must, in any case, be of a very high order.

Under the auspices of the Elder Brethren of the Trinity House, and as their scientific adviser, Prof. Tyndall has conducted an investigation on the acoustic properties of the atmosphere. The instruments employed included steam whistles, trumpets, steam syrens, and guns. The propagation of sound through fog was proved to depend not upon the suspended aqueous particles, but upon the condition of the sustaining air. And as air of great homogeneity is the usual associate of fog, such a medium is often astonishingly transparent to sound. Hail, rain, snow, and ordinary misty weather, were also proved to offer no sensible obstruction to the passage of sound. Every phenomenon observed upon the large scale was afterwards reproduced experimentally. Clouds, fumes, and artificial showers of rain, hail, and snow were proved quite ineffectual to stop the sound, so long as the air was homogeneous, while the introduction of a couple of burners into a space filled with acoustically transparent air soon rendered it impervious to the waves of sound. As long as the continuity of the air in their interstices was preserved, the sound-waves passed freely through silk, flannel, green baize, even through masses of hard felt half an inch in thickness, the same sound-waves being intercepted by goldbeater's skin. A cambric handkerchief which, when dry, offered no impediment to their passage, when dipped into water became an impassable barrier to the sound-waves.

Echoes of extraordinary intensity were sent back from non-homogeneous transparent air; while similar echoes were afterwards obtained from the air of the laboratory, rendered non-homogeneous by artificial means. Detached masses of non-homogeneous air often drift through the atmosphere, as clouds pass over the face of the sky. This has been proved by the fluctuations observed with bells having their clappers adjusted mechanically, so as to give a uniform stroke. The fluctuations occur only on certain days; they occur when care has been taken to perfectly damp the bell between every two succeeding strokes; and they also occur when the direction of the sound is at right angles to that of the wind. Numerous observations were also made on the influence of the wind, the results obtained by previous observers being thereby confirmed. From his own observations, as well as from the antecedent ones of Mr. Alexander Beazeley and Prof. Osborne Reynolds, Prof. Tyndall concludes that the explanation of this phenomenon given by Prof. Stokes is the true one.

Turning now to biological branches of science, I find that the discoveries and researches of the past five years in this department also are far in advance of those of any previous period of equal length. The *Challenger* Expedition was, in point of the magnitude of the undertaking and completeness of its equipment, the rival of that for observing the Transit of Venus. Its general results, as far as hitherto made known, have been dwelt upon at length in my previous addresses, and the publication of them in detail is being rapidly pushed forward. Some very important papers by Mr. Moseley on the corals collected on the voyage have indeed been published in our *Transactions* with admirable illustrations by himself.

To the botanist and geologist no subject has a greater interest than that of the conditions under which the successive floras, which inhabited the polar area, existed and were successively dispersed over lower latitudes previous to their extinction, some *in toto* and over the whole globe, whilst others, though extinct in the regions where they once flourished, exist now only in lower latitudes under identical or under representative forms. It is only during the last few years that, thanks to the labours of those engaged in systematic botany in tracing accurately the directions of migrations of existing genera and species, and in determining the affinities of the extinct ones, and of palæontologists in referring the latter to their respective geological horizons, that any material advance has been made towards a knowledge of the origin and distribution of earlier and later Floras. I cannot better illustrate the condition of this inquiry than by calling your attention to two most recent publications on the subject, which have both appeared within the last few months.

[1] The Astronomer-Royal informs me that Capt. Tupman, who has taken the principal share in the superintendence of the calculation, fixes provisionally on a mean parallax of 8″·8455, corresponding to a distance of 92,400,000 British miles, but that the observations would be fairly satisfied by any parallax between 8″·82 and 8″·88, which in distance produces a range of from 92,044,000 and 92,770,000 miles, differing by 726,000 miles, a quantity almost equal to the sun's diameter.

As a contribution to the principles of geographical botany, Comte Gaston de Saporta's essay, entitled "L'Ancienne Végétation Polaire" (which appeared in the *Comptes Rendus* of the French International Geographical Congress) is a very suggestive one, and, having regard especially to its author's eminence as a geologist and palæontologist, is sure to command attentive study. Although it may be argued that neither solar nor terrestrial physics, nor geology, nor palæontology are in a sufficiently advanced condition to warrant the acceptation as altogether established truths of all conclusions advanced in it, still the array of facts adduced in evidence of these conclusions is very imposing, while the ability and adroitness with which they are brought to bear on the subject are almost worthy of the great French genius whose speculations from the starting-point of the theory, which is that life appeared first in the northern circumpolar area of the globe, and that this was the birthplace of the first and of all subsequent floras.

I should premise that Count Saporta professedly bases his speculations upon the labours of his friend, Professor Heer, whose reasonings and speculations he ever puts forward with generous appreciation, while differing from him wholly on the subject of evolution, of which he is an uncomprising supporter, Professor Heer holding to the doctrine of the sporadic creation of species.

In his "Epoques de la Nature" Buffon argues that the cooling of the globe, having been a gradual process, the polar regions must have been the first in which the heat was sufficiently moderate for life to have appeared upon it; that other regions being as yet too hot to give origin to organised beings, a long period must have elapsed, during which the northern regions, being no longer incandescent, as they and all others originally were, must have had the same temperature as the most tropical regions now possess.

Starting from this thesis, Count Saporta proceeds to assume that the termination of the azoic period coincided with a cooling of the water to the point at which the coagulation of albumen does not take place; and that then organic life appeared, not in contact with the atmosphere, but in the water itself. Not only does he regard life as originating, if not at the North Pole, at least near to it, but he holds that for a long period life was active and reproductive only there. In evidence of this he cites various geological facts, as that the older, and at the same time the richest, fossiliferous beds are found in the cool latitudes of the North, namely in lats. 50° to 60°, and beyond them. It is in the North, he says, that Silurian formations occur, and though they extend as far south as lat. 35° N. in Spain and America, the most characteristic beds are found in Bohemia, England, Scandinavia, and the United States. The Laurentian rocks again, he says, reach their highest development in Canada, and palæozoic rocks cover a considerable polar area north of the American great lakes, and appear in the coasts of Baffin's Bay, and in parts of Greenland and Spitzbergen. It is the same with the Upper Devonian and marine carboniferous beds preceding the coal formations; these extend to 76° N. in the polar islands and in Greenland, and to 79° N. in Spitzbergen, and he adds that M. d'Archiac has long ago remarked that, though so continuous to the northward, the coal-beds become exceptional to the southward of 35° N. Hence Count Saporta concludes that the climatic conditions favourable to the formation of coal were not everywhere prevalent on the globe, for that while the southern limit of this formation may be approximately drawn, its northern must have extended to the Pole itself.

I pass over Saporta's speculations regarding the initial conditions of terrestrial life, which followed upon the emergence of the earlier stratified rocks from the Polar Ocean, and proceed to his discussion of the climate of the carboniferous epoch as indicated by the characters of its vegetation, and of the conditions under which alone he conceives this can have flourished in latitudes now continuously deprived of solar light throughout many months of the year. In the first place, he accepts Heer's conclusions (founded on the presence of a tree-fern in the coal measures specifically similar to an existing tropical one), that the climate was warm, moist, and equable, and continuously so over the whole globe, without distinction of latitude. This leads him to ask whether, when the polar regions were inhabited by the same species as Europe itself, they could have been exposed to conditions which turned their summers into a day of many months' duration, and their winters into a night of proportional length?

A temperature so equable throughout the year as to favour a rich growth of cryptogamic plants, appears, he says, to be at first sight incompatible with such alternating conditions (as a winter of one long night and a summer of one long day); but equability, even in high latitudes, may be produced by the effect of fogs due to southerly warm oceanic currents, such as bathe the Orkneys and even Bear Island (in lat. 75° N.), and render their summers cool and winters mild. To the direct effects of these he would add the action of such fogs in preventing terrestrial radiation, and hence the cold this produces; and he would further efface the existing conditions of a long winter darkness by the hypothesis that the solar light was not, during the formation of the coal, distributed over the globe as it now is, but was far more diffusive, the solar body not having yet arrived at its present state of condensation.

That the polar area was the centre of origination for the successive phases of vegetation that have appeared in the globe is evidenced, under Count Saporta's view, by the fact that all formations, carboniferous, jurassic, cretaceous, and tertiary, are alike abundantly represented in the rocks of that area, and that, in each case, their constituents closely resemble that of much lower latitudes. The first indications of the climate cooling in these regions is afforded by *Coniferæ*, which appear in the polar lower cretaceous formations. These are followed by the first appearance of dicotyledons with deciduous leaves, which again marks the period when the summer and winter season first became strongly contrasted. The introduction of these (deciduous-leaved trees) he regards as the greatest revolution in vegetation that the world has seen; and he conceives that once evolved they increased, both in multiplicity and diversity of form, with great rapidity, and not in one spot only, and continued to do so down to the present time.

Lastly, the advent of the miocene period, in the polar area, was accompanied with the production of a profusion of genera, the majority of which have existing representatives which must now be sought in a latitude 40° further south, and to which they were driven by the advent and advance of the glacial cold; and here Count Saporta's conclusions accord with those of Prof. A. Gray, who first showed, now twenty years ago, that the representatives of the elements of the United States flora previously inhabited high northern latitudes, from which they were driven south during the glacial period.

Perhaps the most novel idea in Count Saporta's essay is that of the diffused sunlight which (with a densely clouded atmosphere), the author assumes to have been operative in reducing the contrast between the polar summers and winters. If it be accepted it at once disposes of the difficulty of admitting that evergreen trees survived a long polar winter of total darkness, and summer of constant stimulation by bright sunlight; and if, further, it is admitted that it is to internal heat we may ascribe the tropical aspect of the former vegetation of the polar region, then there is no necessity for assuming that the solar system at those periods was in a warmer area of stellar space, or that the position of the poles was altered, to account for the high temperature of pre-glacial times in high northern latitudes; or, lastly, that the main features of the great continents and oceans were very different in early geological times from what they now are. Count Saporta's views in certain points coincide with those of Prof. Le Conte of California, who holds that the uniformity of climates during earlier conditions of the globe is not explicable by changes in the position of the poles, but is attributable to a higher temperature of the whole globe, whether due to external or internal causes, to the great amount of carbonic acid and water in the atmosphere, which would shut in and accumulate the sun's heat, according to the principles discovered by Tyndall and applied by Sterry Hunt in explanations of geological times, and possibly to a warmer position in stellar space, a more uniform distribution of surface temperature, and a different distribution of land and water.[1]

Before Count Saporta's essay had reached this country[2] another continuation of the subject of the origin of existing floras had been communicated to our own Geographical Society, by Mr. Thiselton Dyer in a lecture on "Plant Distribution as a Field for Geographical Research." Mr. Thiselton Dyer's order of procedure is the reverse of Count Saporta's, and his method entirely different. He first gives a very clear outline of the distribution of the principal existing floras of the continents and

[1] Professor Jos. Le Conte, in NATURE, vol. xviii. p. 668.

[2] Count Saporta's essay was presented to the International Congress of Geographical Science which met in Paris in 1875, and was not received in England till the autumn of 1878, though it bears date of 1877 on the title page.

islands of the globe, their composition, and their relations to one another, and to those of previous geological epochs. He then discusses the views of botanists respecting their origin and distinctive characters, and availing himself of such of their hypotheses as he thinks tenable, correlates these with those of palæontologists, and arrives at the conclusion "That the northern hemisphere has always played the most important part in the evolution and distribution of new vegetable types, or in other words, that a greater number of plants has migrated from north to south than in the reversed direction, and that all the great assemblages of plants which we call floras, seem to admit of being traced back at some time in their history to the northern hemisphere." This amount of accordance between the results of naturalists working wholly independently, from entirely different standpoints, and employing almost opposite methods, cannot but be considered as very satisfactory. I will conclude by observing that there is a certain analogy between two very salient points which are well brought out by these authors respectively. Count Saporta, looking to the past, makes it appear that the fact of the several floras which have flourished on the globe being successively both more localised and more specialised, is in harmony with the conditions to which it is assumed our globe has been successively subjected. Mr. Dyer, looking to the present, makes it appear that the several floras now existing on the globe, are in point of affinity and specialisation, in harmony with the conditions to which they must have been subjected during recent geological time on continents and islands with the configuration of those of our globe.

(*To be continued.*)

HAECKEL ON THE LIBERTY OF SCIENCE AND OF TEACHING[1]

PROF. HAECKEL has recently published his reply to the address on "The Liberty of Science in the Modern State," delivered at last year's meeting of the German Association, by Prof. Virchow. If we enter into this subject at greater length than is our custom with pamphlets we do so mainly from a sense of common fairness to both parties, since we reproduced Prof. Virchow's address *in extenso* (NATURE, vol. xvii. pp. 72, 92, and 111). We shall, however, confine ourselves merely to stating Prof. Haeckel's views on the subject, and leave it to our readers to judge of the value of his remarks for themselves.

In the preface to the little book before us Prof. Haeckel states that the general views developed by Virchow are in such complete contrast to his own that no reconciliation of the two is possible. Yet he refrained for a considerable time from publishing his reply; and this for two reasons. On the one hand, he thought he might safely leave the judgment of the strife between them to the future; first, because the evolution theory, which Virchow attacks, has, *de facto*, become the basis of biological science of the present day; secondly, because Virchow's objections to the theory of descent have been so frequently and thoroughly refuted that it seemed superfluous to refute them again. On the other hand, he felt great reluctance in opposing a man whom a quarter of a century ago he honoured as the reformer of medical science, and whose pupil and zealous admirer he was for many years.

"The more I for years regretted Virchow's position as the enemy of our new evolution theory, and the more I was challenged to reply by his repeated attacks upon it, the less inclination I felt, nevertheless, to appear publicly as the antagonist of the highly-honoured and meritorious man. If now I find myself forced to reply, I do so with the conviction that longer silence would only augment the erroneous views which my resignation hitherto has already produced. . . . . I must point out distinctly that it is not Virchow but I who am the person attacked, and that in my case there is no question of *attacking* a formerly highly-honoured friend, but of *defending* myself by necessity against his repeated and violent attacks. Another reason which compels me to speak at last lies in the continued fertile use made of Virchow's speech by all clerical and reactionary organs for the last nine months. . . . Already Friedrich von Hellwald has pointed out the great danger which lies in the fact that it was a Virchow who, under the banner of political Liberalism, and wrapped in the mantle of pure science, combated the liberty of science and of its teaching."

The author then continues to point out that the danger was never so great in Germany as at the present moment, where the political and religious life of the German nation seems to approach a profound reaction. The two mad attempts upon the life of the honoured and aged Emperor have called forth a storm of just indignation of such violence that even many Liberal politicians not only press for severe measures against the utopian teachings of social democracy, but, far overshooting the mark, demand that free thought and free teaching should be confined within the narrowest bounds. What more welcome support can the reactionary party wish for than that a Virchow should publicly demand the suppression of the liberty of science? The danger appears still greater to Prof. Haeckel if Virchow's great influence as a "liberal Progressist" is taken into consideration, now that the Prussian Diet will shortly open its debates on the educational law. "What," Prof. Haeckel asks, "may we expect of this educational law, if in the discussions Virchow, as one of the few authorities who will be consulted, raises his voice in favour of the principles which in his Munich speech he proclaimed as the safest guarantees for the liberty of science in the modern state. Article 20 of the Prussian Constitution, and § 152 of that of the German Empire say: Science and its teachings are free. Virchow's first action, according to his present principles, must be a proposal to cancel this paragraph. In view of the menacing danger, I cannot hesitate any longer with my reply. Amicus Socrates, amicus Plato, magis amica veritas!" The rest of the preface is concerned with a refutation of the "denunciation" by which Virchow wants to make the theory of descent responsible for the horrors of the Paris Commune. Haeckel thinks that by an intentional coupling of Darwinism with Social democracy, Virchow intended to do considerable damage to the former, indeed he sees in it an attempt to remove all "Darwinists" from their Academical chairs. At the same time he points out that nine out of every ten zoologists and botanists now teaching at European universities are Darwinists. Virchow's attempt is therefore perfectly futile, and will certainly never have any effect at Jena. "What the Wartburg was for Martin Luther, what Weimar was for the greatest heroes of German literature, what Jena has been during three centuries for a large number of scientific men, that will Jena continue to be for the evolution-theory of the present day, as well as for all other scientific theories which develop freely, viz., a firm stronghold for free thought, free research, and free teaching."

We now come to the first chapter, which is headed "Evolution and Creation." The author remarks at the beginning that nothing has so greatly facilitated the progress of the evolution theory, as the fact that its principal problem, the question of the origin of species, was placed before the alternative: *Either* organisms have been *developed* naturally, in which case they must descend from the simplest and common ancestral forms—*or* this is not the case, and the different species of organisms have originated independently of one another, in which case they can only have been *created* in a supernatural way, *i.e.*, by a miracle. Natural development or supernatural creation of species—the choice must here be made, since a third way does not exist. Since Virchow and many other antagonists of the evolution theory constantly mix this up with the theory of descent, and this again with the theory of natural selection or Darwinism, Prof. Haeckel does not think it superfluous to give a concise definition of each of the three great theories at starting. He then states his definitions as follows:—"The relation of these theories according to the present state of science is therefore simply the following:—I. *Monism*, the universal theory of evolution, or the monistic pro-genesis theory, is the *only* scientific theory, which *rationally* explains the universe and satisfies the desire for causality in the human mind, since it brings all natural phenomena into a *mechanical* causal connection as parts of a great and uniform (*einheitlich*) process of development; II. *Transformism*, or the theory of descent, is an essential and indispensable part of the monistic evolution theory, because it is the *only* scientific theory which explains the origin of organic species in a rational manner, viz., by transformation, and reduces this transformation to *mechanical* causes; III. The theory of selection, or *Darwinism, is up to the present the most important one* amongst the different theories, which try to explain the transformation of species by *mechanical* causes; but it is by no means the *only one*. Even if we suppose that most species have originated through natural selection, yet we know, on the other hand, that many forms called species are merely hybrids from two different species and are propagated as such; at the same time we

[1] Freie Wissenschaft und freie Lehre. Eine Entgegnung auf Rudolf Virchow's Münchener Rede über "die Freiheit der Wissenschaft im modernen Staat." Von Ernst Haeckel.

can easily conceive that other causes may be acting in the formation of species, causes of which we have no idea at present. To decide what importance natural selection has in the origin of species is left to the judgment of the various naturalists, and in this question the authorities differ materially even to-day. Some ascribe a greater, others a smaller importance to it. But the different estimation of the value of Darwinism is quite independent of the absolute validity of the theory of descent, because the latter is up to the present the *only* theory which explains, in a *rational* way, the origin of species. If we abandon this theory then nothing remains but the *irrational* supposition of a miracle, of a supernatural '*creation*.' We will briefly designate this mystical belief in a creation as 'creatism.' In this decisive and inexorable alternative Virchow has now publicly stated his belief in creatism and his disbelief in transformism." The author then dwells at some length on this, and quotes from the *Zeitschrift für Ethnologie*, edited by Virchow and Prof. Bastian, in which the lattter applauds Virchow's Munich address, and ridicules Haeckel's "deliramenta and absurdities." There is no doubt, therefore, that Virchow has confessed himself a "dualist and creatist," and is as convinced of the truth of his principles as Haeckel is of the contrary as a "monist and transformist," yet the former still refrains from acknowledging his principles in all their consequences. "On the contrary he still clothes his opposition in the favourite phrase of the clerical papers, viz., that the theory of descent is an 'unproved hypothesis.' It is perfectly clear, however, that this theory will never be 'proved' if the proofs in existence to-day are not considered sufficient. How often has it been repeated that the scientific certainty of the theory of descent is not based upon this or that single experience, but upon the *totality* of biological phenomena, upon the *Causal-nexus* of evolution? What are we to think, therefore, of the new proofs for the theory of descent which Virchow demands?"

In Chapter II. Haeckel undertakes to give some "certain proofs of the theory of descent." He shows that all general phenomena of morphology and physiology, of chorology and oecology, of ontogeny and palæontology, can only be explained by the theory of descent, and reduced to *mechanical causes*. The guarantee of the truth of the theory lies particularly in the fact that the last simple causes are the same for all these complicated phenomena, and that other mechanical causes cannot be imagined. "Where are we, therefore, to find still further proofs for the truth of the theory of descent? Neither Virchow nor any other of the clerical antagonists or dualistic philosophers show us where we possibly might have to look for further proofs. Where in the world are we to find '*facts*' which speak louder than the *facts* of comparative morphology and physiology, the *facts* of rudimentary organs and of embryonal development, than the *facts* of palæontology and of the geographical distribution of organisms—in short, than all the known *facts* from the most various biological domains?" If proofs by actual experiments are demanded, these proofs have also been furnished by the domestication of animals and plants, and their variations under such domestication. All working naturalists are perfectly well aware to-day that the *morphological* value of the word "species" is not an absolute but a relative one; nor has it any physiological value. Haeckel points out here that the class of animals which furnish the best "certain proof" that the conception of species has only a relative value, are the sponges: "their liquid form wavers to and fro with extraordinary uncertainty and variability, and makes all distinction of species quite illusory." The species question, one of the principal points in the theory of descent, is not even mentioned by Virchow, and Haeckel considers this highly characteristic. He arrives at the conclusion that Virchow has never thoroughly digested the evolution theory, and has never attentively studied Darwin's principal work on the Origin of Species, nor any other work by this author. The remainder of this chapter is devoted to an account of Virchow's activity, first at Würzburg, and afterwards at Berlin. Virchow left Würzburg for Berlin in 1856; it was at the former university that he made the celebrated application of the cell-theory to pathology, which caused quite a revolution in the latter science. Haeckel considers that the exchange of the narrow sphere for the wider one was not beneficial in its consequences, and that the Virchow of the present day has completely changed from the Virchow of 1848-1856; he points out that only those who are aware of the enormous progress morphology has made during the last twenty years, and have followed this science in all its details, can estimate the full value of the theory of descent, and of Darwin's theory of selection. "Whoever wishes to convince himself of what an enormous revolution was caused by these theories, in comparative anatomy particularly, let him compare the classical 'Grundzüge der vergleichenden Anatomie' of Carl Gegenbaur (1870) and his 'Grundriss' (1878) with the older books on this science. Of all the colossal progress of morphology Virchow has no idea, since he was ever a stranger to this domain. His great reforms in pathology lie in the domain of physiology, and particularly in that of cellular physiology. But during the last twenty years these two principal branches of biological research have separated more and more. The great Johannes Müller was the last biologist who could comprise the entire domain of organic natural research, and who acquired immortal merit in both branches alike. After Müller's death (1858) the two halves fell apart. Physiology, as the special science of the functions of organisms, turned more and more towards the *exact* and experimental method. Morphology, on the other hand, as the science of the shapes and forms of animals and plants, could of course make but little use of this method; it was bound to have recourse to the evolution theory, and thus became essentially a *historical* natural science. It was just upon this historical and genetic method of morphology, in contrast with the exact and experimental method of physiology, that I laid a particular stress in my Munich address. If Virchow, in his counter address, had really refuted the latter in its various details, instead of fighting it with phrases and denunciations, this contrast of principles would at least have been worth minute examination. Yet I do not wish to reproach Virchow with this, since he is completely involved in the one-sided views of the school-physiology of the present day, and because morphology lies far too widely apart from his domain, to make it possible for him to judge its methods and ends for himself. If, nevertheless, upon every occasion he pronounces a depreciatory judgment upon it, then we must question his *competence* in doing this. It is true that in his Munich speech he prints with large type the phrase, 'That which adorns me is the *knowledge of my ignorance*.' But I regret that I must deny him this adornment most completely. Virchow does *not* know how ignorant he is of morphology. Otherwise he would not have pronounced those crushing sentences regarding it; he would not constantly designate the theory of descent as 'a hobby,' or 'dreaming,' or as personal speculation, which now inflates itself on many domains of natural science.' Indeed Virchow honours me too much if he calls that my 'personal hobby' which for more than a decade has become the most valuable common property of morphological science. If Virchow were not so unacquainted with morphological literature he would know that it is already completely impregnated with the principle of descent; that in all morphological work which is carried on systematically and with a certain end in view, the theory of descent is now accepted as quite *indispensable*. But he is unaware of all this, and thus we understand why he continues to ask for 'certain proofs' for that theory when these proofs have in reality been furnished long ago."

Chapter III. treats of craniology as applied to the theory of the descent of man from the ape. Haeckel points out the importance of comparative and genetic craniology, but cannot help regretting that a great deal of time and labour has been wasted during the last ten years by "craniologists" with discussions as to the best method of measuring skulls, and also that numerous naturalists, Virchow among others, seem to have seen in "craniometry" the highest aim and object of craniological science, and to have forgotten what they really want to prove with their measurements. Turning to the question of the descent of man, the author remarks that the well-known phrase "man has descended from the ape," which is so often misunderstood and misapplied, can in the sense of the evolution theory but have this meaning: The human race as a whole has descended from the order of apes, *i.e.*, from one (or perhaps more) species of apes now long extinct; the latest ones in the long series of man's vertebrate ancestors were apes or ape-like animals. Of course none of the species of apes now existing can be regarded as the unchanged descendant of the old parental form. Virchow in his address expresses his doubt of the truth of the descent in question, while Haeckel looks upon it as one of the most certain phylogenetic hypotheses. He does not deny that the relative certainty of this as well as that of any other phylogenetic *hypothesis* cannot be compared to the *absolute* certainty of the *theory* of descent, and draws special attention to the great difference between the whole theory and any particular hypothesis relating to an individual order or class

of animals; such hypotheses are always dependent on our actual biological knowledge and may be changed at any time for better ones, while the theory as a whole needs no further proof; it is absolutely certain. But for the objective zoologist it is impossible, according to the principles of comparative systematics, to assign to man any other place in the animal kingdom than in the order of apes, or primates, as Linnæus calls them; this classification, which is inevitable, leads to the *common* descent of man and ape from *one* ancestral form; and this is the essential part of the question. The views as to the exact appearance of this ancestral form may be divided, but we must eventually arrive at the conclusion, if we consider all facts connected with the subject, that our long extinct ancestors can but have been real apes, *i.e.*, some placental mammal, which, if it existed to-day, we should certainly classify among apes. Finally, Haeckel points out how characteristic it is of Virchow's view on the matter that he again places palæontology into the foreground, and, before accepting the theory of descent, demands that an uninterrupted series of fossil transition forms between ape and man should first be found. As Darwin himself has minutely stated the reasons why the solution of this problem cannot be expected, and has shown the cause of the extraordinary incompleteness of the palæontological records, and of the natural impediments to a geological proof of the ancestral tree (in Chapter X. of the "Origin of Species"), Haeckel again arrives at the conclusion that Virchow has never attentively read Darwin's great work, and has never digested the teachings of palæontology.

Chapter IV. is entitled "Cell-Soul and Cellular Psychology." Haeckel states here that the views he expressed at Munich with regard to the soul of the cell, *i.e.*, "that we must indeed ascribe an independent soul-life to each organic cell," are but the natural consequence of Virchow's own teachings, viz., of the very fertile application which Virchow made of the cell theory to pathology. He then proceeds to give the definition of the word "soul" according to both philosophical theories, first according to the monistic or realistic theory, and then according to the dualistic or spiritualistic theory; he compares the simplicity of the former with the mystery and irrationality of the other. He adduces the various phrases in Virchow's address which leave no doubt on the subject that Virchow has completely abandoned the realistic theory in favour of the dualistic one, and shows the utter futility of Virchow's view that we cannot find psychic phenomena in the lower animals. "Volition and sensation, the most general and most indubitable qualities of all mental life, cannot be overlooked in the lower animals. Indeed, with most *Infusoria*, particularly with *Ciliata*, independent motion and conscious sensation (of pressure, heat, light, &c.) are so very evident, that one of their most patient observers, Ehrenberg, maintained up to his death that all *Infusoria* must have nerves and muscles, organs of sense and of mind (*Seelenorgane*) just like all higher animals.

"Now it is known that the enormous progress which science has recently made in the natural history of these low organisms has reached its climax in the maxim that they are *unicellular* (a maxim which Siebold pronounced thirty years ago, but which has been proved with certainty only recently); therefore in the *Infusoria a single cell* performs all the different functions of life, including the mental functions, which in the *Hydræ* and *Spongiæ* are divided amongst the cells of the two germinal lobes, and in all higher animals amongst those of the various tissues, organs, and apparatus of a complicated organism. . . . By the same right by which we ascribe an independent 'soul' to these unicellular *Infusoria*, we must ascribe one to all other cells, because their most important active substance, the protoplasm, shows everywhere the same psychic properties of sensitiveness (sensation) and movability (volition). The difference in the higher organisms is only that there the numerous single cells give up their individual independence, and like good state-citizens, subordinate themselves to the 'state-soul' which represents the unity of will and sensation in the 'cell-association.' We must distinguish between the central soul of the total polycellular organism or the 'personal soul' and the separate elementary souls of the single cells, or 'cell-souls.' This maxim is excellently illustrated by the interesting group of *Siphonophora*. There is no doubt that the whole *Siphonophora*-state has a very determined and uniform (*einheitlich*) will and sensation; yet each one of the single individuals which compose this state (or *Cormus*) has its separate personal will and sensation. Indeed each one of these is originally a separate *Medusa* and the 'individual' *Siphonophora*-state has resulted only by association and division of labour of this united society of *Medusæ*. Next to the unicellular *Infusoria* no phenomenon affords such ample and immediate proof for the truth of our cellular-psychology than the fact that the *human ovum*, like the ovum of all other animals, is a simple and single cell. According to our monistic conception of the cell-soul, we must suppose that the fertilised ovum already possesses *virtually* those psychic properties which in the particular mixture of parental peculiarities (*i.e.*, those of mother and father) characterise the individual soul of the new being. In the course of the development of the ovum the cell-soul of course developes itself simultaneously with its material substratum, and becomes apparent *actually* when the child is born. According to Virchow's dualistic conception of the 'Psyche,' we must suppose, on the contrary, that this immaterial being enters the soulless germ at some period of embryonal development (perhaps when the spinal tube separates from the germinal lobe?). Of course this way the pure *miracle* is complete, and the natural and uninterrupted *continuity of development* is superfluous."

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

St. Peter's College, Cambridge, has made a statute assigning one of its Fellowships to the Jacksonian Professor. It is intended to limit this professorship by statute to some branch or branches of chemistry or physics, a specially constituted electoral body, including representatives of non-resident science, making the selection freely on each occasion of a vacancy.

The site most favoured for the Sedgwick Memorial Museum, Cambridge, is Downing Street, in front of the present new museums. There will be a good opportunity of concealing from public view these extremely plain buildings and of erecting a satisfactory façade. The Sedgwick Committee have informed the University that 1,200*l.* is in their hands for this purpose, but this amount is insufficient, and the University, when better supplied with funds, must supply a good deal more. A Syndicate, including Drs. Paget and Humphry, Profs. Liveing, Newton, Hughes, and Colvin, has just been appointed to select a site, to obtain plans, to confer with the Sedgwick Committee, and report by midsummer next.

Prof. Leith Adams, F.R.S., has been appointed to the Chair of Natural History in the Queen's University of Ireland, rendered vacant by the lamented death of Prof. Harkness.

SOCIETIES AND ACADEMIES

London

Royal Society, November 21.—"On a Method of Using the Balance with great Delicacy, and its Employment to Determine the Mean Density of the Earth," by J. H. Poynting, B.A., Fellow of Trinity College, Cambridge, and Demonstrator in the Physical Laboratory, Owens College. Communicated by Prof. Balfour Stewart, LL.D., F.R.S.

The two chief causes of error in the use of the balance are:—1. Disturbances through changes of temperature, such as convection currents, or unequal expansion of the two arms. 2. The possibility that after raising the beam on the supporting frame and lowering it again, the same parts of the knife edges may not come into contact with the planes. Errors from the first cause may be to a great extent avoided by protecting the balance with a gilded case, and reading the oscillations from a distance by means of a mirror on the beam. The residual effects may then be detected by taking three observations at equal intervals of time, the first and third having the same weights in the pan, and their mean being compared with the second (*i.e.*, for a short time the disturbance is assumed to be a linear function of the time). The second cause of error has been removed by not raising the beam between successive weighings. For this purpose a clamp is placed underneath one pan, which can be brought into action at any time to fix the pan in whatever position it may be. The weights can then be interchanged while the counterpoise (Borda's method being employed) maintains the beam in the same state of flexure, and the knife edges always remain in contact with the same parts of the planes.

The value of a given deflection was estimated by riders, and the weights were interchanged each by special arrangements.

The greatest deviation from the mean in the comparison of two 1 lb. weights in one group of twenty comparisons, when the weather was unfavourable, was 1·20 millionth of 1 lb., while in another group of twenty-seven comparisons (the weather being much finer and more favourable) the greatest error was 1·50 millionth.

To determine the mean density of the earth, a 1 lb. weight was hung from one arm of the balance at a distance of about six feet below it, and was accurately counterpoised in the other pan. A large sphere of lead (about 340 lbs.) was then alternately inserted under the hanging weight, and withdrawn. The difference which its attraction made in the weight of the hanging weight was about 1·45 millionth of 1 lb. This increase of the weight was measured as accurately as possible by means of riders on the beam of the balance. Comparing this with the attraction of the earth on the weight—that is, its weight—we can calculate the mass of the earth in terms of the mass of the lead sphere. The results hitherto obtained are only preliminary, though they seem near enough to former determinations to show that with improved arrangements which the author intends to make, a good value may ultimately be obtained. The mean of 11 determinations is 5·69, with a probable error of 0·15.

Physical Society, November 23.—Prof. W. G. Adams, president, in the chair.—Prof. Ayrton read a paper on the music of colour and of visible motion, which we give elsewhere. —Dr. Schuster then described his new method of adjusting the collimator of the spectroscope for parallel rays of different refrangibility. His plan is very simple, and is based on the fact that if the rays entering the prism are parallel, the focus seen in the telescope will remain constant when the prism is turned round, but if they are not parallel, the focus will shift. The process, therefore, consists in looking through the telescope while turning the prism. If the focus shifts, the collimator has to be adjusted until no shifting takes place. The adjustment must be made with a prism whose sides are perfectly plane, and a good one may be kept for the purpose.

Statistical Society, November 19.—A numerous list of candidates were balloted for and elected Fellows.—The Howard prize medal, with 20*l*., has been awarded to Surgeon John Martin, L.R.C.S. Edin., of the Army Medical Department, at present serving in India with the Royal Artillery. An *extra* prize medal has also been awarded to Capt. H. Hildyard, of the 71st Highland Light Infantry, his essay being scarcely inferior to that of Mr. Martin.—The President, Mr. G. J. Shaw-Lefevre, M.P., in his opening address, commented on the past work of the Society, especially the papers read by its members during the past session, and the various statistics collected through its operations. Their great object was to study the past so as to understand the present and be able to give a forecast of the future.—Prof. Jevons afterwards exhibited and explained to the meeting the arithmometer of M. Thomas, long in use among actuaries.

EDINBURGH

Royal Society, November 25.—The following office-bearers were elected:—President, Prof. Kelland; Vice-Presidents, David Stevenson, C.E., Bishop Cotterill, Sir Alexander Grant, Bart., David Milne Home, Sir C. Wyville Thomson, Prof. Douglas Maclagan; General Secretary, Prof. Balfour; Secretaries to Ordinary Meetings, Prof. Tait, Prof. Turner; Treasurer, David Smith; Curator of Library and Museum, Alexander Buchanan; Council, Prof. Fleeming Jenkin, Rev. R. Boog Watson, Dr. Hugh Cleghorn, Prof. T. P. Fraser, Prof. Rutherford, Dr. R. M. Ferguson, Rev. W. Lindsay Alexander, Dr. Thomas A. G. Balfour, J. T. Buchanan, Rev. Thomas Brown, Robert Gray, and Dr. William Robertson.

PARIS

Academy of Sciences, November 25.—M. Fizeau in the chair.—The following papers were read:—Critical examination of a posthumous writing of Claude Bernard on alcoholic fermentation, by M. Pasteur. He represents this writing as a sterile attempt to substitute for facts well established the deductions of an ephemeral system.—On the reduction in continuous fractions of $e^{F(x)}$, $F(x)$ designating an entire polynome, by M. Laguerre.—On isosceles figures, by M. Badoureau.—Reply to various communications by M. Levy.—Reclamation of priority, in regard to M. Werdermann's communication on an electric lamp, by M. Regnier.—On a new phenomenon of static electricity, by M. Duter. In certain cases electrification may change the volume of bodies. A large thermometric envelope containing water is made into a condenser by pushing a piece of platinum wire into its exterior, and fixing outside a sheet of tin. Whenever, like a Leyden jar, it receives a charge, the water is observed to descend, remain stationary while the charge persists, and resume its former level on discharge. It is inferred that the glass is dilated. With any kind of armatures the same result is had. Another experiment was to place the above-mentioned arrangement in another thermometric envelope containing water; on electrifying, the water in the measuring-tube of the outer envelope rose, while the other fell. M. Jamin recalled the fact that M. Govi, ten years ago, made an experiment similar to M. Duter's first, and attributed the effect to a contraction of the liquid; M. Duter's second experiment proves that the expansion of the glass is really the cause.—Reply to a note of M. Meunier on the artificial crystallisation of orthose, by MM. Fouqué and Levy. M. Meunier (they hold) had not sufficient data to determine the nature of the minerals produced; his experiments are a mere repetition of those of James Hall in 1798, who fused natural rocks, subjected them to long annealing, and found the metal grains obtained had sometimes a crystalline texture. The authors, far from having got results with orthose like those of M. Meunier, find a marked difference between this felspar and others as to its structure after reproduction by igneous fusion; it does not take the ordinary crystalline structure, and this reveals the necessity of intervention of volatile elements in genesis of acid rocks.—Note on the element called *Mosandrum*, by Prof. Lawrence Smith. He claims priority in having called attention to the absence of the oxide of cerium, and to new characters of certain earths in the mineral samarskite, and having indicated a new one he called *mosandrum*.—Double stars; certain groups of perspective, by M. Flammarion. He gives a list of couples that are merely optical groups, due to the meeting, in the same visual ray, of stars situated one beyond the other in space, and having different proper motions.—On the number of complete arrangements where consecutive elements satisfy given conditions, by M. André.—On various derivatives of spirit of turpentine, by M. De Montgolfier. He has studied the action of sodium chiefly in chlorhydrates of turpentine, solid and liquid.—On a cyanised derivative of camphor, by M. Haller.—Action of salts of chromium on salts of aniline in presence of chlorates, by M. Grawitz. He notes the advantage of using these salts in place of vanadic salts; they are less rare and dear, and have even greater energy. $\frac{1}{10}$ of a milligramme of bichromate of potash, per 125 grammes of aniline salt dissolved in water, still developes black.—On the physiological action of borax, by M. De Cyon. Borax added to meat to the extent of twelve grammes daily (a quantity ten times that required by the Jourdes process), may be taken in food without causing the least disorder in general nutrition. Substituted for marine salt, borax increases the power of assimilating meat, and may cause a large increase of weight in the animal, even when the alimentation is exclusively albuminoid. This all applies to pure borax only.

CONTENTS

THURSDAY, DECEMBER 12, 1878

LORD RAYLEIGH'S "THEORY OF SOUND"

The Theory of Sound. By J. W. Strutt, Baron Rayleigh, F.R.S. Vol. II. (London: Macmillan and Co., 1878.)[1]

THE second part of Lord Rayleigh's highly instructive work on acoustics contains the mechanics of oscillatory motions in liquids and gases. Atmospheric air is that medium by which by far the greater number of sound-waves are conveyed to our ear, since it is only exceptional that this happens through solid bodies which are in contact with our teeth or with the bones of the skull. But it is just for this reason that all circumstances are of considerable importance, which influence the transmission of sound-waves in the air, *i.e.*, change either their velocity, their direction, or their intensity. This part of the theory has been worked out very minutely and completely by the author. We find here the compilation and demonstration of a large number of facts which, in other works on acoustics, are hardly mentioned. The author, after having first developed (in Chap. XI.) the general laws of the motion of liquids as expressed in hydrodynamical equations, and then explained the difference between rotational and irrotational motion of fluids, passes on to the simplification of the equations, which is determined by the circumstance that with sound, as a rule, we have to do with oscillations of extremely small amplitude. First, the motion of plane waves is investigated, and it is shown that with waves which move only in one direction half their equivalent of work consists in the *vis viva* of motion, and the other half in the potential energy of the compression and dilatation of the medium. Then follows the explanation of the influence which the change of temperature, taking place with compression or dilatation of gases, exercises upon the velocity of transmission of sound. It is shown, in the manner first employed by Prof. Stokes, that if a perceptible quantity of heat could be exchanged between the compressed and dilated layers of the waves during the lapse of one oscillation, the intensity of the sound-waves would very quickly decrease in their transmision and they would die away.

The subjects treated of up to this point are generally known among physicists; less known are a series of other results of the theory. The author next gives a comparatively very elegant and easily intelligible demonstration of the results at which Poisson and Riemann arrived, when investigating the propagation of sound-waves for which the velocities of oscillation are no longer infinitesimal when compared to the velocity of transmission. It appears that the different layers of the wave transmit their phases with different velocities, viz., with that velocity which represents the sum of the ordinary velocity of transmission of the smallest waves and of the oscillation velocity of the particles of air oscillating in the same direction. The compressed layers of the wave, therefore, are propagated quicker than the dilated ones, thus they must gradually change the shape of the wave, and finally overtake the preceding dilated layers. What would happen in that case, whether perhaps a breaking of the waves of air would take place, is not yet clear, since the hydro-dynamical equations apply only to velocities changing continuously.

These circumstances have not always been considered in experimental researches concerning the velocity of sound. A precise answer to the question regarding the magnitude of this velocity can only be given, if we confine ourselves to oscillations of extreme smallness.

The author has also investigated under what conditions a sound-wave of finite amplitude can move forward without changing its form. It appears that this could happen only under the supposition of a special law for the compressibility of the medium, which does not correspond with the law applying to gases.

The propagation of sound in the atmosphere is subjected to yet other perturbations, which partly arise from the different temperatures and moistures of the superposed strata, and partly from the different force of winds. At the surface of water or extensive masses of solid substance, the sound-waves of the air are totally reflected even under very small angles of incidence; under perpendicular incidence their reflection, although not total in the strict sense of the word, is nearly as complete. For that part of the sound which enters the new medium, the same law of refraction holds good which applies to waves of light. But also from a surface of hydrogen one-third of the sound coming through air at a vertical incidence is reflected, and the angle of incidence for total reflection is not larger than $15\frac{1}{4}$ degrees.

The problem to determine theoretically how the propagation of sound in the atmosphere is changed by the different temperatures of its strata cannot yet be solved completely. However, it can be ascertained in what direction the most powerful effect must travel. On account of the great dimensions of the strata of the atmosphere, compared to which the wave-length of the large majority of audible tones disappears entirely, the conditions of the propagation of sound are similar to those of light. We may imagine the sound-waves dissolved, as it were, into rays of sound, and then look upon each separate ray as being almost completely independent of the motion of its neighbouring rays. This is no longer admissible if obstacles are in the way of the travelling sound, the dimensions of which exceed the sound wave-lengths only in moderate proportions, as is the case in our houses and rooms, with the transmission of sound through windows and doors. Then, as in the case of light under similar circumstances, diffraction takes place. The great difference in the propagation of sound and light, as it becomes evident in ordinary experience, has its cause in the very different magnitude of wave-lengths. The greater the wave-length the greater the diffraction on the passage through the same aperture. These circumstances, which are forgotten so frequently, the author considers in Chapter XIV. When sound is propagated in the unbounded space of the atmosphere the conditions of the problem are such, that they allow of its decomposition into rays of sound. If a source of sound is near the ground then its sound rays are all bent into an upward direction, as Prof. Osborne Reynolds first pointed out, and those which travel in a direction parallel to the ground are mostly annihilated through friction or other obstacles. The sound proceeding from

[1] For Vol. I. see NATURE, vol. xvii. p. 237.

a source near the ground is, therefore, not heard far off by an observer standing on the ground. It is heard at a much greater distance if the observer or the source of sound be in an elevated position. The state of the atmosphere will have great influence upon these conditions. In dry air and sunshine the deflection of sound upwards will be greater than in moist air which forms clouds above, or during rain.

The decrease of temperature in the upper strata of the atmosphere causes sound to travel at a lesser speed than in the lower ones. Now if a wind is blowing, the velocity of which increases with the height, then, as Prof. Stokes remarks, this causes an increase in the velocity of the sound-waves in the direction of the wind, and a decrease in velocity in the opposite direction; thus, for sound which travels in the former direction, the retarding effect of decrease of temperature is neutralised, and for that travelling in the latter it is augmented. We therefore hear better if the wind blows towards us from the source of sound than if the contrary takes place. Indeed, by an upper windy layer, sound produced in the lower tranquil air may be totally reflected. This influence of wind is remarkable also because it forms an exception to the law demonstrated by myself, viz., the law of reciprocity in the propagation of sound, if the sound-source and the observer change places.

The problems of the reflection of sound by fixed walls, for instance, the phenomena of whispering-galleries, speaking-trumpets, and the echo, are treated in the same manner. Although here the admissibility of the decomposition of sound into sound-rays does not, as a rule, appear quite so unquestionably justified, yet the phenomena observed agree with this hypothesis on the whole.

An essential progress in the application of the theory upon experiments has been made by the author in the calculation of the influence of the open apertures of organ pipes and resonators upon their pitch. In my own demonstration of this part of the theory I had started from those forms of motion which did not render the calculation too difficult, and had then derived the corresponding forms of pipes; finally, I had so determined the optional constants of my hypotheses, that the form of pipe approached the form wished for, the cylindrical one, for instance; yet I remained confined to a few forms if I did not wish to complicate the calculation too much. Lord Rayleigh, on the contrary, supposes a given form of pipe, and has employed the maxims, developed in the first volume of his work, regarding the variation of conditions under which sound-motion takes place, to determine the limits within which the true value of the desired magnitude must lie, and has indeed been able to draw these limits so narrowly for the most important problems, like that of the cylindrical open pipes, that practically the solution is perfectly sufficient. In this way he has been able to treat simultaneously a number of problems, which hitherto had not even received an approximate solution, for instance, the determination of the proper tones of resonators of the shape of bottles with wide body and narrow neck.

Besides the problems mentioned, which are of direct importance to experimental physics, a series of others are worked out, where the mathematical solution can be completely given, such as the propagation of sound in balls, spherical layers and rectangular boxes filled with gas, the reflection of sound from the outer surface of a ball, and the communication of sound to air by oscillating balls and strings. These problems are valuable not only as theoretical exercises, but also with regard to our understanding of physical phenomena. They are examples affording to the mental eye of the physicist a particularly perfect insight into the essence of sound-motion and the changes it undergoes, when the conditions under which it occurs are changed. Thus he obtains quite as good a conception of the typical behaviour of sound as if he had actually seen the phenomena, and this conception will also guide him safely in cases of observation, where the exterior conditions are not as simple as they are in the theoretical example.

At the end of the volume Lord Rayleigh has placed the words: "The End." We hope that this may be only the provisional, not the definite end. There is still an important chapter wanting, viz., that on the theory of reed-pipes, including the human voice. For the former, at least, the principles of their mechanics can already be given, and the methods the author employs seem to me to be particularly well adapted for further progress in these domains.

After reed-pipes we would mention the theory of singing flames, and the blowing of organ-pipes. In the latter case the leaf-shaped current of air, which comes from the wind-case, forms a sort of reed, which oscillates under the influence of the oscillating column of air in the interior of the pipe, and which throws its air now into the interior of the pipe, and now outside.

Altogether, the whole of this important class of motions, where oscillatory movements are kept up through a cause which acts constantly, deserves detailed theoretical consideration. The action of the violin bow, and the sounding of the Æolian harp, also belong to this class.

Lord Rayleigh certainly deserves the thanks of all physicists and students of physics; he has rendered them a great service by what he has done hitherto. But I believe I am speaking in the name of all of them, if I express the hope, that the difficulties of that which yet remains will incite him to crown his work by completing it.

H. HELMHOLTZ

OUR BOOK SHELF

Zoologischer Anzeiger. Herausgegeben von Prof. J. Victor Carus. (Leipzig: Wilh. Engelmann.)

THE idea of a zoological "advertiser" seems to us a most excellent one, and we both welcome and heartily recommend to our readers who are interested in the animal kingdom this little fortnightly journal of Prof. Carus. The editor purposes to publish a number every two weeks, each number to contain from sixteen to eighteen pages, and the yearly subscription to be six shillings. The first number bears the date of July 1, 1878, and already a dozen numbers have appeared. The plan of each number is to commence with a retrospect of the current literature of zoology, a retrospect that we need hardly say will be well done by one so learned and able in the matter of bibliography as the editor of that most necessary work, the "Bibliotheca Zoologica." Then each number contains some short notices on zoological subjects in connection with museums, chiefly such as have a practical bearing. Thus in No. 2 we find an article by G. von Koch, of Darmstadt, on a method by which sections can be made of substances of different

degrees of consistency without altering the relative position of the same, which cannot fail to be of interest and value to such as wish to make and mount thin sections of corals or alcyonarians, while in most of the numbers there are under this head to be found notices of collections for sale or specimens to be exchanged, and when this journal becomes, as we have no doubt it will, known to all directors and assistant-directors of zoological museums we anticipate for this section a very extended use. Another portion of the journal is devoted to short notices on general zoological subjects. Thus No. 11 contains a short notice by Prof. Salensky, of Kasan, on the embryology of the ganoids; one by Prof. Goette, of Strasburg, on the development of the bones in the limbs of vertebrates; a note by Dr. A. Gruber, of Freiburg, on the formation of the oviducts in the Copepods; one by Prof. Entz, of Klausenberg (Hungary), on the evolution of gas from the protoplasm of some protozoa, in confirmation of a record of the same fact by Prof. T. W. Engelmann; and one by Prof. E. Martens, on our knowledge of thread-spinning snails.

Another characteristic of this new journal is that, under the heading "Personal-Notizen," will be found a very exact list of all the museums and schools of anatomy and zoology in the world, commencing with those in Germany. This list has now got as far as Belgium. The directors' and assistant-directors' names, with those of the professors and assistant-professors, are given in full, and if, when the list is complete, an index of the names of the various teachers in all the colleges and schools were added, the list would serve many a useful purpose.

We feel persuaded that this most useful little journal will require only to be known in this country to be duly appreciated, and we wish its talented editor every success in his undertaking. E. P. W.

La Vegetacion del Nordeste de la Provincia de Entre-Rios. Informe Cientifico. Del Dr. Don P. G. Lorentz. (Buenos Aires, 1878.)

THIS is a book, or rather a pamphlet, of 179 pages of closely-printed matter, and illustrated by two maps of the country described, the nature of which with regard to its vegetation is very carefully recorded in the first division, which occupies forty-seven pages. The second part consists of a list of species arranged scientifically under each natural order, the paragraph referring to the individual plant comprising such information as to the frequency or scarcity of the species, the colour of the flowers, period of flowering, and any properties for which the plant may be economically valuable. These lists are useful in many ways, for instance they often show the widespread geographical range of many well-known plants, and in the lists before us we find many European introductions. A separate list of thirty-two species of fungi is added, and some notes on the maps given.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Was Homer Colour-Blind?

UPON reading Dr. Pole's two papers (NATURE, vol. xviii. pp. 676, 700) my first feeling was to ask: "But how could

'The blind old man of Scio's rocky isle'

know anything at all about colour?" Presuming, however, that the tradition of his blindness might be unwarranted, and further, that it may be a mistake to suppose, as many do, that the "Iliad" is a collection of rhapsodies by different poets, I again asked myself: "Are there in Homer more anomalies in the nomenclature of colours than may be accounted for by the vague use of words? Are there more than we should find in this country among uneducated men of the labouring class?" About two years ago I made extensive inquiry as to the prevalence of colour-blindness among children, and in the village schools of this part of Somersetshire I found that the girls could name the neutral as well as the other tints readily and correctly, but that many of the boys had but about half-a-dozen words to use, and would refer orange to red or to yellow, and purple to brown or to blue, merely for want of terms; for they could match the test papers with other papers, or with the girls' dresses.

If we refer to the old ballads and early romance poetry of our own and other languages, we shall see that the popular poets of the middle ages, like the peasant boys of the present day, misused terms of colour as much as Homer; although the many beautiful paintings that still exist prove that people could see and distinguish colours as well then as now, and that Mr. Gladstone's theory of a development of the sight from one generation to another is a mere delusion. Certain terms are adopted and handed down traditionally as stock epithets in poetry and technical terms in trades. They are known to be wrong, but they are used from habit.

Dr. Pole assumes that the colour-blind see black and white as others do; or, to use his own words (p. 700), that their vision in regard to them is normal. This I doubt. One of the gentlemen who is so affected tells me that he cannot distinguish snow upon the steps of his front door. Now if white is a combination of all the colours of the prism, and we omit red and green, there will be seen a combination of blue and yellow, and these when spun together in a colour top produce stone colour, which I believe to be the white of those who have a dichromal vision. As to black, it is singular that in Anglo-Saxon *blac* means not "black," but as the Flemish *bleek* and Germ. *bleich*, "pale," a case in point to show the instability of language in these matters.

To take Homer's terms *seriatim*:—

Ἐρυθρός.

A poet must not be pronounced colour-blind if he compares wine to blood, and calls it red.

"The king sate in Dumfermline town, drinkin' the *bluid-red wine*."
Sir Patrick Spens, l. 1.

"And aye she dighted her father's wounds, His *blood* ran down *like wine*:"
Douglas Tragedy, st. 8.

Conversely, in slang language to give a man a bloody nose is to "tap his claret." The chair-cushion upon which I am sitting and the curtain of my window are of a dark crimson, but in the language of upholstery would be called "maroon." Now *marron*, from which the word is derived, is a Spanish chestnut, and that is a full brown without any visible red in it.

In our old English ballads and early romances, and in the German, Flemish, Swedish, and Danish, and in some more modern poems gold is constantly called "red" and "ruddy;" as in Dryden:—

"A crown of ruddy gold enclosed her brow."

In a German ballad by Ehrhardt, "Die Nonne," st. 5:—

"Was zog er von seinem Finger? Einen Ring von *Gold so roth*."

In a Flemish ballad of the sixteenth century called "Het Soudaen's Dochterken" in Thijm's "Gedichten," v. i. p. 246:—

"Sijn hayr dat blinckt van verwe schoon,
Als waer het *roode gouden*."

In an ancient Swedish ballad called "Gångarpilten," Arwidss v. ii. p. 156:—

"För jag har intet *rödt guld* att sätta mod er.

And in the corresponding Danish ballad, Dan. Viser iv. p. 122:

"Jeg haver ei *det röde guld*."

Milton, "Paradise Lost," bk. ii. l. 889, calls flame "ruddy flame."

In old herbals, as in that of Lyte, fol. 1578, p. 162, marigolds are called "Ruddes." "They be called in Englishe Marygoldes and *Ruddes*."

Hair, such as is usually called "red hair," is better named in Greek πυῤῥός, fiery; for certainly its colour is widely different from blood. Where in the Old Testament the word "red" is applied to horses and heifers, the Greek of the Septuagint has πυῤῥοί.

What is really red is on the contrary called by Jenner "pink" in the well-known poem upon the signs of wet weather:—

"Closed is the *pink-eyed* pimpernel."

To come to the very age we live in, old croquet-players persist in calling the second ball of the series "pink," although for the last ten years it has always been painted red.

Now if English should become a dead language, what will some future critic suppose *red* to have meant? a term that he finds applied to blood, to gold, to wine, to the marigold, to flame, and to bay horses; and replaced with *pink* in the case of the pimpernel and the second croquet-ball?

Φοίνιξ.

This word meant originally Phœnician, a people from whom the richly-dyed robes that they imported were called; as in an old poem we have a colour designated from Bristol:—

"Her kirtle *Bristol red;*"

and as a deep blue dye is called "indigo" from being first brought from India.

The same word φοίνιξ was applied to horses, probably Syrian ones in the first place, just as from Rouen we call those of a certain colour "roan," agreeably to a common usage in all languages. Thus porcelain is called "China," and a certain leather made of goats' skin "Morocco," although manufactured in Europe.

As to the term φοίνιξ being applied to the lion and the jackal, we may well suppose that Homer never saw the one or the other. It is quite as unlikely that he ever saw a live dragon. If the horses that the Phœnicians introduced were tawny, it would be no misnomer to call the lion and jackal φοίνιξ. We are not to presume that if Phœnician robes were crimson, everything else that was named after them must also have been crimson. A future critic might as reasonably argue that porcelain from China was of an orange colour, because there are "China oranges."

Dates were also called φοίνιξ as being a Phœnician fruit, just as the small grapes imported from Corinth are called "currants."

Ῥοδόεις.

This term referred, no doubt, to a crimson variety of rose, the so-called Damask rose, the one usually cultivated in ancient times. Thence a comparison of its colour to blood implied by a line of Bion:—

Αἷμα ῥόδον τίκτει τὰ δὲ δάκρυα τὰν ἀνεμώναν.

Where this word is applied to oil in the account of the funeral of Patroclus (Il. xxiii. 186), it is to a heavenly oil with which Venus anoints the corpse of Hector to preserve it from putrefaction, and not ordinary olive oil:—

ῥοδόεντι δὲ χρῖεν ἐλαίῳ Ἀμβροσίῳ.

It may have meant either "rose-coloured" or "rose-scented."

Κυάνεος.

This term, which seems in so many passages to mean "dark," would have been very properly applied to the sand of volcanic islands, like those in the Egean Sea. On the coast of the Gulf of Naples near Pompeii it is quite black, and walking over it on a hot sunshiny day I had cause to remember its colour, for my feet were roasted.

Χλωρός.

Grass in the Mediterranean countries soon withers and dries to a pale colour, and remains so the greater part of the summer. It was to this withered grass that Homer seems to have compared a pale complexion, and honey, and olive wood, and the nightingale. Our evergreen meadows are unknown in the south.

Οἶνοψ.

The houses of the ancients were unprovided with glass windows, and were very dark within, so that entertainments must have been given by lamp-light, when wine of a dark colour would have appeared darker still.

Πορφύρεος.

A vague term, but equally vague our *purple;* for while we apply it to the foxglove and many other flowers which present an equal mixture of red and blue, we at the same time apply it to a beech, the foliage of which is of a deep copper colour merging into black without any blue in it at all; and in milliners' language to a deep blue without any red in it.

Ἰοειδής.

What was the flower that the Greeks called ἴον, is very doubtful. That which Pindar describes (Ol. vi. 91) as with ξανθαῖς καὶ παμπόρφυροις ἀκτῖσι, with brilliant yellow and richly purple rays, cannot be our own modest violet. I have always supposed it to mean Centauries of different species, some of them, as the *C. ragusina*, of the brightest gold colour, others, as the *C. cyanus*, of a clear blue, and others of a dark purple. The late J. Hogg in his treatise upon the classical plants of Sicily most unaccountably omits all mention of it. At the present day it is the stock, *Matthiola incana*, which in Italy is called *Violetta*. In the above line quoted from Pindar it must have been a radiate flower that he intended. In this respect uncultivated nations are very inaccurate. The Illyrians at the present day call all wild flowers alike indiscriminately *rosje*, roses; and we may be sure that Jesus Christ in his beautiful apologue—"Consider the lilies"—used the language of the people he was addressing, and did not mean lilies in the strict sense of the word; plants that would not burn if cast into an oven on the morrow of being cut down.

It is very strange that Mr. Gladstone in the essay published in the *Nineteenth Century* of October, 1877, has entirely passed over κροκόπεπλος, saffron-robed, an epithet twice applied to Eos, the dawn of day, in the first lines of "Il.," bk. 8 and bk. 19, a word that proves that Homer saw yellow distinctly; for he never calls Eos yellow-fingered, κροκόδακτυλος, or rosy-robed, ῥοδόπεπλος.

In the above it has been my desire to prove that any inaccuracy in Homer's names of colours was due to the unfixed character of the language, and not to a defective vision on the part of the poet. In illustration of this view let me give a case that occurred to me about two years ago. I took to a flower show at Taunton a dahlia of a rather common variety, and such as most gardeners would call purple; a dark pink with a shade of blue over it, and requested forty-four different people to write me down what they would call its colour. In their replies I got fourteen different names for it. I sent a flower of the same kind to a lady who returned me twelve replies from members of her family and friends, and in the twelve were eight different names. How much more then may we expect diversity and inaccuracy in the nomenclature of their colours among the popular poets of an early period! and how little reason have we for believing in any gradual development of colour vision in successive generations of men!

R. C. A. PRIOR

Colour-Blindness

IN answer to Mr. Podmore's question in NATURE, vol. xix. p. 73, as to the appearance to me of the green of the solar spectrum, I may say that such part of it as inclines to yellow is seen by me as faint yellow, and such part of it as inclines to blue is seen by me as faint blue. The line of division, which I may call neutral green, appears simply colourless or white; there is no dark space, no pigments; neutral green appears to me gray.

When I wrote the paper for the *Phil. Trans.* I applied the descriptions to colours obtained by pigments, because that was the mode that had previously been adopted in treating the subject, and I had not, at that time, the opportunity of making any good observations on direct light. At a later period I went through a series of experiments of the kind with an eminent physicist, but I am not aware that the results have been published. I will endeavour, if possible, to supply the desideratum.

WILLIAM POLE

The Colour Sense

THE note of Mr. Grant Allen in NATURE, vol. xix. p. 32, induces me to state that in the year 1877 I arrived at and developed exactly the same conclusions in several articles of the German journal, *Kosmos* (vol. i. pp. 264-275 and 428-433), namely:—

1. The colour-sense manifestly appears already in insects and many of the lowest vertebrates; its complete absence could therefore hardly be supposed in the very lowest race of men.

2. The anomalies shown in the expressions of colours among the most ancient civilised nations by Gladstone, Geiger, and Magnus, may be perfectly explained, partly by the insufficiency of the primitive store of words for this subject, partly by climatic, physiological, and optical reasons, as stated at length in the above-mentioned articles.

3. The usage of telling terms for the single colours closely followed the progress of the art of dyeing. ERNST KRAUSE

Berlin, December 2

History of the Speaking Telephone

As the writer of the article on the history of the telephone, to which so eminent an authority as Prof. Watson takes exception in the long and interesting letter he has contributed to your columns, perhaps you will allow me to say a few words. Prof. Watson expresses his "astonishment at the claim now made that he (Mr. Gray) anticipated Mr. Bell in the invention of the speaking telephone," and speaks of the "erroneous statement of facts" contained in the article in question (NATURE, vol. xviii. p. 696). Unfortunately Prof. Watson has not specified the statements which are erroneous, and appears to have overlooked the fact that the article is a review of the works of Mr. Prescott and M. du Moncel on the telephone, and that the "statements of facts" are chiefly quotations from those works. At the same time, using all the materials within my reach, careful inquiry had led me to concur, and in that article I expressed my concurrence in the following opinion, quoted from Count du Moncel's book:—"Si M. Bell a été le premier à construire et à rendre pratique le téléphone parlant, M. Elisha Gray avait le premier conçu le principe de cet instrument."

Gray and Bell were both exhibitors at the Philadelphia Exhibition, and Prof. Watson, writing as one of the judges of the scientific instruments exhibited, shows that whilst Gray merely submitted to the judges an apparatus for the multiple transmission of musical notes, and no speaking telephone, Bell not only exhibited a speaking telephone, but towards the end of June (1876) the judges, Prof. Watson and Sir William Thomson, obtained with Bell's instrument the clearest evidence of the electric transmission of speech;[1] whereupon Mr. Gray was both surprised and incredulous, and even after the publication of Prof. Bell's discovery, he delivered a lecture exhibiting his musical telephone, but making no mention of a speaking telephone.

If the Philadelphia Exhibition were the only means for scientific publication during the year it existed, Prof. Watson's letter would effectually dispose of Gray's claim. An exhibition, however, is not the place for conceptions, but for accomplished facts, and I believe no one denies that to Mr. Bell is due all the credit of having been the first to construct, and that entirely independently of Gray, an articulating electric telephone. Gray's claim, as I take it, rests on his having registered in the American Patent Office, on February 14, 1876, "a means of transmitting and receiving vocal sounds telegraphically," and the drawing he gives of his invention shows a correct appreciation of the true principle of an articulating telephone, to which his previous researches had been gradually leading him.

I should be sorry to appear in any way to depreciate the splendid achievement of Prof. Bell through having referred to other workers in the field of electric-telephony. In fact up to the time the article in NATURE appeared, I fear that, through ignorance, I had done but scant justice to Mr. Gray, having attributed the conception of the principle of an articulating telephone solely to Prof. Graham Bell.

There are two points in the history of the telephone upon which I should be very glad to have authoritative information from Prof. Watson or other of your American readers; the first relates to the claim made by Prof. Dolbear, and the second to the introduction of the ferrotype diaphragm. W. F. BARRETT

Royal College of Science, Dublin, December 9

[1] I am glad to learn the exact date of the trial in question, which was given as August in the article.

The Formation of Mountains

IN the account of M. Favre's experiments in NATURE, vol. xix. p. 103, I find the following passage:—"It is, in fact, very probable that our globe is at the stage when, according to Élie de Beaumont, 'the mean annual cooling of the mass exceeds that of the surface, and exceeds it more and more.' It must follow that the external strata of the globe, tending always to rest on the internal parts, are wrinkled, folded, dislocated, depressed at certain points, and elevated at others."

The whole theory of these dislocations, &c., thus depends on the assumption that the interior of the globe is cooling more rapidly than the crust. This has always seemed to me an impossibility, and even an absurdity, and I shall be very glad if any of your correspondents will explain how it is possible. I have always understood that the surface of the earth does not now derive any appreciable portion of its heat from the interior; but if the interior is cooling rapidly, to what can it part with its heat but to the crust? Volcanoes and hot springs no doubt allow a certain portion of heat to escape, but it must be an infinitesimal part of the heat of the entire mass. If the meaning of the statement is, that the heat received from the sun now keeps the surface at a permanent mean temperature, quite irrespective of central heat or cold, and that therefore the loss of heat by volcanoes, &c., causes the centre to cool while the crust does not—this may be admitted, but it is doubtful whether it can have any bearing on the effects observed. For, on this theory, all the compression would take place in that shallow superficial layer which is kept above its normal temperature by the sun's radiation; and as we go back into past time this superficial layer would be thinner and thinner. But all geological evidence goes to show that folded and contorted rocks were subject to compression at considerable depths; and further, that such contortion was greater in comparatively early than in very late geological times—both facts directly opposed to the theory in question. Will any one of our great physicists enlighten us? ALFRED R. WALLACE

AFTER reading your *résumé* of Prof. Alphonse Favre's interesting experiments on the formation of mountains by lateral thrust, it occurred to me that it would be easy to devise a mode of experimenting which would more nearly correspond with what takes place in nature. In M. Favre's experiments the lateral thrust was simply in one direction. In nature it is in all directions.

If a disk of india-rubber were stretched by means of a steel ribbon bent into a circular spring, on letting the spring slowly recoil there would be a lateral contraction of the india-rubber in all directions. A layer of clay upon that disk would, I think, show not the transverse inequalities of M. Favre's drawings, but a diversified unevenness more nearly resembling the actual surface of the earth. ARTHUR RANSOM

Leicester, December

New Galvanometer for Strong Currents

I OBSERVE in NATURE (vol. xviii. p. 707) an article on a new galvanometer for strong currents by Mr. Eugen Obach. I published a paper on the same form of galvanometer seven years ago, and inclose a copy of my paper which was published in the *American Journal of Arts and Sciences*, vol. ii., August, 1871. JOHN TROWBRIDGE

Harvard College, Cambridge, Mass., U.S.A., November 23

Explanatory

I MUST ask you, in common fairness, to allow me to protest against P. G. T.'s mistaken statement (vol. xix. p. 71) respecting a sentence which he quotes without the explanatory context. The moving force exerted by the earth on the moon as a whole is of course precisely equal to the moving force exerted by the moon on the earth. I had not to learn this from P. G. T., but had said so in so many words. But the moving force exerted by the earth on a given amount of matter in the moon is eighty-one times greater than the moving force exerted by the moon on an equal amount of matter in the earth. P. G. T. will scarcely deny this, and he cannot deny that the whole statement from which he quotes one sentence meant this, and this only. Nor, if he did, would any one who has read the chapter on the moon's motions in my treatise on the moon, believe such a statement.

He quotes a passage from my last book without comment, but, unfortunately, not without serious alteration. Apart from the undue emphasis which he thus gives to certain parts of it, the passage expresses my honest opinion. That I may be mistaken is quite possible. Men are always misunderstanding each other. If I find I have erred, I will acknowledge as much.

Until the word "heat" ceases to be used in common speech in two senses, or I am shown that when used for "temperature" (as when we say blood heat, boiling heat, a heat of 90° F., and so forth), it can be understood to mean "caloric," I intend always so to use it in familiar writing about science. I deliberately struck out the word "temperature" wherever I had used it, and replaced it by the word "heat," in the same way and for the same reason that I often replace the word "velocity" by the

word "speed." If in any passage ambiguity has thus been occasioned—or, as I would rather say, if anything I have thus said can be mistaken—I shall be glad to hear of it and set it right.

I must have failed, however, to make my meaning clear to P. G. T. in pp. 194 and 240. If at least he rightly understands me, I must leave him to settle with observed facts in one case and with the recognised authorities in the other.

My account of the earlier experiments of Professors Andrews and Tait was taken, as stated, from a paper by Prof. Heaton. P. G. T. ought to know the facts, and I accept his correction. When my article was written, several years ago, the "now received idea" was not yet received. I did not err in calling that theory "beautiful" and "ingeniously conceived" which is now generally accepted. But if I had, it is a less serious mistake to describe a sound theory as still open to doubt, than to describe a doubtful theory as demonstrated. This the author of the sea-bird theory of comets might remember with advantage.

RICHARD A. PROCTOR

Graphic Granite

I HAVE been spending some time of late in the examination of the rocks of this district, and was pleasantly surprised, a few weeks back, at finding some well-marked specimens of graphic granite among the waste material raised from Huel Agar Mine. It very closely resembles that found at Portsoy, N.B., but the felspar is grey instead of red. As I am not aware that this interesting rock is known to exist in any other locality in England, the observation may be worthy of record.

W. End, Redruth, December 2 FRANK JOHNSON

The Phonograph and Vowel Sounds

IN the interesting paper on "The Phonograph and Vowel Sounds" (vol. xviii. p. 340, *et seq.*), the authors remark that although the general results are the same as I have inferred from my own researches, the special numbers expressing the distribution of total intensity of vowel sounds among the partial tones are very different. Perhaps you will have the kindness to communicate to your readers the following reasons explaining, as I believe, the differences mentioned above.

1. The tables given by the authors, which contain the distribution not of intensity but of amplitudes, must be altered in a manner readily seen in order to be comparable with my tables.

2. The marks impressed by the phonograph contain certain peculiarities which, although without influence on the tones spoken from the instrument, remain effective in modifying the form of the curves obtained by mechanically transferring them.

3. The objective intensity (kinetic energy) determined by the authors is nearly, but not quite, proportional to the subjective intensity (quantity of sensation) which I have measured with the aid of resonators.

4. As I have observed, the differences of English and German pronunciation cause remarkable differences in the distribution of total intensity of vowel sounds among the partial tones.

Taking the above points into consideration it will be seen that the differences mentioned by Messrs. Jenkin and Ewing appear much smaller.

Besides I am pleased to notice that the authors, like myself, consider the flexibility of mouth cavity as important in explaining, where it exists, the characteristic pitch and other properties of vowel sounds.

F. AUERBACH

Local Colour-Variation in Lizards

THIS subject has recently been very fully discussed by my friend, Dr. Max Braun, assistant in the zoological laboratory of the University of Würzburg. His paper, which has especial reference to the lizards of Minorca and of some of the smaller islets of the Balearic group which lie round that island, is entitled "*Lacerta Lilfordi* und *Lacerta muralis*," and will be found in Part I. of the fourth volume of Prof. Semper's "Arbeiten aus dem zoologisch-zootomischen Institut in Würzburg," published in May, 1877.

Braun refers constantly in this paper to a memoir by J. von Beidraga, entitled "Die Faraglione-Eidechse und die Entstehung der Farben bei Eidechsen," which was published at Heidelberg in 1876.

P. HERBERT CARPENTER

Eton College, December 9

The Range of the Mammoth

ON November 6 Prof. Boyd Dawkins read a paper before the Geological Society on "The Range of the Mammoth in Space and Time." As the professor and several other recent writers have taken it to be proved that *Elephas primigenius* occurs in pre-glacial beds, it will, perhaps, be as well at once to review the evidence.

Geologists often speak of "pre-glacial beds" when they only mean beds beneath some one boulder clay, perhaps No. 6, or even later in the list given below. The succession is roughly as follows :—

6. Hessel Boulder Clay	Upper, Middle, and Lower of the North of England (?)
Hessel Gravel	
5. Purple Boulder Clay	Upper, Middle, and Lower of Lincolnshire, &c. (?)
Bridlington Crag	
4. Chalky Boulder Clay	Upper, Middle, and Lower of East Anglia.
Mid-glacial[1]	
3. Contorted Drift[1]	Lower Boulder Clay of the Norfolk Coast.
Sands[1]	
2. Second Till[1]	
Intermediate Beds[1]	
1. First Till[1]	
Arctic Freshwater Beds.[1]	
Temperate Freshwater Beds.[1]	Pliocene.
(Land surface.)	
Weybourn Beds, estuarine,	
including the "Forest Bed."[1]	

As the *lower* boulder clay of Northwich, in Cheshire, appears to be No. 5 or No. 6, and consequently newer than the *upper* boulder clay of East Anglia, the molar of *E. primigenius* found beneath it need not be pre-glacial. The Hertfordshire boulder clay, beneath which Prof. Prestwich found a tooth is, I believe, No. 4.

In East Anglia I have seen two molars of *E. primigenius* from the contorted drift, No 3 in the list, but it has not yet been found lower. All the specimens said to come from the forest bed have been dredged or picked up on the beach, and are of no value as evidence. At Bacton, on the Norfolk coast, I dug out a jaw and three teeth of the mammoth from a post-glacial deposit; if the denudation of the cliffs had proceeded these teeth would have been found on the beach mixed with those of *E. meridionalis*. There appears to be one specimen, and one only, found *in situ* in the Forest Bed which can with any probability be referred to *E. primigenius ;* this was found some years ago by Mr. Savin, of Cromer, it has not yet been satisfactorily determined, but from its peculiarity and the difference of opinion about it, it appears certainly not to be the ordinary form.

CLEMENT REID

Egton Bridge, Yarm, Yorks

The Bunsen Flame a Sensitive Flame

IT is not generally known, *if* it has ever been noticed before, that the Bunsen lamp gives a flame sensitive to sounds. A lamp should be chosen which has a tendency to "burn below;" this may usually be secured by opening the air passages to the utmost and lessening the supply of gas. The flame should burn quietly. My most sensitive flame is four inches high; the gas at about one inch pressure of water. A smart tap with a penholder on a glass cylinder a yard from the flame causes the characteristic "ducking," which is sometimes so energetic as to extinguish the flame or to cause it to burn below. The acute sound of rattling bottles, of a glass rod against a beaker, and many such familiar sounds of the laboratory, are the most effective. This may explain burning below without obvious cause. A tap on a mortar with the pestle twenty feet distant from a well-adjusted flame causes it, and so, often unintentionally, we may have the same result.

W. W. HALDANE GEE

Preston, December 3

OUR ASTRONOMICAL COLUMN

JEAN DOMINIQUE CASSINI.—In the course of his examination of the older archives of the Paris Observatory, which had been placed at his disposal with unrestricted permission to make extracts for use in his lunar re-

[1] These will be described in the Geological Survey Memoir on the Cromer Cliffs

searches, in the first instance by Delaunay and afterwards by Leverrier, Prof. Newcomb discovered that the wide-spread belief that Cassini I. was director of the Observatory, which is even stated to have been the fact in French histories of astronomy, is an error. The establishment appears to have been assigned for the common use of the Academy of Sciences, and no such office as that of director was known or recognised. Prof. Newcomb suggests it may have been the celebrity of Cassini which gave rise to the impression that he was director of the Observatory. Of the astronomical records of that time preserved in the archives a large portion were evidently never intended to be understood or used except by the observers themselves. The note-books have no titles, no indications of the observer or indications of the instruments employed, except in the case of clocks: each observer seems to have had his own instruments, without any reference to or comparison with those of others. In the earlier observations no designations even of occulted stars were attached, so that it was necessary for Prof. Newcomb's investigation to calculate the places of the moon as affected by parallax for the times of observation before the objects could be identified, an operation which, though laborious, was always successful except in the cases of a few small stars. Lalande, in his notice of the work of Cassini I., does not call him director of the Observatory at Paris. Louis XIV., he states, applied to the Pope, Clement IX., for permission for Cassini to pass some years in Paris, where the Academy of Sciences was in course of formation, offering him, through Colbert, 9,000 livres per annum for the period of his residence in France. He arrived at Paris on April 4, 1669, and his reception by the French king was such that he wished to remain permanently in the country. The Pope offered opposition at first, which the king succeeded in overcoming, and Cassini was naturalised, and, as Lalande says, obtained a considerable fortune. He commenced observations at the Paris Observatory in September, 1671.

THE SECOND COMET OF 1582.—In the list of cometary radiant-points and meteor-showers in the Report for 1877 of the Luminous Meteors' Committee of the British Association, the distance of this comet's orbit at the descending node from the earth's path is stated to be 0·00, and the conjunction with the cometary shower is fixed to November 9 for 1875. This is a mistake, whichever orbit of those calculated may be adopted for the comet. Pingré, in his *Cometographie*, gave two sets of elements, the second being calculated apparently with the view to bring in Santucci's reported observation of a comet on March 10, of which he was the only observer. Tycho observed the comet on May 12, 13, and 17, and it is upon the observations on these three days that the orbits depend. They were reduced with modern elements by Mr. Hind (*Astron. Nach.* No. 880), and from the resulting positions, D'Arrest calculated elements in 1853. In 1865 Mr. Marth, after rectifying one oversight in the reduction, also computed an orbit, and his elements will no doubt be preferable to the other systems, though they do not differ materially from D'Arrest's in *Astron. Nach.*, No. 891. Mr. Marth's elements are as follow:—

Perihelion passage 1582, May 6·4485 M.T. at Uraniburg.

Longitude of perihelion	255 16 43	Equinox
" ascending node ...	227 13 33	of 1582.
Inclination	61 25 51	
Log. perihelion distance	9·22716	

Motion—retrograde.

Whence the comet's radius-vector at descending-node is 2·87, or the comet is far outside the earth's orbit at that point in its path. The radius-vector at the opposite node is 0·18, so that there is no near approach to our track.

The comet of 1582 was observed by the Chinese for about twenty days from the day of discovery, May 20. Their annals have no reference to Santucci's comet, the existence of which is doubtful, but we follow Pingré in designating Tycho's comet—the second of the year.

GEOGRAPHICAL NOTES

AT the meeting of the Geographical Society on Monday evening, after a few remarks from the Earl of Dufferin, who occupied the presidential chair for the first time, Mr. C. R. Markham, C.B., read three papers on Arctic matters. In the first he treated of the Swedish expedition, of the progress and results of which we have kept our readers informed. The second paper was devoted to the Dutch expedition in the *Willem Barents* during the past season, the results of which are by no means insignificant. Experience of the ice movements between Spitzbergen and Novaya Zemlya was acquired, and a full hourly series of meteorological observations taken, as well as deep-sea soundings with serial temperatures and magnetic observations. Natural history collections were also made, and Mr. W. G. A. Grant, who was in the *Pandora* in 1876, succeeded, in spite of almost constant fogs, in completing an excellent series of photographs. Lastly, Mr. Markham dealt with the best route for future Polar discovery, which he considers to be along the west coast of Franz Josef Land.

WE have to hand a report on the results of the preliminary polar expedition conducted by Capt. Tyson in the *Florence*. The vessel wintered from October 10, 1877, in lat. 66° 13′, at the head of Cumberland Gulf, Amisto Harbour. The published maps locate this place 2° further north. Last winter was very severe, almost an unbroken succession of storms of rain and snow. The *Florence* was not frozen in until the latter part of November, and after the middle of May the ice was unsafe. The coldest temperature was −52° F. on January 21; the highest, on June 9, was 55°·5. The longest period of cold was from March 5 to 13, when the thermometer averaged about −40°. The variations of the thermometer were often from 6° to 8° in a single hour, when the wind was veering. The site had never been visited by any naturalist, but the fauna was found by Mr. Kumlein almost identical with that of Baffin's Bay, with only a few notable and apparently unaccountable exceptions. Some North Pacific species and one European were found by Mr. Kumlein, Birds do not occur in any notable number, except perhaps the eider-duck. Some rare eggs were procured, and a good series of skeletons of young and fœtal seals. The flora appears to be extremely meagre. The same species were collected on the Greenland coast, in lat. 70° N., much more luxuriant and abundant. A considerable collection of lichens was made, and a good series of algæ was collected. Only ten species of fishes were met with, some of them, it is true, of interesting forms. The family collected are of Silurian age. Esquimaux skulls and old implements were procured.

A GOOD deal has been said of late as to the practicability of opening trade-routes to the Chinese province of Yünnan from the side of Assam, Burmah, Tonquin, and even Russian Turkestan, and it is, therefore, not uninteresting to note from the Pakhoi Consular Report that the Chinese have a route thither through the south of the empire, of which we believe foreigners have not heard before. Communication between Pakhoi, on the southern sea-board of Kwang-tung, near the Tonquin frontier, and the province of Yünnan is carried on by the west River pass Nanning-fu to Peisi-ting, thence over the mountains, or still by the West River, in very small boats, to Kwangnan-fu, in Yünnan. In addition to the natural difficulties of the route, the border-land of the two provinces is a wild and lonely region, infested by bands of robbers. The valuable trade which will some day spring up with the rich south-eastern part of Yünnan, not devastated during

the Mohammedan rebellion, will, no doubt, take the route of Haiphong, as it is shorter than that of Pakhoi, and has practicable water-communication throughout its whole length.

THE latest news from Dr. Gerhard Rohlfs informs us that he is at Tripoli, under the protection of the French consul. He was to proceed to Wadai on December 15, and was daily expecting rich presents from the Emperor of Germany, which he was instructed to offer to the new sultan of this remote kingdom.

DR. CREVAUX, the explorer of French Guiana, has reached the source of the Oyapok, and crossed the range which separates Oyapok from the Amazon basin.

L'Exploration, of December 7, contains several interesting letters from M. Soleillet, who, it will be remembered, started some months ago from the French West African settlements for exploration of the African interior. His last letter is dated from Kouniakary, the capital of Segou, from which he hopes to reach Timbuctoo.

ACCOMPANYING an article on the Bolan Pass in the December number of the *Geographical Magazine* is a map of the Bolan, the Mula, and other Passes, by Mr. Trelawny Saunders, an admirable specimen of accurate and carefully executed cartography.

ARE THE FOSSIL FLORAS OF THE ARCTIC REGIONS EOCENE OR MIOCENE? AND ON THE CAUSES WHICH ENABLED THEM TO EXIST IN HIGH LATITUDES[1]

THE question of the conditions and their origin under which floras, presumably requiring a temperate climate, were enabled to exist in Polar regions has been so frequently discussed both before and since the recent Arctic Expedition, that it is strange to find any aspect of the subject having remained unnoticed.

There are some points, however, which can be by no means regarded as conclusively settled, and upon these I wish to say a few words. In the first place the age of the so-called miocene floras has, I believe, been wrongly interpreted. Again, the temperatures required by them may have been over-estimated. Lastly, there seems to me to be no occasion whatever to invoke astronomical causes or to invent recurring periods of heat and cold, of which we have no geological proof, in order to account for their former presence there.

In the first place, then, if we examine the palæontological evidence, the only kind on which the age of the rocks has been decided, we find that it is very far from conclusive, and instead of pointing to a common miocene age for all the tertiary beds in the Arctic regions, we find that there are many reasons for believing some of them at least to be eocene. The plant evidence is, indeed, in the present state of our knowledge, almost negative; but what similarity there is in the floras to those of the miocene is directly against their being of that age instead of in favour of it, *for no two floras which are much alike and met with in widely separated latitudes can possibly have been contemporaneous, although floras of quite distinct facies may have been so.* The fact that a proportion of the plants have been identified by Heer with those of the miocene of Switzerland is of no weight whatever, as a considerable proportion are equally identified with the undoubted eocene lower lignitic of America, and a number of forms in the latter again with the miocene of Switzerland. The truth is, that at present any formation containing dicotyledons may be, with almost equal plausibility, referred to either eocene or miocene to suit the author's requirements; for besides the similarity in the ovate and lanceolate leaves from both, many species actually range right through them. Were the age of the Alum Bay and Bournemouth beds not thoroughly established, from their fossil leaves, even they would certainly have been referred to miocene. This will not, I am happy to think, always be the case, for there are a great number of plant forms which appear to be decidedly typical of, and confined to, each stage respectively. Already a number of hitherto supposed miocene deposits in Europe have been recognised as eocene, and as soon as those forms which from their range are of negative value, have been set aside, the confusion will cease. I have, indeed, strong hopes that we may be able to recognise each stage in the eocene of temperate latitudes by its plants, more from the incoming of new and distinct types, however, than the continued presence of older forms.

The confused way in which floras of many ages have been mixed together, seems to me to have arisen in a very simple manner. The flora of Oeningen must undoubtedly be accepted as a typical miocene flora, and contains but very few eocene forms—if indeed any. It contains, however, many plants common to other isolated fragments of strata which contain mixed floras, that is, floras with percentages of eocene as well as miocene plants. There being no typical series from undoubted eocene available as standards of comparison, the plants common to the miocene have alone been taken to determine the age of these beds, and the unknown eocene forms have thus been enrolled as miocene, and in their turn used to identify other still more distinctly eocene beds as miocene; much in the same way that the Barton beds were formerly identified, from their possessing a few species in common, as London clay, and the species peculiar to the Barton horizon, subsequently made use of to identify *calcaire-grossier* and Bracklesham beds in their turn with the London clay.

The oldest Arctic flora containing dicotyledons, and which, therefore, has any bearing on the subject, is that of Kome in North Greenland. It is mainly composed of ferns and gymnosperms, and its age may probably have been correctly inferred. The next beds, referred to the upper cretaceous, appear to be approximately of the same age as the Dakota beds, and therefore, in my opinion, decidedly supra-cretaceous relative to that formation in Europe.

We have next, if Heer's nomenclature is right, an immense gap right from the cretaceous to the miocene, to which latter he has referred all the rest of the obviously newer beds. He does not account in any way for the absence of eocene deposits, and relies exclusively on plant evidence, which I think should be, for the present, absolutely set aside. Before quitting the subject of the plants, I wish it to be understood that I in no way intend to disparage Heer's judgment. Considering the nature and condition of the specimens submitted to him, it is a marvel that he could have drawn and described them in so admirable a manner. But Heer has unfortunately never had a large series of definitely eocene plants to describe, and hence almost all his floras are cretaceous or miocene. I profoundly respect his work, and only to strengthen my plea that leaf evidence may be temporarily set aside or reconsidered, I mention that already three floras pronounced by Heer to be miocene, have since proved one cretaceous and two eocene; and still others must follow.

Assuming that I may be allowed for the present to dismiss the plants, I will touch upon the purely physical evidence, which seems to me to be, small as it is, entirely in favour of the eocene age of the beds. In the first place we have the great probability that eocene remains would, if they had ever existed, be found in their proper sequence, considering the number of widely-separated places in the North Polar area from which plant-remains have been obtained. It is certain that they must have existed, since the area continued land throughout eocene times, for there are no marine deposits of that age; and there is

[1] This was written before attention was called to Saporta's address at the anniversary meeting of the Royal Society.

abundant evidence of more than one kind that Europe and America were connected then, and that animals and plants passed between them. Besides that, the floras themselves contain both American and European types, and during the middle eocene a great number of plants were common to both continents.

In the next place the temperature of the eocene period in Europe was much hotter than that of the miocene, and therefore presumably more favourable to the growth of such floras in northern latitudes. To call them miocene we have to admit the former existence of a climate sufficiently uniform to have enabled the same species of plants to grow simultaneously from Italy and the United States to the 70th parallel, a state of things not in accordance with our present experience of plant distribution. But if we assume them to be eocene, the decreasing temperature which prevailed from that time to the miocene would have gradually and naturally driven the forms southward, and thus the very similarity of the miocene floras of America and Switzerland to those of the Arctic regions, renders it most unlikely that they were of the same age, and almost certain that the latter were considerably older.

In comparing the eocene and miocene temperatures we find, as already stated, that the former most readily accounts for the growth of temperate floras in high latitudes. Taking Heer's estimate that the miocene temperature in the latitude of Switzerland at the sea-level was only 9° C. warmer than at the present day, the progressive decrease of heat to the north is not so much in accordance with that of the present time, as it is found to be on the supposition that they belong to the eocene; we have to suppose that the mean temperature diminished in a less degree.

Speaking roughly, the present decrease in the isotherm from latitude 50° of south England, to that of Spitzbergen, is about 10° Fahr. for every 10° of latitude. This is as nearly as possible the ratio of decrease between England and Greenland in eocene times as implied by the floras, supposing them to be of one age. If, as I assume from all the data I can collect, England in middle eocene times possessed a mean annual temperature of 70° F., Greenland would naturally have had one of 50°, which is that assigned to it by Heer (9° C.). The decrease thence to Spitzbergen and Grinnell-land is hardly less rapid, being about 1° F. (½° C.) of cold for each degree of latitude. Heer calculates—principally on the mean temperature required by Platanus—that between Greenland and Spitzbergen, 8° lat., it was 4° C., but very unaccountably fancies that between Spitzbergen and Grinnell-land no further decrease took place, and upon this assumes that trees might have extended to the very Pole itself. The evidence against it, however, seems perfectly clear, for all the planes and limes, and more temperate forms have disappeared, and the genera found there, with a single exception, have representatives which at the present day live within the Arctic Circle. This exception, Taxodium, judging from its present habitat, Mexico and the Southern United States, would necessitate a climate completely different from that required by all the other plants with which it is associated. They form a distinctly Arctic assemblage: the spruce, especially, is never met with fossil to the south, except in inter-glacial beds. The fossil Taxodium must, therefore, only be looked upon as an allied extinct species, whose resemblance to living forms does not imply identity of habit, since all other considerations are against it. A similar instance is found in the willow, which is generally characteristic of the north; yet *Salix humboltiana* is found in the Amazon districts and *S. safsaf* in Egypt; and similarly, although Cassia is eminently characteristic of tropical and subtropical zones, *C. marylandica* flourishes on the banks of the Lake of Geneva. As it is essential to get rid of the evidence of Taxodium, if we are to suppose this former climate followed the present natural laws, I shall refer to some remarks by Lesquereux upon the nearly allied redwoods.

In describing the pliocene plants of California, he concludes that they are related to the present flora of the Atlantic slope, and not to that of California. He accounts for their destruction on the eastern side by the powerful agencies of glacial action, marine submersion, and long-sustained volcanic cataclysms. When these had ceased the sheets of water between the Missouri River and the Rocky Mountains and the mountains themselves prevented the old flora from again occupying the Western area. Some of the pliocene species, however, were preserved through the glacial epoch in California, but modified, for the most part, by the cold conditions they had undergone. "The two species of *Sequoia*—one the more predominant, the other the more remarkable, of the flora of California—are evidently also remnants of the pliocene. *S. gigantea*, which in all probability covered the higher slopes of the mountains of that epoch, has been destroyed everywhere, except in some deep valleys. . . . The other, *S. sempervirens*, left here and there, has again taken the ascendency under more favourable physical circumstances. Its present distribution explains its preservation until the present epoch. According to Prof. Bolander, "the distribution of the redwood depends upon sandstone and oceanic fogs. Where either one of these conditions is wanting there is no redwood. The redwoods begin in the northern part of Monterey County, in isolated groups, in deep, moist cañons. A short distance south of Monterey City, on the Monterey Bay, a white bituminous slate sets in, and extends nearly to Pajaro River. On this no redwood is found, but *Pinus insignis*. At Pajaro River, eight to ten miles from the ocean, they set in again, and extend to nearly twenty-eight miles south of this city (San Francisco), either in deep cañons, or in groves extending over several ridges eastward, as far as the fog may reach. Then they continue in similar localities to latitude 42°, the state boundary."

The existing allied species withstood a glacial period in California; there is no improbability in supposing that older and extinct species may have habitually supported a cold temperature. It appears that they belong to a very old type, now confined to a limited area, and becoming extinct, at whose survival we cease to wonder when we reflect that individual trees have been calculated to be 3,000 years old. To pass through the life of such a species, an enormous period must be required, for only 100 generations might carry us back 300,000 years, with as little modification as an annual plant might undergo in 100 years. The sandstone soil and damp sea fogs required by them in their native habitat, may explain the difficulty in getting them to grow under cultivation except in comparatively warm latitudes—and it is upon plants under cultivation Heer's estimate in Europe is based—but lessens our surprise that they should have existed in Greenland or farther north during the eocene time.

Apart from Taxodium, therefore, there is every evidence, in the disappearance of temperate forms and the preponderance of conifers of boreal type, that, as at the present day, there was a natural and progressive decrease of temperature to the north between Grinnell-land, Spitzbergen, and Greenland.

In the next place I would call attention to the possibility that the respective temperatures thought to be requisite for the growth of such associations of plants as are found fossil in these various lands may be in excess of the minima which would have sufficed. If this were the case, it would of course remove to a slight extent an argument I have just brought forward against the miocene age of the deposits. One of the conditions peculiarly favourable to the growth of trees in northern latitudes is the protracted length of the summer days, and it is an

ascertained fact that they require less heat in latitudes above 60°, owing to this rapid lengthening of the days. The chemical action of the sun's rays seems in some way to compensate for feeble warmth, and vegetation receives more impulse from the presence of the sun than from temperature in the shade. As examples of this, De Candolle[1] mentions that *Fagus sylvatica* exists in the north with a less temperature than it can support elsewhere; and that the limits of growth of barley prove the point conclusively.

It appears certain, according to De Candolle, that in very few cases has even intense cold, during natural periods of rest, any injurious influence upon plants, and that their northern limits are not determined by excess of cold but by want of heat. The destructive agents are late spring frosts, or premature heat followed by chills; and so fatal are their effects that one week in May has killed entire stocks of sub-tropical plants which had stood considerable frost in winter. There is no doubt that many plants would grow in much colder latitudes if the temperatures of each month were cyclically regular. *Fraxinus excelsior*, L.,[2] supports great cold, especially when accompanied by fogs, and penetrates as far north as 64°. *Ilex aquifolium*, L., reaches latitude 62° in Norway, and, like Abies, is limited in range, not by excess of cold, but want of heat. *Evonymus europæus*, L., is found just within 60°, and must occasionally suffer intense cold. But perhaps the well-known *Chamærops humilis*, L., affords the most striking familiarly-known instance of capricious distribution. It is indigenous to Nice in latitude 40°, yet it is not found anywhere in Italy, with a trifling exception, until Calabria is reached. Under cultivation it bears a very considerable amount of winter frost, the limit of which I have not ascertained, nor the minimum it encounters at Nice. I merely mention these instances as indicating possible sources of error, for were Chamærops extinct and found fossil at Nice, we should infer from it, with every appearance of probability, that the temperature of Nice had been the same as that of Sicily or Granada, the more normal homes of the palm.

One of the most remarkable facts connected with Alpine or Arctic plants is the length of time they can endure the absence of light while they are covered with snow, and when thus protected they would be unaffected by even Arctic cold. Evergreens, as we see by the Alpine rhododendron, are equally unaffected, and I have in Switzerland seen laurels, bays, and acubas shrouded in snow for many weeks without injury. I will mention but one other instance of the extent to which trees will sometimes bear cold, quoted by Herschel.[3] "In the valley of the black Irkut, in Siberia, Atkinson found a ravine filled with ice, and with large poplars growing in it, with their trunks imbedded 25′ in snow and ice, while the branches were in full leaf. Around each stem was a hollow of 6″ thawed and full of water." Besides mere heat and cold there are many influences known and unknown which limit the range of plants. The distribution of the vine is a case in point, for it is well kown that in historic times it was extensively cultivated in England, Normandy, and parts of Prussia, in which it will no longer ripen its fruit.

While the winter temperatures in these Arctic regions, if accompanied by snow and fogs, may have been of extreme severity, the summer temperature need not have been high, for the present Arctic and Alpine plants, including roses, species of Betula, Salix, Empetrum, Vaccinium, and conifers need but little heat.

Having attempted to show that the amount of heat really required was not so large as has been imagined, I will endeavour to prove that it, even upon Heer's assumption, might easily have been furnished by physical causes which we know did, in all probability, exist in eocene time, and were quite independent of astronomical causes and change in the position of the earth's axis, of which there seems to me no proof whatever, geological or otherwise. At least, to qualify this assertion, if such have existed in the past, there is no need to invoke them in this particular case. Central heat may, of course, be dismissed as having had too little influence in eocene time to be appreciable.

We may roughly estimate, on Heer's basis, that the average temperature between the latitudes and longitudes of England and Iceland was not more than from 15° F. to 20° F. warmer in eocene (or miocene of Heer) times than it is at present, and we may assume also on the evidence we possess that the present climate would permit any of the eocene floras, supposing they still existed, to grow in latitudes not more removed from those in which they are found than 15° to 20° farther south. For example, the English eocene flora could now exist in Madeira, the Iceland eocene flora in the Isle of Wight, that of Spitzbergen in Sweden, and that of Grinnell-land in Northern Norway. We have therefore to seek for some cause adequate to produce a difference in the temperature of Greenland, for instance, equal in degree to that of 20° F. or 20° latitude as a maximum. Following upon a map the isotherms of the 70th parallel, we see that Prince Albert Land has a temperature of but 5° F., whilst Lapland, in the same latitude, has one of 32° F. There is evidently here a cause at work capable of influencing the temperature to the extent of 27° F.; therefore a more powerful cause than is required. The same map shows us obviously that this agent is the sea. Wherever the Arctic waters find egress or penetrate the land, the isothermal lines around the Pole are deflected south. In like manner the line denoting the limits of trees is in many places pushed back more than 10° S. by the ice-laden water flowing from the Arctic Ocean. From Lapland to Siberia it is, except for a short distance, within the Arctic circle, principally within the 70th parallel. Nearing Behring's Straits it is sharply deflected south by the Polar Sea, but away from its influence, it as suddenly rises and again (North America) far overlaps the Arctic circle, until it once more comes under the influence of the cold seas and channels penetrating south into Hudson's Bay, which drive it to below the 60th parallel. Avoiding Greenland, it includes part of Iceland and the whole of the North Cape, owing to the influence of the Gulf Stream. The influence of this warmer water, cold as it is here, is no less remarkable, for, by merely shutting off the Arctic currents from close proximity to the shore, it enables trees to grow on the coast, and at a point on the Arctic circle between Iceland and the Norwegian coast, raises the temperature, according to Herschel, full 20° above that which is normal to the latitude.[1]

We thus see that the limit of trees enters the Arctic circle wherever the land has a great extension south or where the Gulf Stream raises the temperature, but that it especially shuns wherever the Arctic waters penetrate the land, even in the smallest gulfs or bays. The lands between Hudson's Bay and Davis' Straits, cut up by water, and the islands in the Arctic Ocean surrounded by water, are intensely cold and destitute of trees—almost of vegetation. The cause of Greenland's being shrouded in ice is its unknown and exceptional extension towards the Pole and the increased height of land in its northern portion. These appear to be necessary conditions of such complete glaciation as we there see, as shown by the absence of an ice-cap in Grinnell and other equally northern lands. The present condition of Greenland is wholly abnormal, and, presenting such unusual conditions, has heightened the astonishment felt when the former mildness of its climate became known.

If we were able to shut off from the Atlantic the enor-

[1] De Candolle, "Géog. Botanique," vol. i., 1855.
[2] Examples used by De Candolle.
[3] "Physical Geography," p. 312.

[1] "Physical Geography," p. 232.

mous Arctic currents which chill it, we should produce at once a greater increase of heat than is required by the floras. If, further, we impinged the Gulf Stream upon its shores, without cooling it down by floating icebergs upon its back, we might be able to induce at least an even more temperate vegetation to grow there.

Water is thus seen to be the great factor in distributing heat and cold in northern regions, and not land, as has been generally taught. Humboldt believed the rigorous climate of America to be due to high land stretching to the Pole; Lyell taught that with great polar seas and an excess of land at the equator, the hottest conditions possible on the globe would be produced, and that with land at the Poles and a great equatorial sea, the coldest conditions would ensue. A study of the isothermal lines leads to the contrary belief that the presence of land at the Pole, even if ice capped as Greenland is, would be less productive of cold than a polar ocean with free exits, for air has not the distributing power possessed by ocean streams, and when these are ice-laden the effect is still greater.

It only remains to call attention to such proof as we have, that these conditions really did exist in eocene time, and that the Arctic currents were actually shut off from the Atlantic in those days by continuous land which connected the two continents of Europe and North America. In the eocenes of Europe and North America we have evidence of a great, and, comparatively speaking, sudden rise of temperature, and this was followed in due course by a mingling for the first time of the floras and faunas of the two continents. That there was land communication to the north is further evidenced by the occurrence of types of both kinds in the floras now found upon the spots on which they grew. In further support of this theory we have the fact that no trace of sea-deposit of eocene age has ever been found in the polar area, all the vestiges of strata remaining showing that these latitudes were then occupied by dry land.

If we may assume that these conditions really did prevail, and that all the outlets into the Atlantic were closed by the elevation of the present sea-bed between 60° and 70° (where, I believe, the sea is even now shallower) into land of moderate elevation; with or without prolongations south to the 50th latitude; and the north of Greenland submerged, a temperature would ensue more than adequate to support all the plants yet found fossil in eocene or miocene Arctic beds. The result would be that the zone of greatest heat would be far north of the equator; for while the southern hemisphere was still cooled by the Antarctic currents rising to the surface, the North Atlantic would be practically a landlocked sea, cut off from southern cold by the tropics, from northern cold by land, and heated by the sun like the Gulf Stream or Red Sea. There is no need to suppose that the Gulf Stream washed its northern shores, for the temperature would then be raised in excess of what is required, but its aid may be called in to account for the even warmer previous periods evidenced by the older growths of Gleichenia and cycads.

It does not necessarily follow that cold did not then exist towards the Pole. Disko is 20° distant from it, and with an inclosed polar sea we should have a rapid lowering of temperature on the northern shores of the wide belt of land, and might have even a frozen ocean, perhaps as at present, with outlets on the side of Behring's Straits. The assumption that forests stretched to the Poles is not supported by the evidence.

The high temperature in these latitudes would be confined to the Atlantic; and that it was under the same laws as at present seems a reasonable supposition, since the American area even then maintained a relative coolness on account probably of the return and cooler currents being sheared to the west by the rotation of the earth.

To recapitulate. I believe the evidence to be in favour of the eocene age of the Arctic floras in question, and not miocene. I think that the temperature acquired by the plants—especially taking into consideration that their affinities with genera belonging to temperate regions is only inferred upon, in many cases, indistinct fragments—may have been over-estimated. There is no inherent impossibility indeed, that these extinct forms may not be the relics of a flora, like our present Arctic flora, specially adapted to bear a rigorous clime, and colour is lent to this by the abundance of the extinct McClintockia, about whose affinities we know nothing—a flora, perhaps, merely requiring the protecting cover of snow and sea-fog during winter. Finally, I believe that a comparatively slight change in the relative distribution of land and water, such as I have described, would alone account by itself for any fluctuations of temperature, of which we have any record preserved, in, at least, the tertiary rocks.

It does not come within the scope of the present subject, but it is worth consideration, whether wider channels still than those we now possess—some flowing from a more easterly point, so that our land might form the western coast of such a current—would not produce a glacial epoch, intensified by the absence of the Gulf Stream when there was no connecting isthmus (of which there is evidence in recent days) between the two Americas. The present distribution seems, at all events, one productive of more than average cold, as we become aware through the geological record, for the many and wide-existing channels conduct the Arctic waters south, and lower the general temperature of the ocean even to the Tropics.

J. STARKIE GARDNER

ON GAUSSIN'S WARNING REGARDING THE SLUGGISHNESS OF SHIP'S MAGNETISM[1]

Practical Rule and Caution

1. AFTER steering for some time on westerly courses expect—

1. (*a*) Westerly error if you turn to the north;

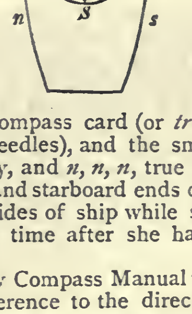
1. (*b*) Or easterly error if you turn to the south.

2. After steering for some time on easterly courses expect—

2. (*a*) Easterly error if you turn to north;

2. (*b*) Or westerly error if you turn to the south.

The diagram representing case 1 (*a*) illustrates the physical explanation, N and S representing the north and



south points of the compass card (or *true south* and *true north* poles of its needles), and the small letters, *s*, *s*, *s*, true southern polarity, and *n*, *n*, *n*, true northern polarity, induced in the port and starboard ends of deck beams and port and starboard sides of ship while steering east, and remaining for some time after she has been turned to north.

In the "Admiralty Compass Manual" Gaussin's warning is given with reference to the direction of swinging, in correcting the compass by magnets according to Airy's

[1] Being an abstract of a Communication by Sir Wm. Thomson, F.R.S., to Section A of the British Association at its last meeting (Dublin).

first method. In the Reports of the Liverpool Compass Committee and in Mr. Towson's "Information for Masters and Mates regarding Ship's Magnetism," instances of perplexing changes in the compass are given, and are referred to the same cause. The "sluggishness" of ship's magnetism, according to which it depends generally in part on the influence experienced some time before the time of observation, and not wholly on the influence at the time, seems to have been first definitely noticed and discussed scientifically by Sir Edward Sabine in his analysis of the results of the magnetic observations in the Antarctic Exploring Expedition of Sir James Ross in the *Erebus* and *Terror*, in the years 1840–41.

The practical rule and caution given above is of great importance in the navigation of iron ships. The amount of the error which may be found cannot be predicted for ships in general, nor for any particular ship except after much experience and careful observation. A small effect of two or three degrees,[1] such as that referred to in the Admiralty Manual as found in M. Gaussin's experience, may be observed in the course of quietly swinging a ship by hawsers or steam-tugs. If the ship under way is steamed round on the different courses the amount of the "Gaussin error" may generally be greater than if she is hauled round by warps; but we must not be sure that it will be so, because the *shake* of the screw which enhances the magnetisation on the east or west courses may shake it out again before the observation is made on the north or south courses.

A good practical rule in correcting the compass is, after having got it quite correct on the north and south courses, correct just half the error which is found after that on the south and north course, in the regular swinging of the ship.

The warning at the head of this article is particularly important for ships of war after firing guns when on easterly or westerly courses; if the course is then changed to north or south, and particularly if, after the firing, the change of course is effected under canvas, without the shaking of the ship's magnetism produced by the engines and screw.

The warning is also very important for ships steaming through the Mediterranean eastwards or westwards, and then turning south through the Suez Canal or north round Cape St. Vincent; and for ships steaming eastwards from America and then turning northwards or southwards into St. George's Channel.

MATHEMATICAL DRAWING INSTRUMENTS[2]

IN his preface the author states that we nowadays expect to find somewhere in print an account of the little mysteries of any particular art, and that partly with the hope of enabling this expectation to be fulfilled, and partly to meet the constant inquiries made respecting certain of the more complicated instruments manufactured by him, he has written his book. The author offers as an apology for any shortcomings in his work, that he is conscious his powers are greater with the lathe and file than in the ways of gentle rhetoric. In our opinion this is unnecessary; we would rather have had the file marks more distinct, and the technical details of construction not so carefully polished out in the work before us as in the well-finished instruments for which the author is so well known. The drawing instruments in most common use, pen, compasses, and dividers, are first described, and the patterns most recommended are illustrated; the type of these instruments now in use seems to meet all requirements, and if of the best quality, appears to require little or no improvement; the needle-point, however, shown on p. 34 (Fig. 1), adapted to compasses or pricker, is an improvement in steadiness on the old form, which was always liable to a little play. The earlier chapters will probably be of use to the beginner in facilitating his choice of the requisite instruments for his work, but he must recollect that dexterity in their use,

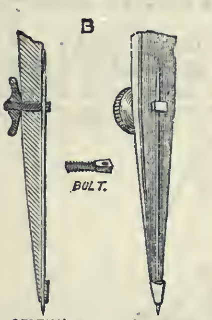


FIG. 1.

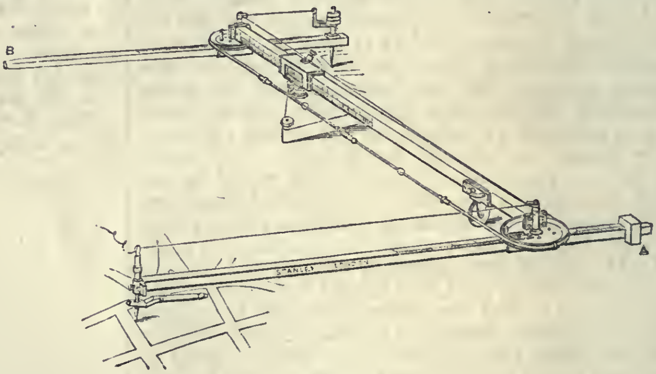


FIG. 2.

even if of the most improved form, can only be acquired with very considerable practice; much time and temper may be saved by the use of good instruments, and there is nothing particularly meritorious in the production of good work with bad instruments if good ones are within his reach.

Of the more complicated instruments next described, some must be regarded rather as mechanical curiosities than of every-day use; others, however, are indispensable where accuracy and the saving of time are of importance; as chief among these we select the eidograph and planimeter. A plan can be reduced or enlarged by dividing it into small squares and filling the details contained in each square into the corresponding squares ruled on the sheet prepared for the copy. This is a tolerably rapid process when the plan is simple in character, and with the help of proportional compasses a good draughtsman may attain considerable accuracy, but for a complicated plan or where great exactness is required, either the pentegraph or eidograph is indispensable. The author justly expresses astonishment at the little use at present made of the eidograph as compared with the pentegraph; the latter as made in this country appears for large work a most clumsy contrivance, offering much unavoidable resistance to motion, and even if made on the most improved Continental pattern is much less handy than the former. The eidograph, as improved by the author, is shown in the cut (Fig 2).

[1] Much greater effects than this are actually found in the cases of gun-practice and of long steaming on easterly or westerly courses referred to below.

[2] "Mathematical Drawing Instruments," by William Ford Stanley. (London: E. and F. N. Spon, 1878.)

The main bar is supported and turns on the pivot carried by the triangular weight; its position with respect to the pivot is adjusted by sliding longitudinally in the box fitted with clamping screw and vernier. On vertical pins at the ends of the main bar turn the two equal pulleys shown; attached to these on their under sides are small boxes also fitted with verniers for the longitudinal adjustment of the two transverse bars. On the similarly situated ends of the transverse bars the tracer and pencil point are carried. When the permanent adjustment of the instrument has been made the transverse arms are parallel, and the pulleys being of equal sizes any rotation given to one communicates an equal rotation to the other by means the flat steel band passing tightly round both; thus the parallelism of the arms is maintained in any position. If now the temporary adjustments are so made that the ratio of the two parts into which the axis of the pivot divides the main beam is equal to ratio of the lengths of the corresponding transverse arms measured from the axes of the pulleys to the pencil and tracer, it is evident that each of those latter is at the apex of a similar triangle, and that the line joining them passes through the axis of the main pivot. Thus the path described by the pencil point is similar to that described by the tracer. The graduations on the bars provides the means of setting the instrument in the required ratio. In the old form the distance between the axes of the pulleys was divided into 200 equal parts, the graduations reading each way from the centre. The transverse arms were made of equal length divided into 200 parts, also reading each way from the centre. For enlargement the setting would be on one side of the centre in each of the three bars and for reducing on the other side. In the improved form shown the tracer and pencil are made interchangeable, and thus the graduation on one half only of each bar is required, while at the same time part of the half arm, B, is dispensed with, making the instrument more handy. In the figure the instrument is set for reducing. The setting is obtained as follows:—Let $\frac{a}{A}$ be the ratio of the scales of the original and reduced plan, and x the reading on the graduations, then for the similar triangles we have $\frac{100 - x}{100 + x} = \frac{a}{A}$, or $x = 100. \frac{A - a}{A + a}$. The chief improvements introduced by the author in the construction of the eidograph consist in making the pencil and tracer interchangeable, which is a considerable simplification, and the introduction of the small roller under the larger arm of the main beam. The improved instrument is stated to be capable of making a reduction down to one-eighth, while the old form certainly became unmanageable at anything beyond one-third.

There is perhaps no instrument whose true value is so little known in the drawing-office as Amsler's Polar Planimeter. The accurate measurement of an area bounded by curved or irregular lines is daily required; and although this can be effected readily and correctly by the aid of the polar planimeter, it is usually laboriously performed by cutting up the area into triangles whose areas are separately determined, or by the measurement of ordinates.

The instrument may be described with the assistance of the figure (Fig. 3). The weight retaining the pin below it at a fixed point, forms the centre about which the more distant arm revolves; to the other extremity of this arm is pivoted a rod carrying a tracing-point at its free extremity. A small roller is mounted on this rod so that its axis is in a line passing through the tracer and pivot at its ends. The roller is provided with a thin projecting edge and is retained in contact with the paper and free to rotate on its axis during any motion given to the instrument. The rotation of the roller is read off from the graduations on its rim by means of a vernier, the number of whole revolutions being shown on the small dial driven by a worm wheel and screw-pinion on the roller-axle. Any motion on the surface of the paper that is given to the point of contact of the roller is resolved into two components, one at right angles to the axis of the roller which is recorded by the dial and vernier readings, and the other parallel to the axis which is a sliding of the roller longitudinally, and is not recorded. To measure the area inclosed by a boundary line as shown in the illustration, the tracing-point is adjusted to any point of the boundary, the dial and wheel are then read off; the tracing-point is then carried round the boundary-line, carefully following it throughout until the starting-point is again reached. The dial and roller are then read off, and the difference of the readings gives the actual area in square inches, or any other units for which the instrument has been graduated. We may now attempt an explanation of the principle of the instrument. Consider first the motion of a straight line parallel to itself. The motion of the centre of the line is compounded of a motion at right angles to it, and one in the direction of its length. The area passed over by the line is equal to its length multiplied by the distance travelled by the centre at right angles to its length.

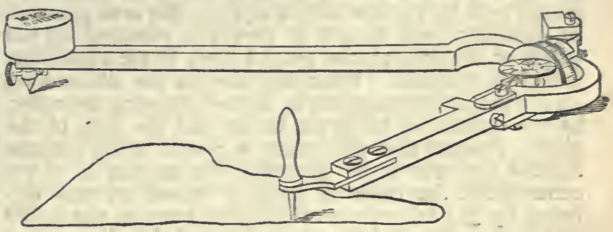


Fig. 3.

If, however, the line be moved, not parallel to itself, but into any other position, it could have been made to reach this position by first moving parallel to itself until its centre reached its new position, and thus, by a rotation of the line about its centre as a fixed point, it could be made to assume the position sought. If a figure representing this be drawn it will be seen that, when the movement is small, the area passed over by the line is approximately equal to its length, multiplied by the perpendicular distance traversed by its centre, as before, and that, when the movement is diminished indefinitely, the area described is in the limit equal to the length multiplied by the perpendicular distance traversed. Now let a roller be mounted at the centre of the line, so as to rotate about it as axis, and let it be so graduated on the rim that the length of one division, multiplied by the length of the line, is equal to a unit of square measure. Then, as the line passes from one position to another, parallel to the plane of the paper, the roller will record the sum of the separate infinitesimal movements of the line at right angles to its length, and will thus, by the graduations on its rim, read off, say, at the point of contact with the paper, give the area passed over by the line. Let $l =$ the length of the line, $\Delta x =$ the perpendicular distance travelled by the roller. Then, in the

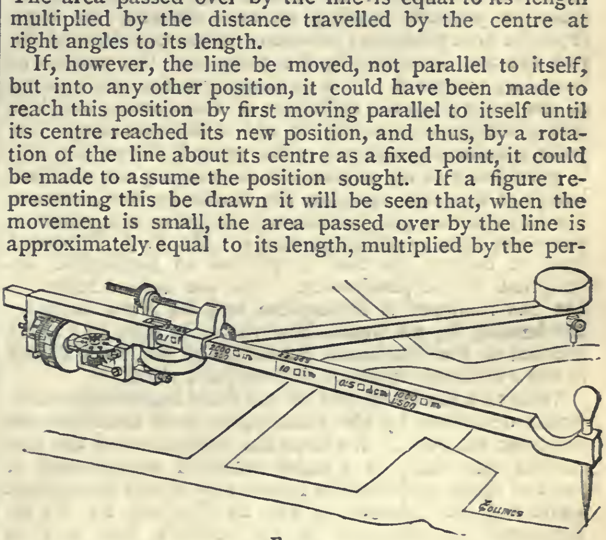


Fig. 4.

limit, $l\,dx =$ the element of area described. In passing over a finite area this will equal $\int l\,dx = lx$, where x is the whole distance recorded by the roller. If now the roller be mounted at a distance r from the centre of the line, in traversing the element of area as before, it will first record $l\,dx$ in its motion parallel to itself, and then, as the line turns about its centre, it will either add or subtract from that reading an amount corresponding to the arc of the circle of radius r, through which it turns; the roller will thus show $l\,dx \pm r\,d\theta$, where $d\theta$ is the small angle turned through. In traversing a finite area this will record $lx \pm r\theta$ where θ is the angle between the initial and final positions of the line. It is now obvious that if the initial and final positions of the line are parallel or coincident, the area passed over by the line will be equal to the area recorded by the roller, even though it were not mounted on the centre of the line, as the second term in the expression just given vanishes. Returning now to the illustration, we see the line represented by the rod carrying the roller, its two extremities being the tracer and the axis of the pivot. Assume that the tracer is placed at the extreme right of the area to be measured: by moving it to the extreme left the roller is made to record an area differing by $\pm r\theta$ from the whole area between the initial and final positions of the rod, and bounded by the portion of the circle described by the further extremity of the rod and by that portion of the boundary of the area traversed, r and θ having the meanings previously assigned to them. Let the tracer be now carried round the boundary back to its starting point on the other side of the area; the roller will now revolve the reverse way, and will subtract from its previous reading an area differing by $\mp r\theta$ from the area contained between the two positions of the rod, the arc described by the pivot and the portion of boundary traversed. It is now apparent that the reading of the roller gives the difference of these areas, which is that of the figure required (Fig. 4).[1] A different form of the polar planimeter is shown in the second illustration, and is provided with an adjustment for varying the effective length of the arm carrying the tracer, by which means the dial and the graduations on the roller are made to show the area to different scales. Mr. F. J. Bramwell was, we believe, the first to publish an intelligible description of the planimeter. This the author refers to, but has rendered it incomplete and far from lucid by condensing it; we would, however, refer him to a short account of the instrument published by Mr. F. P. Purvis in the *Philosophical Magazine* for July, 1874.

With this sketch of two of the most important instruments described by Mr. Stanley, we must conclude our notice of his book. We hope the publication of the new edition may lead to a more extended employment at least of these, and that the favour now shown throughout India and the Colonies, as well as England, for the instruments manufactured by the author's firm may be sufficient inducement to keep them up to their present high standard.

[1] It is here assumed for simplicity sake that the points at which the motion of the roller is reversed are at the extreme right and left of the figure.

NOTES

THE announcement that Dr. W. B. Carpenter is about to retire from the post of Registrar of the University of London will be received with general regret. He has filled the office for twenty-three years.

PROF. MAREY has been elected to fill the place of the late Claude Bernard in the Section of Medicine in the Paris Academy of Sciences.

THE tenth annual report of the U.S. Geological and Geographical Survey of the Territories, in charge of Prof. Hayden, will be ready for distribution in a few weeks. The report has been in type nearly a year, but has been delayed on account of the engraving of the plates. These are now completed and the report will be issued at once. It contains 546 closely printed pages octavo, with eighty plates, sections, maps, &c. Fifty of the plates illustrate the remarkable cliff dwellers in Southern Colorado and Northern New Mexico. This is the last annual report pertaining to Colorado and contains a very interesting series of chapters on the geology of that remarkable country. On the whole this report will prove one of great popular interest and ought to have been published in great numbers. Only 4,500 copies have as yet been ordered. About 250 pages of the eleventh annual report of the field work for 1877 are in type at the Public Printing Office. This will contain a detailed description of the geological and geographical features of Southern Wyoming and Idaho. The reports of Sir Joseph D. Hooker and Dr. Asa Gray will give this volume a high character as well as great popular interest. 10,000 copies have been ordered by Congress. There will be very extended geological reports by Messrs. Endlich, White, St. John, and Peale, and geographical reports by Messrs. Gannett and Wilson, and special reports by Leidy, &c., &c.

VARIOUS items on electric lighting are to hand. It is telegraphed from Washington, December 7, that Mr. Edison's application for a patent for his electric light has been favourably passed by the Patent Office, and that the letters patent were to be issued on Tuesday. The *Journal* of the Society of Arts for December 6 contains a useful *résumé* of the practical application of electricity to lighting purposes, by Mr. J. N. Shoolbred. A new system of electric lamp has been invented in Paris and will be experimented on shortly in public. The carbon rods are four in number, as in the Rapieff system, but instead of crossing at an angle they are arranged in two parallel lines. The consumption of carbon for electric lighting is increasing so rapidly that M. Carré, the well-known Paris maker, is extending his works. He is manufacturing now at the rate of 2,000 meters a day. The electric light illumination has been prolonged, by a vote of the Municipal Council of Paris, up to January 19, for the Avenue de l'Opéra and the front of the Legislative Palace. The city engineers have received orders to prepare, during the interval, a report on the several systems which are now in operation or may be proposed.

THE New York papers report that Mr. Edison has stated that he has made an improved receiver for his telephone by means of which persons standing 15 feet from the instrument can hear a whisper uttered miles away.

AFTER the masterly works of Tschudi on "Animal Life in the Alps," and of Heer on the "History of Vegetation in Switzerland," another work likely to be of high value is announced,—H. Christ on the plants of that country—"Das Pflanzenleben in der Schweiz." The interest of the work is all the greater that Switzerland contains on its narrow area nearly all the diversity of plants which grow in middle and northern Europe. Many years' research of the author in the field, his previous works on separate parts of the Alps, as well as his connection with the botanists of Europe, have enabled Dr. Christ to publish a work which may be expected to range with those above-mentioned. It will appear in four fascicules, with many illustrations, and four maps of vegetable zones, one of which, the distribution of grapes and of several plants of the Föhn and lake regions, will appear this month. The whole work will be finished about the spring of 1879, the first half fascicule having just appeared.

A NEW Botanical Society has just been formed at Munich. the president is Prof. Robert Hartig, and the vice-president Dr. Arnold, an eminent lichenologist.

HERR ALBERT KÜPPERS, an eminent sculptor at Bonn, has

just finished the model of a statue to be erected in memory of Prof. Jacob Nöggerath, the well-known mineralogist.

THE Chinese are about to commence the erection of a line of telegraph from Tientsin to Taku at the mouth of the Pei-ho, and also to make the necessary surveys for another line between Tientsin, Paoting-fu, and Peking.

ACCORDING to the *Colonies and India*, the surveyor, in making a survey of the new road at Mohikinui, in Buller County, New Zealand, struck a coal-seam five feet thick at a distance of only two miles from the township. The coal is bituminous and excellent in quality; it is, moreover, easily accessible and can be brought to the port at a very small cost.

THE Council of the Society of Arts have addressed a memorial to Lord Salisbury asking him to request her Majesty's Ministers abroad to collect information on the system of technical and industrial education in foreign countries, in continuation of what was published in 1868. Lord Salisbury has promised to give the matter his consideration, though we think this is rather an unfortunate time to address the Foreign Secretary on so peaceful a subject.

IN reference to the statement that Sir William Armstrong has employed electricity, generated by water power, a mile and a half distant, to light his picture gallery, and that he proposes to use the same force to turn machinery about his house, Mr. D. R. Jones sends us a copy of a letter sent by him to the *Australasian*, October 25, 1870, in which, referring to the phenomena of dynamic electricity, he states that it is evident that motion may be transformed into electricity, and *vice versâ*. He suggests that we may have here "the means of utilising the motions of the air and of water, which, for want of means of transmission, have been hitherto allowed to run to waste. Various methods of converting, transmitting, and utilising force will readily suggest themselves as a combination of these well-known facts. While we have such superabundant constant supplies of force it is not right that the stores of Nature should be ransacked."

THE *Times* of Monday contains another letter from Dr. Schliemann, giving an account of his further excavations on the site of Troy. He has succeeded in exploring much of the remains of the ancient structures so that the plan can be very distinctly traced. Dr. Schliemann himself left London on Monday for Paris, and intends to recommence work on March 1. A number of the objects found during the last explorations have been deposited in the South Kensington Museum, where they will be exhibited.

THE *Midland Naturalist*, we are glad to see, has concluded a successful first volume. Its past twelve numbers contain many papers of value, both on local and general scientific matters, and its conduct is creditable to its editors and to the many societies of which it is the organ. The December number contains a carefully compiled index, which must be a great comfort to those in search of any paper or subject in the volume. Several good contributions are promised for the next volume, and we trust that the journal will receive every encouragement from the members of the societies it represents, and that its conductors will strive to make it thoroughly representative of the science of the Midlands.

A CORRESPONDENT "R. C. J.," writes as follows from Driefontein, Heilbronn District, Orange Free State, South Africa, under date October 14:—"Our last winter was dry throughout, and unusually cold, that is, June, July, and August, and on August 3 a piece of country in the Transvaal, about seventy or eighty miles north of this part, and on the road from this to Pretoria, about fifteen or twenty miles wide, and perhaps the same in length, was visited in twenty-four hours with such a sudden change of temperature, from 85° to 42° F., that more than 100 bodies of dead Kafirs, besides oxen, were found as if killed by the sudden abstraction of caloric. There was no wind or rain, but a fall of snow. The land is about as high as this, about 4,600 feet."

EXCAVATIONS are now in progress on the Limburg, in the Bavarian palatinate, which will lead to important results for prehistoric investigation, inasmuch as they are directed to the elucidation of the much contested question regarding the constructors and former inhabitants of the Ringwall near Dürkheim.

La Semaine Française is the title of a new weekly French paper published in London for English readers, and which is meant to appeal "to all those who wish to read good French in the way in which it is most likely to be read with interest and profit." The number before us is well selected as to contents, and contains news of French matters and expressions of French opinion in various departments. A small amount of space, we are pleased to see, is devoted to science.

DR. RAE writes that at about 14m. past midnight of December 5–6, whilst there was bright moonlight, he observed a meteor of intensely bright and white light passing obliquely downwards from west to east. It was first noticed almost directly below the western foot of Orion, and disappeared when slightly to eastward of Sirius, having passed at 3° or 4° of arc below these stars. It was spherical and apparently of 6′ or 8′ diameter, with a fiery red tail four or five times that length.

AN earthquake occurred in Scotland on Tuesday morning last week at Balnacara and other parts of the district of Loch Alsh, on the west coast of the county of Ross, opposite the Isle of Skye. The shock was very marked, the tremulous motion of the earth being distinctly felt, and the houses shaking violently At Balnacara the shock occurred at 5 o'clock, and at Plocton, five miles distant, between 7 and 8.

AN Indian paper states that in the Ferozepore district the rise of the Sutlej has once more broken the head-works of the inundation canals, and over 100 square miles of country are under water. The damage done to property has been great, but on the other hand a quantity of treasure has been uncovered by the floods in the old fort of Momdote, a few miles from Ferozepore.

MR. CORNELIUS WALFORD has reprinted in a separate form his elaborate and valuable paper on "The Famines of the World, Past and Present," read in March last before the Statistical Society.

THE Eleventh Annual Report of the Trustees of the Peabody Museum of American Archæology and Ethnology to the President and Fellows of Harvard College is an unusually interesting one. The description of the new museum buildings at Cambridge is very full, and illustrated with plans and a photographic view; the building seems admirably adapted to the purpose for which it is intended. Besides an account of the additions to the Museum and the work done since the last Report, the present publication contains the following papers:—"Second Report on the Implements found in the Glacial Drift of New Jersey," by Dr. C. C. Abbot; "The Method of Manufacture of several Articles by the former Indians of South Carolina," by Mr. Paul Schumacher; "Cave Dwellings in Utah," by Mr. Edward Palmer; "Notes on a Collection from an Ancient Cemetery in Southern Peru," by Mr. J. H. Blake; "Archæological Explorations in Tennessee," by Mr. F. W. Putnam (this long and amply illustrated paper is separately reprinted); "Observations on the Crania from the Stone Graves in Tennessee," by Mr. Lucien Carr; "On the Tenure of Land Among the Ancient Mexicans," by Mr. A. F. Bandelier. Besides Mr. Putnam's paper most of the others are accompanied by numerous illustrations.

In a note on "Colonial Grasses as Paper-making Materials," the *Colonies and India* suggests the possibility of utilising some of the coarse grasses which grow with such provoking pertinacity in South Africa, Australia, New Zealand, &c. The *Typha angustifolia*, for example, a large kind of tussock grass (known as *raupo* to the New Zealand natives, who use it for thatching their houses), which grows in enormous quantities in the swampy flats near rivers and lakes, may, like its neighbour, the *Phormium tenax*, prove a rival to Esparto grass; the *wiwi*, a coarse, wiry kind of grass, growing chiefly in the interior of North Island, is also worth an experiment. In New South Wales the grass-cloth plant (*Böhmeria nivea*) has already received some attention, being used for the manufacture of a fine kind of matting. South Africa is probably richest of all in its grasses; in the great Karroo district thousands of square miles are covered with the twaa-grass, the sour-veldt, and the sweet-veldt, the importance of which as fodder may be found equalled by their value as paper-making material. Still more likely to prove valuable is the *Stipa capensis*, a member of the family to which Esparto belongs.

The *Transactions* of the Cumberland Association for the Advancement of Literature and Science, Part III., 1877-78, edited by Mr. Clifton Ward, is a thickish volume containing papers by members of some of the Associated Societies. The first paper, however, after various reports, is that by Sir George Airy, on the "Probable Condition of the Interior of the Earth," a report of which we gave at the time of its delivery; accompanying it is a diagram of an ideal earth. Mr. Ward has a paper on "Quartz in the Lake District;" Mr. C. Smith one on "Boulder Clay;" Mr. Pickering, on a "Submerged Forest at St. Bees;" and Mr. Fisher Crosthwaite gives an interesting account of Peter Crosthwaite, who, at the end of the last and beginning of the present centuries, did much to promote science in the district.

M. J. Poliakoff, who was sent last summer by the St. Petersburg Academy of Sciences to examine the remains of the stone period in the governments of Yaroslaff and Vladimir, gives the following results of his explorations:—Very interesting collections were found in excavating a mound, close by Yaroslaff; numerous skulls of men of the neolithic period were discovered here, together with polished silex hatchets and hammers, and numerous bones of animals of existing species. Far richer collections were found in the valley of the Oka River, in the district of Murom. Here, in the sandy mounds of the valley, as well as in the alluvium of the river, M. Poliakoff has discovered immense quantities of silex implements, polished and rough, of the most varied forms. The implements were always found together with bones of the *Castor fiber*, the *Sus scrofa ferus*, and the *Bos primigenius*, none of which exist now in those regions. Besides, he also discovered vestiges of old wood buildings, very like the lacustrian dwellings of Switzerland. The most important discovery during these explorations was made by M. Poliakoff, in company with Count Uraroff, close by Karacharovo Town, in a very old lake alluvium, being a somewhat washed-up glacier deposit. Here they found rough stone implements of the paleolithic period, together with bones of the mammoth, rhinoceros, and the *Bos priscus*. The character of the deposits proved without doubt the co-existence of man with those extinct mammals in Russia, as well as in other parts of Europe. After having finished his explorations, M. Poliakoff made a journey in Western Europe to study the chief museums, and to compare the implements he has collected during many years in Russia and Siberia, Western and Eastern, with those of England, Sweden, Denmark, France, and Switzerland. We expect that this last journey of M. Poliakoff will accelerate the opening of the projected pre-historic museum at St. Petersburg.

The additions to the Zoological Society's Gardens during the past week include two Black-faced Spider Monkeys (*Ateles ater*), two Rufous-vented Guans (*Penelope cristata*) from U. S. of Columbia, two Horsfield's Tortoises (*Testudo horsfieldi*) from Turkestan, presented by Mr. A. Gonzalez Carazo; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. A. G. Lytton Squires; two Black-eared Marmosets (*Hapale penicillata*) from South-East Brazil, presented by the Countess of Cotterham; two Laughing Kingfishers (*Dacelo gigantea*) from Australia, presented by Mr. Edwin A. B. Crockett; a Ceylon Jungle Fowl (*Gallus stanleyi*) from Ceylon, two Japanese Pheasants (*Phasianus versicolor*) from Japan, a Grey Francolin (*Francolinus ponticerianus*) from India, presented by Mr. Geo. Lyon Bennett; a Rhomb-marked Snake (*Psammophylax rhombeatus*), three Rufescent Snakes (*Leptodira rufescens*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Kinkajou (*Cercoleptes caudivolvulus*) from South America, three Snow Buntings (*Plectrophanes nivalis*), European, purchased.

ROYAL SOCIETY—THE PRESIDENT'S ANNIVERSARY ADDRESS[1]

II.

The modern development of botanical science, being that which occupies my own attention, is naturally that on which I might feel especially inclined to dwell; and I should so far have the excuse that there is, perhaps, no branch of research with the early progress of which this Society is more intimately connected.

One of our earliest secretaries, Robert Hooke, two centuries ago, laboured long and successfully in the improvement of the microscope as an implement of investigation. He was one of the first to reap the harvest of discovery in the new fields of knowledge to which it was the key, and if the results which he attained have rather the aimless air of spoils gathered hither and thither in a treasury, the very fulness of which was embarrassing, we must remember that we date the starting-point of modern histology from the account given by Hooke in his "Micrographia" (1667) of the structure of cork, which had attracted his interest from the singularity of its physical properties. Hooke demonstrated its *cellular structure*, and by an interesting coincidence he was one of the first to investigate, at the request, indeed, of the founder of the Society, Charles II., the movement of the sensitive plant *Mimosa pudica*, one of a class of phenomena which is still occupying the attention of more than one of our Fellows. In attributing the loss of turgescence, which is the cause of the collapse of the petiole and subordinate portions of the compound leaf which it supports, to the escape of a subtle humour, he to some extent foreshadowed the modern view which attributes the collapse of the cells to the escape of water by some mechanism far from clearly understood —whether from the cell-cavities or from the cell-walls into the intercellular spaces.

Hooke having shown the way, Nehemiah Grew, who was also secretary of the Royal Society, and Marcello Malpighi, Professor of Medicine in the University of Bologna, were not slow to follow it. Almost simultaneously (1671–3) the researches of these two indefatigable students were presented to the Royal Society, and the publication of two editions of Malpighi's works in London prove how entirely this country was at that time regarded as the head-quarters of this branch of scientific inquiry. We owe to them the generalisation of the cellular structure, which Hooke had ascertained in cork, for all other vegetable tissues. They described also accurately a host of microscopic structures then made known for the first time. Thus, to give one example, Grew figured and described in several different plants the stomata of the epidermis:—"Passports either for the better avolation of superfluous sap, or the admission of air."

With the exception of Leeuwenhoek no observer attempted to make any substantial addition to the labours of Grew and Malpighi for more than a century and a half, and however remarkable is the impulse which he gave to morphological studies, the view of Caspar Wolff in the middle of the eighteenth cen-

[1] Address of Sir Joseph Hooker, C.B., K.C.S.I., the President, delivered at the Anniversary Meeting of the Royal Society, on Saturday, November 30, 1878. Continued from p. 113.

tury (1759), in regarding cells as the result of the action of an organising power upon a matrix, and not as themselves influencing organisation, were adverse to the progress of histology. It is from Schleiden (1838), who described the cell as the true unit of vegetable structure, and Schwann, who extended this view to all organisms whether plants or animals, and gave its modern basis to biology by reasserting the unity of organisation throughout animated nature, that we must date the modern achievements of histological science. Seldom, perhaps, in the history of science has any one man been allowed to see so magnificent a development of his ideas in the space of his own lifetime as has slowly grown up before the eyes of the venerable Schwann, and it was, therefore, with peculiar pleasure that a letter of congratulation was intrusted by the officers to one of the Fellows of this Society on its behalf on the recent occasion of the celebration of the fortieth anniversary of Schwann's entry into the professoriate.

If we call up in our mind's eye some vegetable organism and briefly reflect on its construction, we see that we may fix on three great steps in the analysis of its structure, the organic, the microscopic, and the molecular, and, although not in the same order, each of the three last centuries is identified with one of these. In the seventeenth century Grew achieved the microscopic analysis of plant tissues into their constituent cells; in the 18th, Caspar Wolff effected the organic analysis (independently but long subsequently expounded by the poet Goethe) of plant structures into stem and leaf. It remained for Nägeli in the present century to first lift the veil from the mysterious processes of plant growth, and by his memorable theory of the molecular constitution of the starch-grain and cell-wall, and their growth by intussusception (1858), to bring a large class of vital phenomena within the limits of physical interpretation. Strasburger has lately (1876) followed Sachs in extending Nägeli's views to the constitution of protoplasm itself, and there is now reason to believe that the ultimate structure of plants consists universally of solid molecules (not, however, identical with chemical molecules) surrounded with areas of water which may be extended or diminished. While the molecules of all the inert parts of plants (starch-grains, cell-wall, &c.) are on optical grounds believed to have a definite crystalline character, no such conclusion can be arrived at with respect to the molecules of protoplasm. In these molecules the characteristic properties of the protoplasm reside, and are more marked in the aggregate mass in proportion to its denseness, and this is due to the close approximation of the molecules and the tenuity of their watery envelopes. The more voluminous the envelopes the more the properties of protoplasm merge in those of all other fluids.

It is, however, to the study of the nuclei of cells that attention has been recently paid with the most interesting results. These well-known structures, first observed by Ferdinand Bauer at the beginning of the century (1802), were only accurately described, thirty years later, by Robert Brown (1833). Up to the present time their function has been extremely obscure. The beautiful investigations of Strasburger (1875) have led him to the conclusion that the nucleus is the seat of a central force which has a kind of polarising influence upon the protoplasm molecules, causing them to arrange themselves in lines radiating outwards. Cell-division he regards as primarily caused by the nucleus becoming bipolar, and the so-called caryolitic figures first described by Auerbach exhibit the same arrangement of the protoplasm molecules in connecting curves as in the case of iron-filings about the two poles of a bar-magnet. The two new centres mutually retire, and each influencing its own tract of protoplasm, the cell-division is thereby ultimately effected. This is but a brief account of processes which are greatly complicated in actual detail, and of which it must be remarked that, while the interest and beauty of the researches are beyond question, caution must be exercised in receiving the mechanical speculations by which Strasburger attempts to explain them. He has himself shown that cell-division presents the same phenomena in the animal kingdom, a result which has been confirmed by numerous observers, amongst whom I may content myself with mentioning one of our own number, Mr. F. Balfour. Strasburger further points out that this affords an argument for the community of descent in animal and vegetable cells; he regards free cell-division as derivable from ordinary cell-division by the suppression of certain stages.

Turning now to the discoveries made during the last five years in physiological botany, we find that no one has advanced this subject so greatly as Mr. Darwin. In 1875 was published his work on insectivorous plants, in which he ascertained the fact that a number of species having elaborate structures adapted for the capture of insects, utilised the nitrogenous matter which these contain as food. The most important principle established in the course of these researches was that such plants as *Drosera*, *Dionæa*, *Pinguicula*, &c., secrete a digestive fluid, which has led, through Gorup Bezanez's investigations on the ferment in germinating seeds, to a recognition of the active agency of ferments in the transmission of food-material, which marks a great advance in our knowledge of the general physiology of nutrition.

The extreme sensitiveness of the glands of *Drosera* to mechanical and chemical stimulus (especially to phosphate of ammonia), the directive power of its tentacles, depending upon the accurate transmission of motor impulses, and the "reflex" excitation of secretion in the glands, were all discoveries of the most suggestive nature in connection with the subject of the irritability and movements of plants. The phenomenon of the aggregation of the protoplasmic cell-contents in the tentacles of *Drosera* is a discovery of a highly remarkable nature, though not yet thoroughly understood. Lastly, Mr. Frank Darwin, following his father's footsteps, as it were crowned the edifice by showing to what an extent insectivorous plants do profit by nitrogenous matter supplied to their leaves.

In close relation to these researches are those, also by Mr. Darwin, on the structure and functions of the bladder of *Utricularia*, which he has shown to have the power of absorbing decaying animal matter; and those of Mr. Frank Darwin on contractile filaments of extraordinary tenuity attached to the glands on the inner surface of the cups formed by the connate bases of the leaves of the teasel, and which filaments exhibit motions suggesting a protoplasmic origin. It is to be hoped that their discoverer will pursue his investigations upon these curious bodies, whose origin and real nature in relation to the plant and its functions is involved in obscurity.

The subject of the cross-fertilisation of plants, which, though a long-known phenomenon, first become a fruitful scientific study in Mr. Darwin's now classical work, "On the Various Contrivances by which Orchids are Fertilised," has within the last few years made rapid advance under its author's hand. The extreme importance of avoiding self-fertilisation might indeed be inferred from the prevalence in flowers of elaborate contrivances for preventing it; but it remained to be shown that direct benefit attended cross-fertilisation, and this has now been proved by an elaborate series of experiments, the results of which are not only that both increased fertility or greater vigour of constitution attend cross-fertilisation, but that the opposite effects attend self-fertilisation. In the course of these experiments it became evident that the good effects of the cross do not depend on the mere fact of the parents being different individuals, for when these were grown together and under the same conditions, no advantage was gained by the progeny; but when grown under different conditions a manifest advantage was gained. As instances, if plants of *Ipomœa* and *Mimulus*, which had been self-fertilised for seven previous generations, were kept together and then intercrossed, their offspring did not profit in the least; whereas, when the parent plants were grown under different conditions, a remarkably vigorous offspring was obtained.

Mr. Darwin's last work, "On the Different Forms of Flowers," though professedly a reprint of his paper on dimorphic plants, published by the Linnean Society, contains many additions and new matter of great importance in reference to the behaviour of polygamous plants, and on cleistogamic flowers. Among other points of great interest is the establishment of very close analogies between the phenomena attending the illegitimate union of trimorphic plants and the results of crosses between distinct species: the sterile offspring of the crosses of the same species exhibiting the closest resemblance to the sterile hybrids obtained by crossing distinct species; while a whole series of generalisations, founded on the results of the one series of experiments, are closely paralleled by those founded on the other. The bearing of this analogy on the origin of species is obviously important.

Besides these investigations, Mr. Darwin has produced within the last five years second editions of his volume on the "Fertilisation of Orchids," and on the "Habits and Movements of Climbing Plants," as also of his early works on "Coral Reefs," and "Geological Observations in South America;" all of them abounding in new matter.

Of special interest to myself, as having been conducted in the

Jodrell Laboratory at Kew, are Dr. Burdon Sanderson's investigations on the exceptional property possessed by the leaves and other organs of some plants which exhibit definite movements in response to mechanical, chemical, or electric stimuli. In 1873 this physiologist showed us in our meeting-room that the closing of the laminæ of the leaf of *Dionæa* is preceded by a preliminary state of excitement, and is attended with a change in the electric conditions of the leaf; and this so closely resembled the change which attends the excitation of the excitable tissues of animals that he did not hesitate to identify the two phenomena.

This remarkable discovery immediately directed the attention of two German observers to the electromotive properties of plants, one, Dr. Kunkel, in the laboratory of Prof. Sachs; the other, Prof. Munk, in that of the University of Berlin.

Prof. Munk, whose researches are of much the greater scope and importance, took as his point of departure Dr. Burdon Sanderson's discovery. The leading conclusion to which he arrived was that in *Dionæa* each of the oblong cells of the parenchyma is endowed with electromotive properties which correspond with those of the "muscle-cylinder" of animals; with this exception, that whereas in the muscle-cylinder the ends are negative to the central zone, in the vegetable cell they are positive; and he endeavours to prove that according to this theory all the complicated electromotive phenomena which had been observed could be shown to be attributable to the peculiar arrangement of the leaf-cells.

During the last two summers Dr. Burdon Sanderson has been engaged in endeavouring to discover the true relations which subsist between the electrical disturbance followed by the shutting of the leaf-valves of *Dionæa* and the latent change of protoplasm which precedes this operation. He has found that though the mechanism of the change of form of the excitable parenchyma which causes the contraction is entirely different from that of muscular contraction, yet that the correspondence between the exciting process in the animal tissues and what represents this in plant tissues appears to be more complete the more carefully the comparison is made; and that whether the stimulus be mechanical, thermal, or electrical, its effects correspond in each case. Again, the excitation is propagated from the point of excitation to distant points in the order of their remoteness, and the degree to which the structure is excited depends upon its temperature. Notwithstanding, however, the striking analogies between the electrical properties of the cells of *Dionæa* and of muscle-cylinders, Dr. Burdon Sanderson is wholly unable to admit with Prof. Munk that these structures are in this respect comparable.

In morphological botany attention has been especially directed of late to the complete life-history of the lower order of cryptogams, since this is seen to be more and more an indispensable preliminary to any attempt at their correct classification.

The remarkable theory of Schwendener, now ten years old, astonished botanists by boldly sweeping away the claims to autonomous recognition of a whole group of highly characteristic organisms—the lichens—and by affirming that these consist of ascomycetal fungi united in a commensal existence with algæ. The controversial literature and renewed investigations which this theory has given rise to is now very considerable. But the advocates of the Schwendenerian view have gradually won their ground, and the success which has attended the experiments of Stahl in taking up the challenge of Schwendener's opponents, and manufacturing such lichens as *Endocarpon* and *Thelidium*, by the juxtaposition of the appropriate algæ and fungi, may almost be regarded as deciding the question. Sachs, in the last edition of his "Lehrbuch," has carried out completely the principle of classification of algæ, first suggested by Cohn, and has proposed one for the remaining thallophytes, which disregards their division into fungi and algæ. He looks upon the former as standing in the same relation to the latter as the so-called saprophytes (*e.g. Neottia*) do to ordinary green flowering-plants.

This view has especial interest with regard to the minute organisms known as *Bacteria*, a knowledge of the life-history of which is of the greatest importance, having regard to the changes which they effect in all lifeless and, probably, in all living matter prone to decomposition. This affords a morphological argument (as far as it goes) against the doctrine of spontaneous generation, since it seems extremely probable that just as yeast may be a degraded form of some higher fungus, *Bacteria* may be degraded allies of the *Oscillatoriæ*, which have adopted a purely saprophytal mode of existence.

Your *Proceedings* for the present year contain several important contributions to our knowledge of the lowest forms of life. The Rev. W. H. Dallinger, continuing those researches which his skill in using the highest microscopic powers and his ingenuity in devising experimental methods have rendered so fruitful, has adduced evidence which seems to leave no doubt that the spores or germs of the monad which he has described differ in a remarkable manner from the young or adult monads in their power of resisting heated fluids. The young and adult monads, in fact, were always killed by five minutes' exposure to a temperature of 142° F. (61° C.), while the spores germinated after being subjected to a temperature of 10° above the boiling-point of water (222° F.).

Two years ago, Cohn and Koch observed the development of spores within the rods of *Bacillus subtilis* and *B. anthracis*. These observations have been confirmed, with important additions, in these two species by Mr. Ewart, and have been extended to the *Bacillus* of the infectious pneumo-enteritis of the pig, by Dr. Klein; and to *Spirillum* by Messrs. Geddes and Ewart; and thus a very important step has been made towards the completion of our knowledge of the life-history of these minute but important organisms. Dr. Klein has shown that the infectious pneumo-enteritis, or typhoid fever of the pig, is, like splenic fever, due to a *Bacillus*. Having succeeded in cultivating this *Bacillus* in such a manner as to raise crops free from all other organisms, Dr. Klein inoculated healthy pigs with the fluid containing the *Bacilli*, and found that the disease in due time arose and followed its ordinary course. It is now, therefore, distinctly proved that two diseases of the higher animals, namely, "splenic fever" and "infectious pneumo-enteritis," are generated by a *contagium vivum*.

Finally, Messrs. Downes and Blunt have commenced an inquiry into the influence of light upon *Bacteria* and other *Fungi*, which promises to yield results of great interest, the general tendency of these investigations leaning towards the conclusion that exposure to strong solar light checks and even arrests the development of such organisms.

The practical utility of investigations relating to *Bacillus* organisms as affording to the pathologist a valuable means of associating by community of origin various diseases of apparently different character, is exemplified in the "Loodiana fever," which has been so fatal to horses in the East. The dried blood of horses that had died of this disease in India has been recently sent to the Brown Institution, and there afforded seed from which a crop of *Bacillus anthracis* has been grown, which justified its distant pathological origin by reproducing the disease in other animals. Other equally interesting experiments have been made at the same Institution, showing that the "grains" which are so largely used as food for cattle, afford a soil which is peculiarly favourable for the development and growth of the spore filaments of *Bacillus;* and that by such "grains" when inspected, the anthrax fever can be produced at will, under conditions so simple, that they must often arise accidentally. The bearing of this fact on a recent instance in which anthrax suddenly broke out in a previously uninfected district, destroying a large number of animals, all of which had been fed with grains obtained from a particular brewery, need scarcely be indicated.

In systematic botany, which, in a nation like ours, that is ever extending its dominions and exploring unknown regions of the globe, must always absorb a large share of the energies of its phytologists, I can but allude to two works of great magnitude and importance.

Of these the first is the "Flora Australiensis" of Bentham, completed only a year ago; a work which has well been called unique in botanical literature, whether for the vast area whose vegetation it embraces (the largest hitherto successfully dealt with), or for the masterly manner in which the details of the structure and affinities of upwards of 8,000 species have been elaborated; its value in reference to all future researches regarding the geographical distribution of plants, the southern hemisphere, and the evolution therein of generic and specific types, cannot be over-estimated.

The other great work is the "Flora Braziliensis," commenced by our late foreign Fellow, von Martius, and now ably carried on by Eichler, of Berlin, assisted by coadjutors (amongst whom are most of our leading systematists) under the liberal auspices of His Majesty the Emperor of Brazil. When completed, this gigantic undertaking will have embraced, in a systematic form, the vegetation of the richest botanical region of the globe.

Having now endeavoured to recall to you some of the great advances in science made during the last few years, it remains for me, after the distribution of the medals awarded by your Council, to retire from the Presidency in which I have so long experienced the generous support of your officers and yourselves. This support, for which I tender you my hearty thanks, together with my sense of the trust and dignity of the office, and the interest attached to its duties, has rendered my resignation of it a more difficult step than I had anticipated. My reasons are, however, strong. They are the pressure of official duties at Kew, which annually increase in amount and responsibility, together with the engagements I am under to complete scientific works, undertaken jointly with other botanists, before you raised me to the Presidency, and the indefinite postponement of which works delays the publication of the labours of my coadjutors. I am also influenced by the consideration that, though wholly opposed to the view that the term of the Presidency of the Royal Society should be either short or definitely limited, this term should not be very long; and that, considering the special nature of my own scientific studies, it should, in my case, on this as well as on other grounds, be briefer than might otherwise be desirable. Cogent as these reasons are they might not have been paramount were it not that we have among us one pre-eminently fitted to be your President by scientific attainments, by personal qualifications, and by intimate knowledge of the Society's affairs; and by calling upon whom to fill the proud position which I have occupied, you are also recognising the great services he has rendered to the Society as its treasurer for eight years, and its ofttimes munificent benefactor.

HAECKEL ON THE LIBERTY OF SCIENCE AND OF TEACHING[1]

II.

CHAPTER V. treats of the methods of teaching, and contrasts the *genetic* method, as advocated by Haeckel, with the *dogmatic* one recommended by Virchow. The sensation which Virchow's address caused in wider circles was only partly the result of his opposition to the descent theory; its principal cause was his surprising conclusions with regard to the liberty of teaching. Virchow demands that in the school—from the elementary school up to the university—*nothing should be taught which is not absolutely certain*; only *objective* but no *subjective* knowledge is to be communicated to the pupils by the teacher; only facts, no hypotheses. Haeckel remarks that rarely has an eminent representative of science made such an attack upon the liberty of science as did Virchow at Munich. "Where," he asks, "are we to find the limits between subjective and objective knowledge?" According to his conviction no such limit exists, and all human knowledge as such is subjective. "An objective science consisting only of facts, without subjective theories, cannot be imagined." He then proceeds to review various sciences in turn, and to point out how much objective knowledge and "facts," and how much subjective knowledge and "hypotheses" they contain. He begins with *Mathematics* as the science which is eminently the most *certain* one of all: "What about the simplest and deepest maxims upon the firm basis of which the whole proud building of mathematics rests? Can they be proved for certain? Certainly not! The most fundamental maxims are indeed 'maxims,' and incapable of 'proof.' Only in order to show by an example how even the first mathematical maxims may be attacked by sceptics and shaken by philosophical speculation we recall the recent discussions regarding the three dimensions of space and the possibility of a fourth dimension, discussions which are still continued by a number of the most illustrious mathematicians, physicists, and philosophers. So much is certain that mathematics is absolutely objective as little as any other science, but has a subjective basis in man's own nature. . . . But even if we own that mathematics is an absolutely certain and objective science, how about all other sciences? No doubt those are 'most certain' amongst the 'exact' sciences, the maxims of which are founded on pure mathematics, in the first line therefore a great part of *physics*. We say a great part, because another great part—upon close examination by far the greater—is incapable of an exact mathematical foundation. Or what we do know with certainty about the essence of *matter* or the essence of *force?* What do we know for certain about gravitation, about mass-attraction, about action at a distance, &c.? We look upon Newton's gravitation theory, the basis of mechanics, as the most important and most certain theory of physics, and yet gravitation itself is only a hypothesis. And then the other branches of physics—electricity and magnetism, for instance. The whole knowledge of these important branches is based upon the hypothesis of 'electric fluids' or of imponderable substances, the existence of which is certainly not proved. Or optics? No doubt optics belongs to the most important and most complete branches of physics, yet the vibration theory, which to-day we consider to be its indispensable basis, rests upon a hypothesis which cannot be proved, viz., upon the 'subjective' supposition of the light-ether, the existence of which nobody can objectively prove. Nay, even more; before Young established the vibration theory of light, the emanation theory taught by Newton reigned supreme in physics for centuries; this theory has to-day been abandoned as untenable. According to our view the mighty Newton acquired the greatest merit with regard to the development of optics, as he made the first attempt to connect and explain the mass of objective optical facts by a subjective leading hypothesis. But according to Virchow's view Newton sinned most heavily by teaching this false hypothesis; because in 'exact' physics only *single* and *certain facts* are to be taught and to be ascertained by 'experiment as the highest means of proof;' but physics as a *whole*, resting as it does upon a number of unproved hypotheses, may be the object of research, but must not be taught!" Turning to *Chemistry*, Haeckel shows that its objectiveness stands upon still weaker feet than that of physics. Here the whole of the science is built upon the hypothesis of the existence of atoms, a hypothesis as unproved and as incapable of proof as any. No chemist has ever seen an atom, and yet he thinks the mechanics of atoms the highest problem of his science, and describes and constructs the positions and groupings of atoms, as if they were before him on his dissecting table. According to Virchow, we therefore ought *to banish chemistry from the school* and teach only the properties of bodies and their reactions, which can be shown to the pupils as "certain facts." This matter becomes still more ludicrous when we turn to the other sciences, which are all more or less *historical*, and therefore do not possess that "half-exact" basis upon which chemistry and physics rest. Geology, for instance, would, according to Virchow, have to confine itself to the description of certain facts, *i.e.*, the structure of rocks, the forms of fossils, the shape of crystals, &c., but would in the school have completely to abandon all speculation regarding the development of the earth's crust, *i.e.*, nothing but unproved hypotheses from beginning to end. We might not even teach that fossils are the actual remains of organisms which existed in former periods, because even this is an "unproved" hypothesis. Even down to the eighteenth century many eminent naturalists believed fossils to be "freaks of nature," an enigmatic "*lusus naturæ*." In a later part of his address Virchow admits fossils as "objective material proofs;" but even here we may go no further than our actual experience allows, and we may not draw subjective deductions from the objective facts. Virchow's remark about quaternary man being an "accepted fact" affords Haeckel an opportunity for pointing out his inconsistency, and the uncertainty and vagueness of most hypotheses concerning the age and the first geological occurrence of man; indeed, the distinction of a tertiary and a quartery age in itself is nothing but a *geological hypothesis*. "Virchow tells us that never has a fossil ape skull been found which really belonged to a human proprietor, and that we cannot consider it as a revelation of science, we cannot teach, that man descends from the ape or from any other animal. If that be true, then nothing remains but the descent from a god or from a clod of earth."

Zoology, botany, and other biological doctrines do not fare better, if we consider them in the light in which Virchow would have them taught. Haeckel shows the utter untenability of Virchow's demands, since no science, not even history, and certainly not philosophy, could be tolerated in our schools; indeed, the only one which could remain would be theology. "Incredible as it seems, Virchow, the sceptical antagonist of dogmas, the combatant for the liberty of science, now finds the only certain basis of instruction in the dogma of Church religion. After all that has happened the following phrase leaves no doubt on this point:—'All attempts to transform our problems into doctrines, to introduce our theories as the basis of a plan of education, par-

[1] Freie Wissenschaft und freie Lehre. Eine Entgegnung auf Rudolf Virchow's Münchener Rede über "die Freiheit der Wissenschaft im modernen Staat." Von Ernst Haeckel. Continued from p. 115.

ticularly the attempt simply to depose the Church, and to replace its dogma by a religion of descent without further trouble—these attempts, I say, must fail, and their failure would at the same time bring the greatest dangers upon the position of science generally.' After this every one will easily understand the joyful outbursts of the whole clerical press after Virchow's Munich address. It is known that there is ten times more joy in heaven over one repentant sinner than oven ten just men. If Rudolf Virchow, the 'renowned materialist,' the 'radical progressist,' the principal representative of the 'atheism of science,' is suddenly so completely converted, if openly and loudly he proclaims the 'dogmas of the Church' as the only certain 'basis of instruction,' then, indeed, the combatant Church may sing 'Hosanna in excelsis!' There is only one point to be regretted, and that is that Virchow has not stated which of the many different Church religions is the only true one, and which of the numberless and contradicting dogmas are to become the certain basis of education. We all know that each Church thinks itself the only one leading to eternal bliss, and its dogma the only true one. Now whether it is Protestantism or Catholicism, Reformed or Lutheran confession, Anglican or Presbyterian dogma, Roman or Greek doctrine, Mosaic or Islamitic tenets, Buddhaism or Bramahism, or one of the fetish creeds of the Indians or coloured tribes which is to become the lasting and certain 'basis of instruction,' this, no doubt, Virchow will not hesitate to state at the next meeting of the German Association of Naturalists and Physicians."

"At all events, the instruction of the future," according to Virchow, "will be very much simplified. Because the dogma of the Trinity as the basis of mathematics, the dogma of the resurrection of the flesh as the basis of medicine, the dogma of the infallibility as the basis of psychology, the dogma of the Immaculate Conception as the basis of the science of generation, the dogma of the stoppage of the sun as the basis of astronomy, the dogma of the creation of the earth, animals, and plants, as the basis of geology and phylogeny, these or some other dogmas from other creeds, will make all further doctrines rather superfluous. Virchow, this 'critical nature,' of course knows as well as I do, that these dogmas are *not true*, and yet, according to his view, they are not to be replaced as 'bases of instruction' by the theories and hypotheses of modern natural science, of which Virchow says himself that they *may* be true, probably *are* true to a great extent, but have not been 'proved for certain' as yet."

Finally, Haeckel points out that it seems to be bitter irony if Virchow, at the opening address, recalls the memory of Oken, whom he celebrates as the martyr of free science, and at the end of the same address demands that this "liberty of science" shall apply only to *research*, but not to *instruction*, and that no problems, no theories, no hypotheses are to be taught.

In Chapter VI. the application of the theory of descent to socialism is discussed. The author entirely endorses Prof. Oscar's Schmidt's view on the subject, and shows that the theory of descent and the socialistic theory are "like fire and water to one another." The theory of descent is, on the contrary, aristocratic in the highest sense of the word. Of course, from any theory, be it ever so true and sound, the most absurd deductions may be drawn if it is misapplied, and the author warns particularly against the misapplication of scientific theories to political or social questions. Theory and practice rarely correspond in human life. Haeckel points to the history of Christendom to illustrate his argument. "It is certain that the Christian religion, as well as the Buddhistic doctrine, if freed from all dogmatic fables, contain an excellent humane kernel; now it is just this humane and truly social-democratic part of the Christian creed, which proclaims the equality of all men before God, and preaches the 'Love thy neighbour as thyself,' in fact 'love' in its noblest sense, compassion with the poor and unfortunate, &c., —we say these truly humane sides of the Christian faith are so natural, so pure and noble, that we comprise them with pleasure amongst the moral laws of our monistic natural religion. . . . But what, we must ask, have the chosen representatives, the 'God-taught' (Gottgelehrte) priests, made of this 'religion of love'? It is written with letters of blood upon the pages of the history of mankind for the last 1,800 years! All that different Church religions have done for the forcible propagation of their creeds and for annihilation of 'heretics,' all that Jews have done against heathens, Roman Emperors against Christians, Mohammedans against Christians and Jews, all that is surpassed by the hecatombs of human victims which Christianity has slaughtered for the propagation of its doctrine. And indeed Christians against Christians. Rightly-believing Christians against wrongly-believing Christians. We need only think of the middle ages, of the Inquisition, of the unheard-of and inhuman cruelties which the '*most Christian kings*' of Spain and their worthy colleagues in France, Italy, &c., have committed. Hundreds of thousands then died the most cruel death of fire, simply because they did not bend their reason beneath the yoke of the most flagrant superstition, and because their dutiful conviction forbade them to deny what they had recognised to be natural *truths*. There exists no detestable, abominable, or inhuman action which was not then and up to the present day committed in the name of and on account of 'true Christianity.' And what about the *morals of priests*, who designate themselves as servants of God's word, and whose duty first of all should be to regulate their own lives according to the teachings of Christianity? The long, uninterrupted, and horrible chain of crimes of all kinds which forms the history of the Roman pontiffs, gives the best reply to this question. And as these 'representatives of God upon earth' have done, so did their helpmates and subordinates, so did the 'rightly-believing' priests of other confessions, not failing to establish as glaring as possible a contrast between the practices of their own lives and the noble teachings of Christian love which they always talk about. What we have just said of Christianity applies to all other religious and moral doctrines, indeed to all doctrines which in the wide domain of practical philosophy, in the education of the young, and the civilisation of the masses, are to show their power. The theoretical kernel of these doctrines can always and everywhere form the greatest contrast with its practical application, according to the contradictory nature of man. But what does all this matter to the scientific investigator? His sole and only task is to find out *truth*, and to *teach* that what he has *found to be true*, unheeding what consequences may be drawn from his teachings by the various parties in the State or Church."

In the last chapter Haeckel compares the "Ignorabimus" speech of Dubois-Reymond (delivered at the Leipzig meeting in 1872) with Virchow's "Restringamur" address of Munich, and refutes some of the views expressed by Dubois-Reymond, particularly the view that there are two *insurmountable* obstacles in the way of our completely understanding nature and the world, viz., the essence of matter and force and the human consciousness. Haeckel points out that even if the problems in question are not solved at present, no one has a right to declare them unsolvable. He then proceeds to explain the reasons why the opposition to the theory of descent has mainly originated amongst the Berlin biologists, and adduces examples from the history of science to show that a similar opposition to what have now become established truths, has repeatedly sprung from the same quarter. "It seems, indeed, to be the fate of the most interesting of all sciences, of the *history of evolution*, that its most important steps of progress and its greatest discoveries meet with the most powerful and lasting resistance. Just as Wolf's fundamental epigenesis theory, which was founded in 1759, but was acknowledged only in 1812, so Lamarck's theory of descent, founded in 1809, had to wait during full fifty years before Darwin, in 1859, transformed it into the most important acquisition of modern science. And how was this most comprehensive of all biological theories fought against during this time, in spite of all progress of the empirical sciences? Let us only remember how, in 1830, the celebrated George Cuvier silenced the most eloquent advocate of this theory, Geoffroy Saint Hilaire, in the midst of the Paris Academy, and how almost at the same time, in 1829, its founder, the great Lamarck, ended his laborious life, blind and in misery and poverty, while his antagonist, Cuvier, enjoyed the highest honour and the greatest splendour. And yet to-day we know that the despised and derided doctrines of Lamarck and of Geoffroy Saint Hilaire then contained the most important truths, while Cuvier's much-admired and generally-adopted creation doctrine has been now abandoned generally as an absurd and empty error. Now if neither Haller against Wolff, nor Cuvier against Lamarck, could permanently impede the progress of free research, then still less will Virchow succeed in crushing Darwin's admirable theory, even if he be assisted in an unenviable manner by the noisy Capuchin sermons of his friend Bastian. Much as we regret Virchow's hostile position in this great 'combat for truth,' we do not underrate the effect of his well-founded authority upon wider circles. No doubt the hostile attitude

which the greatest part of the Berlin press has assumed with regard to the doctrine of evolution must be attributed to this authority. But much as we must regret the reactionary current in this and other intelligent Berlin circles, yet we must point out that by this evil we are guarded from a far greater one. This greater, indeed the greatest, evil which could befall German science would be a Berlin 'monopoly of knowledge,' the *centralisation of science*. What highly disastrous fruit this centralisation has borne in France, for instance; how the Paris 'monopoly of knowledge' causes a constant degradation of French science, and has led it downwards from the greatest heights for the last half-century, is well known. Probably the wide-spread differentiation and the many-sided individuality of the German national spirit, the often-decried German particularism, will save us from a centralisation of science of this kind, which particularly in our capital, Berlin, would be doubly dangerous. Little as our 'small states' could be politically of any duration or could lead to a useful state-form, they have certainly been most beneficial and fertile for German science. Because this owes its principal advantages over others to the numerous little centres of education, which the capitals of the German small states formed, and to the many little universities which were always in healthy competition with one another. Let us hope that this beneficial decentralisation of science in our politically united Fatherland will continue permanently. Next to the centrifugal striving of our German national spirit, nothing will further this object so much as an energetic resistance to the free progress of science, just as now again it begins to show itself in the capital of the empire. Because at the same rate as this will remain behind in the mighty current of free and unimpeded mental progress, other numerous centres of education in Germany which follow this current enthusiastically, or at least willingly, will outrun it."

"If Emil Dubois Reymond wanted to make his 'Ignorabimus' the watchword of science, and Rudolf Virchow his still further-reaching 'Restringamur,' then from Jena and from a hundred other educational centres they are met with the call—

"*Impavidi progrediamur!*"

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

STUDENTS of Natural Science who would much rather know French and German than Greek will be glad to learn that a very strong memorial against the retention of Greek as a subject for all honour candidates has been presented to Cambridge University. It is signed by ten heads of public schools, including Drs. Hornby (Eton), Butler (Harrow), and Abbott (City of London), Messrs. Matthew Arnold, Carlyle, W. E. Forster, the Bishops of Exeter and Manchester, Dean Cowie, Dean Stanley, and Dr. Vaughan, Prof. Jebb, and Mr. Roby, to say nothing of such bulwarks of science as Mr. Darwin, Prof. Huxley, Sir Joseph Hooker, Mr. Spottiswoode, and Prof. Tyndall.

THE Board of Musical Studies at Cambridge have applied for the appointment of a University Reader in Acoustics.

THE sum in the hands of the Sedgwick Memorial Committee for the erection of a new building for the geological collection is 12,000*l.*, not 1,200*l.* as we stated last week.

King's College (London) *Magazine*, No. 5, vol. ii. of which has been sent us, contains some pleasant reading, but no one would infer from its contents that the College was an important centre of scientific instruction and research.

DR. J. COSSAR EWART has been appointed by the Crown to the Chair of Natural History in the University of Aberdeen.

MR. F. GUTHRIE, formerly of Graaf-Reinet College, has been appointed to the Chair of Mathematics at the South African College, Capetown.

THE *Journal de St. Pétersbourg* gives the following particulars concerning the public provision for education in Russia:—The sum assigned in the Budget of this year for education is 15,971,289 roubles (about 2,395,000*l.*). There are eight Universities (not reckoning that of Helsingfors for Finland), with 5,629 students. Of these 85 are divinity students, 583 belong to the philological faculty, 1,629 to the faculty of law, 30 to that of Eastern languages, 622 to the mathematical faculty, 550 to that of natural science, and 2,130 to the medical faculty. There are 53 ecclesiastical seminaries, with 12,227 pupils; 195 gymnasia and pro-gymnasia, with 50,701 pupils; 56 middle-class schools, with 10,888 scholars. There are 19 military gymnasia, but the number of pupils is not given. For females there are 223 gymnasia and pro-gymnasia, having 34,878 pupils; and this does not include the many institutions which are subject to the control of the Fourth Division of the Imperial Chancellery. There are 68 normal schools and training colleges for teachers, having 4,968 students. There are 10 other such institutions supported by non-public funds. The number of elementary schools in operation this year is 25,491, with 1,074,559 pupils.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 5.—"On the Illumination of Lines of Molecular Pressure, and the Trajectory of Molecules," by William Crookes, F.R.S., V.P.C.S.

Induction Spark through Rarefied Gases. Dark Space round the Negative Pole

The author has examined the dark space which appears round the negative pole of an ordinary vacuum tube when the spark from an induction coil is passed through it. He describes many experiments with different kinds of poles, a varying intensity of spark, and different gases, and arrives at the following propositions:—

Illumination of Lines of Molecular Pressure

a. Setting up an intense molecular vibration in a disk of metal by electrical means excites a molecular disturbance which affects the surface of the disk and the surrounding gas. With a dense gas the disturbance extends a short distance only from the metal; but as rarefaction continues, the layer of molecular disturbance increases in thickness. In air at a pressure of ·078 mm. this molecular disturbance extends for at least 8 mm. from the surface of the disk, forming an oblate spheroid around it.

b. The diameter of this dark space varies with the exhaustion; with the kind of gas in which it is produced; with the temperature of the negative pole; and, in a slight degree, with the intensity of the spark. For equal degrees of exhaustion it is greatest in hydrogen and least in carbonic acid, as compared with air.

c. The shape and size of this dark space do not vary with the distance separating the poles; nor, only very slightly, with alteration of battery power; nor with intensity of spark. When the power is great the brilliancy of the unoccupied parts of the tube overpowers the dark space, rendering it difficult of observation; but, on careful scrutiny, it may still be seen unchanged in size, nor does it alter even when, with a very faint spark, it is scarcely visible. On still further reduction of the power, it fades entirely away, but without change of form.

The author describes numerous experiments, devised to ascertain if this visible layer of molecular disturbance is identical with the invisible layer of molecular pressure or stress, the investigation of which has occupied him for some years.

The Electrical Radiometer

One of these experiments is as follows:—An ordinary radiometer is made, with aluminium disks for vanes, each disk coated with a film of mica. The fly is supported by a hard steel cup instead of a glass cup, and the needle point on which it works is connected by means of a wire with a platinum terminal sealed into the glass. At the top of the radiometer bulb a second terminal is sealed in. The radiometer can therefore be connected with an induction coil, the movable fly being made the negative pole.

Passing over the phenomena observed at low exhaustions, the author finds that, when connected with the coil, a halo of a velvety violet light forms on the metallic side of the vanes, the mica side remaining dark throughout these experiments. As the pressure diminishes a dark space is seen to separate the violet halo from the metal. At a pressure of half a millimetre this dark space extends to the glass, and positive rotation commences.

On continuing the exhaustion the dark space further widens out and appears to flatten itself against the glass, and the rotation becomes very rapid.

When aluminium cups are used for the vanes, instead of disks backed with mica, similar appearances are seen. The velvety violet halo forms over each side of the cup. On increasing the exhaustion the dark space widens out, retaining almost exactly the shape of the cup. The bright margin of the dark space becomes concentrated at the concave side of the cup to a luminous focus, and widens out at the convex side. On further exhaustion the dark space on the convex side touches the glass,

when positive rotation commences, becoming very rapid as the dark space further increases in size, and ultimately flattening against the glass.

Convergence of Molecular Rays to a Focus

The subject next investigated is the convergence of the lines of force to a focus, as observed with the aluminium cup. As this could not be accomplished during rapid rotation an instrument was made having the cup-shaped negative pole fixed, instead of movable. On exhaustion, the convergence of the lines of force to a focus at the concave side was well observed. When the dark space is very much larger than the cup it forms an irregular ellipsoid drawn in towards the focal point. Inside the luminous boundary a focus of dark violet light can be seen converging, and, as the rays diverge on the other side of the focus, spreading beyond the margin of the dark space, the whole appearance being strikingly similar to the rays of the sun reflected from a concave mirror through a foggy atmosphere.

Green Phosphorescent Light of Molecular Impact

At very high exhaustions the dark space becomes so large that it fills the tube. Careful scrutiny still shows the presence of the dark violet focus, and the part of the glass on which fall the rays diverging from this focus shows a sharply defined spot of greenish-yellow light. On still further exhaustion, and especially if the cup is made positive, the whole bulb becomes beautifully illuminated with greenish-yellow phosphorescent light.

This greenish-yellow phosphorescence, characteristic of high exhaustions, is frequently spoken of in the paper. It must be remembered, however, that the particular colour is due to the special kind of soft German glass used. Other kinds of glass phosphoresce in a different colour. The phosphorescence takes place only under the influence of the negative pole. At an exhaustion of 4 M[1] no light other than this is seen in the apparatus. At ·9 M the phosphorescence is about at its maximum. When the exhaustion reaches ·15 M the spark has a difficulty in passing, and the green light appears occasionally in flashes only. At ·06 M the vacuum is almost non-conductive, and a spark can be forced through only by increasing the intensity of the coil, and well insulating the tube and wires leading to it. Beyond that exhaustion nothing has been observed.

Focus of Molecular Force

In an apparatus specially constructed for observing the position of the focus, the author found that the focal point of the green phosphorescent light was at the centre of curvature, showing that the molecules by which it is produced are projected in a direction normal to the surface of the pole. Before reaching the best exhaustion for the green light, another focus of blue-violet light is observed; this varies in position, getting further from the pole as the exhaustion increases. In the apparatus described, at an exhaustion of 19·3 M, these two foci are seen simultaneously, the green being at the centre of curvature, while the blue focus is at nearly twice the distance.

Nature of the Green Phosphorescent Light

The author adduces the following characteristics of the green phosphorescent light as distinguishing it from the ordinary light observed in vacuum-tubes at lower exhaustions:—

a. The green focus cannot be seen in the space of the tube, but where the projected beam strikes the glass only.

b. The position of the positive pole in the tube makes scarcely any difference to the direction and intensity of the lines of force which produce the green light. The positive pole may be placed in the tube either at the extremity opposite the negative pole, or below it, or by its side.

c. The spectrum of the green light is a continuous one, most of the red and the higher blue rays being absent; while the spectrum of the light observed in the tube at lower exhaustions is characteristic of the residual gas. No difference can be detected by spectrum examination in the green light, whether the residual gas be nitrogen, hydrogen, or carbonic acid.

d. The green phosphorescence commences at a different exhaustion in different gases.

e. The viscosity of a gas is almost as persistent a characteristic of its individuality as its spectrum. The author refers to a preliminary note and a diagram[2] of the variation of viscosity of air, hydrogen, and other gases at exhaustions between 240 M and ·1 M. From these and other unpublished results, the author finds that the viscosity of a gas undergoes very little diminution between atmospheric pressure and an exhaustion at which the green phosphorescence can be detected. When, however, the spectral and other characteristics of the gas begin to disappear, the viscosity also commences to decline, and at an exhaustion at which the green phosphorescence is most brilliant the viscosity has rapidly sunk to an insignificant amount.

f. The rays exciting green phosphorescence will not turn a corner in the slightest degree, but radiate from the negative pole in straight lines, casting strong and sharply-defined shadows from objects which happen to be in their path. On the other hand, the ordinary luminescence of vacuum tubes will travel hither and thither along any number of curves and angles.

Projection of Molecular Shadows

The author next examines the phenomena of shadows cast by the green light. The best and sharpest shadows are cast by flat disks and not by narrow pointed poles; no green light whatever is seen in the shadow itself, no matter how thin or whatever may be the substance from which it is thrown.

From these and other experiments, fully described in the paper, he ventures to advance the theory that the induction-spark actually illuminates the lines of molecular pressure caused by the electrical excitement of the negative pole. The thickness of the dark space is the measure of the mean length of the path between successive collisions of the molecules. The extra velocity with which the molecules rebound from the excited negative pole keep back the more slowly-moving molecules which are advancing towards that pole. The conflict occurs at the boundary of the dark space, where the luminous margin bears witness to the energy of the collisions.

When the exhaustion is sufficiently high for the mean length of path between successive collisions to be greater than the distance between the fly and the glass, the swiftly-moving, rebounding molecules spend their force, in part or in whole, on the sides of the vessel, and the production of light is the consequence of this sudden arrest of velocity. The light actually proceeds from the glass, and is caused by fluorescence or phosphorescence on its surface. No light is produced by a mica or quartz screen, and the more fluorescent the material the better the luminosity. Here the consideration arises that the greenish yellow light is an effect of the direct impact of the molecules, in the same electrical state, on the surface of the glass. The shadows are not optical, but are molecular shadows, revealed only by an ordinary illuminating effect; this is proved by the sharpness of the shadow when projected from a wide pole.

Phosphorescence of Thin Films

An experiment is next described in which a film of uranium glass, sufficiently thin to show colours of thin plates, is placed in front of a thick plate of the same glass, the whole being inclosed in a tube with terminals, and exhausted to a few millionths of an atmosphere. Of this the following observations are recorded:—

a. The uranium film, being next to the negative pole, casts a strong shadow on the plate.

b. On making contact with the coil, the thin film flashes out suddenly all over its surface with a yellowish phosphorescence, which, however, instantly disappears. The uncovered part of the plate does not become phosphorescent quite suddenly, but the phosphorescence is permanent as long as the coil is kept at work.

c. With an exceedingly faint spark the film remains more luminous than the plate, but on intensifying the spark the luminosity of the film sinks and that of the uncovered part of the plate increases.

d. If a single intense spark be suddenly sent through the tube, the film becomes very luminous, while the plate remains dark.

These experiments are conclusive against the phosphorescence being an effect of the radiation of phosphorogenic ultra-violet light from a thin layer of arrested molecules at the surface of the glass, for were this the case, the film could under no circumstances be superior to the plate.

The momentary phosphorescence and rapid fading of the film prove more than this. The molecular bombardment is too much for the thin film. It responds thereto at first, but immediately gets heated by the impacts, and then ceases to be luminous.

[1] M signifies the millionth of an atmosphere.
[2] *Proc.* Roy. Soc., Nov. 16, 1876, vol. xxv. p. 305.

The plate, however, being thick, bears the hammering without growing hot enough to lose its power of phosphorescing.

Mechanical Action of Projected Molecules

When the coil was first turned on, the thin film was driven back at the moment of becoming phosphorescent, showing that an actual material blow had been given by the molecules. Experiments are next described in which this mechanical action is rendered more evident. A small rotating fly, capable of being moved about in any part of an exhausted bulb, is used as an indicator, and by appropriate means the molecular shadow of an aluminium plate is projected along the bulb. Whether entirely in, or entirely out of the shadow, the indicator scarcely moves, but when immersed so that one-half is exposed to molecular impact, the fly rotates with extreme velocity.

Magnetic Deflection of Lines of Molecular Force

With this apparatus another phenomenon was investigated. It is found that the stream of molecules, whose impact on the glass occasions evolution of light, is very sensitive to magnetic influence, and by bringing one pole of an electro-magnet—or even of a small permanent magnet—near, the shadow can be twisted to the right or to the left.

When the little indicator was placed entirely within the molecular shadow, no movement was produced. As soon, however, as an adjacent electro-magnet was excited, the shadow was twisted half off the indicator, which immediately rotated with great speed.

The Trajectory of Molecules

The amount of deflection of the stream of molecules forming a shadow is in proportion to the magnetic power employed.

The trajectory of the molecules forming the shadow is curved; when under magnetic influence the action of the magnet is to twist the trajectory of the molecules round in a direction at an angle to their free path, and to a greater extent, as they are nearer the magnet: the direction of twist being that of the electric current passing round the electro-magnet.

Laws of Magnetic Deflection

An apparatus was constructed so that the deflection of a spot of light was used instead of that of a shadow, a horse-shoe magnet being placed underneath the negative pole to deflect the trajectory. The action of the north pole being to give the line of molecules a spiral twist one way, and that of the south pole being to twist it the other way, the two poles side by side compel the line to move in a straight line up or down along a plane at right angles to the plane of the magnet and a line joining its poles.

The ray of molecules does not appear to obey Ampère's law, as it would were it a perfectly flexible conductor, joining the negative and the positive pole. The molecules are projected from the negative, but the position of the positive pole, whether in front, at the side, or even behind the negative pole, has no influence on their subsequent behaviour, either in producing phosphorescence, or mechanical effects, or in their magnetic deflection. The magnet gives their line of path a spiral twist greater or less according to its power, but diminishing as the molecules get further off.

Numerous experiments were tried in this apparatus with different gases, and with the magnet in and out of position.

Working with exhausted air it was found that the spot of green phosphorescence on the screen is visible at an exhaustion of 102·6 M, when the mean free path of the molecules, measured by the thickness of the dark space round the negative pole, is only 12 mm. Hence, it follows that a number of molecules sufficient to excite green phosphorescence on the screen are projected the whole distance from the pole to the screen, or 102 mm., without being stopped by collisions.

Alteration of Molecular Velocity

If we suppose the magnet to be permanently in position, and thus to exert a uniform downward pull on the molecules, we perceive that their trajectory is much curved at low exhaustions, and gets flatter as the exhaustion increases. A flatter trajectory corresponds to a higher velocity. This may arise from one of two conditions; either the initial impulse given by the negative pole is stronger, or the resisting medium is rarer. The latter is probably the true one. The molecules which produce the green phosphorescence must be looked upon as in a state differing from those arrested by frequent collisions. The latter impede the velocity of the free molecules and allow longer time for magnetism to act on them; for, although the deflecting force of magnetism might be expected to increase with the velocity of the molecules, Prof. Stokes has pointed out that it would have to increase as the square of the velocity, in order that the deflection should be as great at high as at low velocities.

Comparing the free molecules to cannon-balls, the magnetic pull to the earth's gravitation, and the electrical excitation of the negative pole to the explosion of the powder in the gun, the trajectory will be flat when no gravitation acts, and curved when under the influence of gravitation. It is also much curved when the ball passes through a dense resisting medium, it is less curved when the resisting medium gets rarer; and, as already shown, intensifying the induction spark, equivalent to increasing the charge of powder, gives greater initial velocity, and, therefore, flattens the trajectory. The parallelism is still closer if we compare the evolution of light seen when the shot strikes the target, with the phosphorescence on the glass screen from molecular impacts.

Focus of Heat of Molecular Impact

The author finally describes an apparatus in which he shows that great heat is evolved when the concentrated focus of rays from a nearly hemispherical aluminium cup is deflected sideways to the walls of the glass tube by a magnet. By using a somewhat larger hemisphere and allowing the negative focus to fall on a strip of platinum foil, the heat rises to the melting point of platinum.

An Ultra-gaseous State of Matter

The paper concludes with some theoretical speculations on the state in which the matter exists in these highly exhausted vessels. The modern idea of the gaseous state is based upon the supposition that a given space contains millions of millions of molecules in rapid movement in all directions, each having millions of encounters in a second. In such a case the length of the mean free path of the molecules is exceedingly small as compared with the dimensions of the vessel, and the properties which constitute the ordinary gaseous state of matter, which depend upon constant collisions, are observed. But by great rarefaction the free path is made so long that the hits in a given time may be disregarded in comparison to the misses, in which case the average molecule is allowed to obey its own motions or laws without interference; and if the mean free path is comparable to the dimensions of the vessel, the properties which constitute gaseity are reduced to a minimum, and the matter becomes exalted to an ultra-gaseous state, in which the very decided but hitherto masked properties now under investigation come into play.

Rays of Molecular Light

In speaking of a ray of molecular light, the author has been guided more by a desire for conciseness of expression than by a wish to advance a novel theory. But he believes that the comparison, under these special circumstances, is strictly correct, and that he is as well entitled to speak of a ray of molecular or emissive light when its presence is detected only by the light evolved when it falls on a suitable screen, as he is to speak of a sunbeam in a darkened room as a ray of vibratory or ordinary light when its presence is to be seen only by interposing an opaque body in its path. In each case the invisible line of force is spoken of as a ray of light, and if custom has sanctioned this as applied to the undulatory theory, it cannot be wrong to apply the expression to emissive light. The term emissive light must, however, be restricted to the rays between the negative pole and the luminous screen: the light by which the eye then sees the screen is, of course, undulatory.

The phenomena in these exhausted tubes reveal to physical science a new world—a world where matter exists in a fourth state, where the corpuscular theory of light holds good, and where light does not always move in a straight line; but where we can never enter, and in which we must be content to observe and experiment from the outside.

Chemical Society, December 5.—Dr. Gladstone, president, in the chair.—Prof. Tidy read a lengthy and important paper on the processes for determining the organic purity of potable waters. The conclusions at which the author arrives, after many experiments and a careful examination of the comparative analyses of over 1,600 waters, may be briefly summed up as follows: *The ammonia process* furnishes results which are marked by singular inconstancy, and are not delicate enough to allow the recognition

and classification of the finer grades of purity or impurity. The errors incidental to the process form an array of difficulties which become infinitely serious, seeing that the range (as regards albumenoid ammonia) between pure and dirty waters is comparatively small. *The combustion process* has all the evils of evaporation to encounter, but the organic carbon estimation is trustworthy; the organic nitrogen determination, however, scarcely yields absolutely trustworthy evidence on which to found an opinion as to the probable source of the organic matter. The process, nevertheless, is of great value. *The oxygen* (permanganate) *process* avoids the errors incidental to evaporation; its results (when properly used) are constant and extremely delicate; it draws a sharp line between the putrescent or probably pernicious and the non-putrescent or probably harmless organic matter; by it a bad water can never be passed as good. As far as the three processes are concerned, the oxygen and combustion processes give closely concordant results, whilst those yielded by the ammonia process are often at direct variance with both.

Photographic Society, November 12.—James Glashier, F.R.S., in the chair.—After the medals awarded for the best pictures in the exhibition had taken place, a paper by Leon Warnerke was read—photographic notes from a travel in Russia with exhibition of various works, apparatus, and materials. John Thomson, F.R.G.S., described his photographic experiences in Cyprus. Mr. Warnerke described the artistic and scientific progress of photography in Russia as having arrived at a high state of perfection, as also the very important position which photo-lithography occupies in the Government establishments of the country where large maps of the Russian frontiers are produced.

BOSTON, U.S.A.

American Academy of Arts and Sciences, November 13.—Hon. Charles Francis Adams in the chair.—Prof. Mitchell, of the U.S. Coast Survey, read a monograph upon the tides of the Gulf of Maine. The results of the late surveys prove that a lift of the tide far out from shore requires an almost inappreciable time to be felt along the coast. The variations in the tides were shown to come from the north rather than from the south. In the discussion of Prof. Mitchell's paper Prof. Benjamin Peirce maintained that the results corroborated his own theory of vibrations and nodal points.—Prof. A. E. Dolbear, of Tuft's College, read a paper upon his claims of the invention of the speaking-telephone, and exhibited his early models and also a great variety of new speaking-telephones. Among these were a Morse sounder transformed into a telephone; a miniature voltaic cell, one plate of which was spoken against, and speech thus transmitted; an electrophone depending upon the variable resistance of the extra spark, which can transmit speech over a distance of more than two hundred miles; various forms of transmitters and modifications of Reiz's telephone, which Prof. Dolbear said are innumerable.—Mr. N. D. C. Hodges exhibited a new instrument for determining the magnetic dip. Two soft iron bars joined at right angles move on a vertical graduated circle. A small magnet with a mirror is suspended at the point of crossing of the pieces of soft iron. When the latter make equal angles with the line of dip, the magnet remains at zero. By four simple reversals and measurements the residual magnetism of the iron is eliminated. The instrument gives very constant results and requires less time to make an observation than the ordinary dipping needle.

PARIS

Academy of Sciences, December 2.—M. Fizeau in the chair.—The following papers were read:—On the torsion of prisms of mixtilinear base, and on a peculiarity which may be presented by certain uses of the logarithmic co-ordinate of the isothermal cylindrical system of Lamé, by M. De Saint Venant.—M. Marey was elected Member for the section of Medicine and Surgery in the room of the late M. Cl. Bernard, the other candidates being MM. Bert, Charcot, and Gubler.—Experimental researches on meteoritic nickelised irons; mode of formation of concretionated syssidera, by M. Meunier. The mixture of chlorides of iron and nickel gives, by reduction in hydrogen, alloys perfectly definite, and sometimes beautifully crystallised. Association of the alloys together can be effected in two ways (specified). Next, grains of peridot or fragments of dunite can easily be covered with a continuous coat of various alloys of iron and nickel, the metallic concretion sometimes penetrating into the fine fissures of the stone. By placing the pieces of rock repeatedly in the incrusting medium fed with diverse mixtures of the two chlorides, superposed deposits of various alloys are obtained, and a complete facsimile of the cosmic rocks in question.—On a new phenomenon of static electricity, by M. Govi. A reference to his former experiments on the phenomenon studied by M. Duter. Different liquids having given different amounts of contraction, and mercury no contraction, he inferred condensation of the liquid against the walls of the jar. He apparently doubts M. Duter's conclusion.—On the electromotive force of induction arising from rotation of the sun; determination of its amount and direction, whatever the distance of the induced body, by M. Quet. He gives a mathematical estimate of the force, with formulæ.—Note on the effects of vapours of sulphide of carbon, by M. Poincaré. He experimented on animals, with reference to the symptoms presented by workmen in vulcanisation of caoutchouc. Guinea pigs and frogs resist the action much less than man; and the period of excitation is wanting in them, the manifestations being mainly paralytic. The auricles are distended with dark blood, livid spots appear in the lungs, the brain is often reduced to a diffluent pulp, and drops apparently of sulphide of carbon form in the cerebral vessels, blocking the passage of blood corpuscles and sometimes causing rupture. The author considers the use of vulcanised caoutchouc should be restricted to really useful objects, and that the manufacture of small balloons and toys of it should be interdicted.—On the mode of formation of some phylloxeric nodosities, by M. d'Arbaumont.—Latitude of Algiers, and fundamental azimuth of the Algerian triangulation, by M. Perrier. The author describes his method.—Nebulæ discovered and observed at the Observatory of Marseilles, by M. Stephan.—Double stars; certain groups of perspective, by M. Flammarion.—Evaluation of a definite integral, by M. Appell.—On the repulsion resulting from radiation, by Mr. Crookes.—Note on cholalic acid, by M. Destrem.—Researches on vaso-motor nerves, by MM. Dastre and Morat. The principal branch of termination of the sciatic nerve plays, with regard to the region of the finger, the *rôle* of a vaso-constrictor nerve, and there is no ground for supposing, in this nerve-trunk, vaso-dilator nerves either more or less than in the cervical cord of the sympathetic. Thus the controverted point as to whether the sciatic is a vaso-dilator nerve, is answered in the negative. The authors generalise the results, applying them to the nervous trunks which go to the skin.—On the cardiac and respiratory effects of irritations of certain sensitive nerves of the heart, and on the cardiac effects produced by irritation of sensitive nerves of the respiratory apparatus, by M. François-Franck. Different effects are produced according as an irritating injection (say hydrate of chloral) is made into the right or the left heart; in the former case there is diastolic stoppage of the heart, in the latter systolic. (The mechanism of these effects is studied.)—On the change of form of fixed cells of loose connective tissue in artificial œdema, by M. Renaut.

CONTENTS

THURSDAY, DECEMBER 19, 1878

PARADOXICAL PHILOSOPHY

Paradoxical Philosophy. A Sequel to the "Unseen Universe." (London: Macmillan and Co., 1878.)

ON opening this book, the general appearance of the pages, and some of the phrases on which we happened to light made us somewhat doubtful whether it lay within our jurisdiction, as it is not the practice of NATURE to review either novels or theological works.

In the dedication, however, the book is described as an account of the *Proceedings* of a learned society, a species of literature which we are under a special vow to rescue from oblivion, even when, as in this case, the proceedings are those of one of those jubilee meetings, in which learned men seem to aim rather at being lively than scientific.

On the title-page itself there is no name to indicate whether the author is one of those who by previous conviction have rendered themselves liable to our surveillance, but on the opposite page we find "The Unseen Universe; or, Physical Speculations on a Future State," to which this book is a "Sequel," ascribed to the well-known names of Balfour Stewart and P. G. Tait.

Mr. Browning has expressed his regret that the one volume in which Rafaelle wrote his sonnets, and the one angel which Dante was drawing when he was interrupted by "people of importance," are lost to the world. We shall therefore make the most of our opportunity when two eminent men of science, "driven," as they tell us, "by the exigencies of the subject," have laid down all the instruments of their art, shaken the very chalk from their hands, and, locking up their laboratories, have betaken themselves to those blissful country seats where Philonous long ago convinced Hylas that there can be no heat in the fire and no matter in the world; and where in more recent times, Peacock and Mallock have brought together in larger groups the more picturesque of contemporary opinions.

In this book we do not indeed catch those echoes of well-known voices in which the citizens of the "New Republic" tell us how they prefer to regard themselves as thinking, taking care, all the while, that no actual thought shall disturb their enjoyment of the luxury of extravagant opinion. The members of the Paradoxical Society, with their guest, Dr. Hermann Stoffkraft, are far too earnest to adopt this pose of mind, but they exhibit that sympathy in fundamentals overlaid with variety in opinions which is one of the main conditions of good-fellowship. Dr. Stoffkraft, in spite of his name and of his office as the single-handed opponent of the thesis of the book, makes it his chief care so to brandish his materialistic weapons as not to hurt the feelings of his friends; and when, near the end of the book, he gets a little out of temper, it is about matters with which a materialist, as such, has no concern.

As the book is not a novel there is no literary reason for not telling "what became of the Doctor," as narrated in the last chapter. He goes to Strathkelpie Castle to take part in an investigation of spiritualistic phenomena. He begins by detecting the mode in which one young lady performs her spirit-rapping, but forthwith falls into an "electro-biological" courtship of another, and, this proving successful, he is persuaded by his wife and her priest to renounce the black arts in the lump as works of the foul fiend; and then we are told that, having quieted his spirit by a few evolutions in four dimensions, he has now settled down to compose his "Exposition of the Relations between Religion and Science," which he intends to be a thoroughly matured production.

The Doctor—and, indeed, most of the other characters—are no mere materialised spirits, or opinions labelled with names of the *Euphranor* and *Alciphron* type. They do not reduce their subject to a *caput mortuum* by an exhaustive treatment, but take care, like well-bred people, to drop it and pass on to another before we have time to suspect that the last word has been said.

We cannot accuse the authors of leading us through the mazy paths of science only to entrap us into some peculiar form of theological belief. On the contrary, they avail themselves of the general interest in theological dogmas to imbue their readers at unawares with the newest doctrines of science. There must be many who would never have heard of Carnot's reversible engine, if they had not been led through its cycle of operations while endeavouring to explore the Unseen Universe. No book containing so much thoroughly scientific matter would have passed through seven editions in so short a time without the allurement of some more human interest.

Nor need we fear to draw down on NATURE the admonition which fell on the inner ear of the poet—

> "Thou pratest here where thou art least;
> This faith hath many a purer priest,
> And many an abler voice than thou."

For even those words and phrases which seemed at first sight to remove the book from the field of our criticism, are found on a nearer view to have acquired a new, and indeed a *paradoxical* sense, for which no right of sanctuary can be claimed.

The words on the title-page: "In te, Domine, speravi, non confundar in æternum," may recall to an ordinary reader the aspiration of the Hebrew Psalmist, the closing prayer of the "Te Deum," or the dying words of Francis Xavier; and men of science, as such, are not to be supposed incapable either of the nobler hopes or of the nobler fears to which their fellow-men have attained. Here, however, we find these venerable words employed to express a conviction of the perpetual validity of the "Principle of Continuity," enforced by the tremendous sanction, that if at any place or at any time a single exception to that principle were to occur, a general collapse of every intellect in the universe would be the inevitable result.

There are other well-known words in which St. Paul contrasts things seen with things unseen. These also are put in a prominent place by the authors of the "Unseen Universe." What, then, is the Unseen to which they raise their thoughts?

In the first place the luminiferous æther, the tremors of which are the dynamical equivalent of all the energy which has been lost by radiation from the various systems of grosser matter which it surrounds. In the second place a still more subtle medium, imagined by Sir William Thomson as possibly capable of furnishing an explana-

tion of the properties of sensible bodies; on the hypothesis that they are built up of ring vortices set in motion by some supernatural power in a frictionless liquid: beyond which we are to suppose an indefinite succession of media, not hitherto imagined by any one, each manifoldly more subtle than any of those preceding it. To exercise the mind in speculations on such media may be a most delightful employment for those who are intellectually fitted to indulge in it, though we cannot see why they should on that account appropriate the words of St. Paul.

NATURE is a journal of science, and one of the severest tests of a scientific mind is to discern the limits of the legitimate application of scientific methods. We shall therefore endeavour to keep within the bounds of science in speaking of the subject-matter of this book, remembering that there are many things in heaven and earth which, by the selection required for the application of our scientific methods, have been excluded from our philosophy.

No new discoveries can make the argument against the personal existence of man after death any stronger than it has appeared to be ever since men began to die, and no language can express it more forcibly than the words of the Psalmist:—

"His breath goeth forth, he returneth to his earth; in that very day his thoughts perish."

Physiology may supply a continually increasing number of illustrations of the dependence of our actions, mental as well as bodily, on the condition of our material organs, but none of these can render any more certain those facts about death which our earliest ancestors knew as well as our latest posterity can ever learn them.

Science has, indeed, made some progress in clearing away the haze of materialism which clung so long to men's notions about the soul, in spite of their dogmatic statements about its immateriality. No anatomist now looks forward to being able to demonstrate my soul by dissecting it out of my pineal gland, or to determine the quantity of it by the process of double weighing. The notion that the soul exerts force lingered longer. We find it even in the late Isaac Taylor's "Physical Theory of a Future State." It was admitted that one body might set another in motion; but it was asserted that in every case, if we only trace the chain of phenomena far enough back, we must come to a body set in motion by the direct action of a soul.

It would be rash to assert that any experiments on living beings have as yet been conducted with such precision as to account for every foot-pound of work done by an animal in terms of the diminution of the intrinsic energy of the body and its contents; but the principle of the conservation of energy has acquired so much scientific weight during the last twenty years that no physiologist would feel any confidence in an experiment which showed a considerable difference between the work done by an animal and the balance of the account of energy received and spent.

Science has thus compelled us to admit that that which distinguishes a living body from a dead one is neither a material thing, nor that more refined entity, a "form of energy." There are methods, however, by which the application of energy may be directed without interfering with its amount. Is the soul like the engine-driver, who does not draw the train himself, but, by means of certain valves, directs the course of the steam so as to drive the engine forward or backward, or to stop it?

The dynamical theory of a conservative material system shows us, however, that *in general* the present configuration and motion determines the whole course of the system, exceptions to this rule occurring only at the instants when the system passes through certain isolated and singular phases, at which a strictly infinitesimal force may determine the course of the system to any one of a finite number of equally possible paths, as the pointsman at a railway junction directs the train to one set of rails or another. Prof. B. Stewart has expounded a theory of this kind in his book on "The Conservation of Energy," and MM. de St. Venant and Boussinesq have examined the corresponding phase of some purely mathematical problems.

The science which rejoices in the name of "Psychophysik" has made considerable progress in the study of the phenomena which accompany our sensations and voluntary motions. We are taught that many of the processes which we suppose entirely under the control of our own will are subject to the strictest laws of succession, with which we have no power of interfering; and we are shown how to verify the conclusions of the science by deducing from it methods of physical and mental training for ourselves and others.

Thus science strips off, one after the other, the more or less gross materialisations by which we endeavour to form an objective image of the soul, till men of science, speculating, in their non-scientific intervals, like other men on what science may possibly lead to, have prophesied that we shall soon have to confess that the soul is nothing else than a function of certain complex material systems.

Men of science, however, are but men, and therefore occasionally contemplate their souls from within. Those who, like Du Bois-Reymond, cannot admit that sensation or consciousness can be a function of a material system, are led to the conception of a double mind.

"On the one side the acting, inventing, unconscious material mind, which puts the muscles into motion, and determines the world's history; this is nothing else but the mechanics of atoms, and is subject to the causal law, and on the other side the inactive, contemplative, remembering, fancying, conscious, immaterial mind, which feels pleasure and pain, love, and hate; this one lies outside of the mechanics of matter, and cares nothing for cause and effect."

We might ask Prof. Du Bois-Reymond which of these it is that does right or wrong, and knows that it is his act, and that he is responsible for it, but we must go on to the other view of the case, which Dr. Stoffkraft alludes to at p. 78, although by some law of the *Paradoxical*, he is not allowed to pursue a subject which might have afforded excellent sport to the Society.

"I feel myself compelled to believe," says the learned Doctor, "that all kinds of matter have their motions accompanied with certain simple sensations. In a word, all matter is, in some occult sense, alive."

This is what we may call the "levelling up" policy, and it has been expounded with great clearness by Prof.

von Nägeli in a lecture, of which a translation was given in NATURE, vol. xvi. p. 531.

He can draw no line across the chain of being, and say that sensation and consciousness do not extend below that line. He cannot doubt that every molecule possesses something related, though distantly, to sensation, "since each one feels the presence, the particular condition, the peculiar forces of the other, and, accordingly, has the inclination to move, and under circumstances really begins to move—becomes alive as it were;" . . . "If, therefore, the molecules feel something which is related to sensation, then this must be pleasure if they can respond to attraction and repulsion, *i.e.*, follow their inclination or disinclination; it must be displeasure if they are forced to execute some opposite movement, and it must be neither pleasure nor displeasure if they remain at rest."

Prof. von Nägeli must have forgotten his dynamics, or he would have remembered that the molecules, like the planets, move along like blessed gods. They cannot be disturbed from the path of their choice by the action of any forces, for they have a constant and perpetual will to render to every force precisely that amount of deflexion which is due to it. Their condition must, therefore, be one of unmixed and unbroken pleasure.

But even if a man were built up of thinking atoms would the thoughts of the man have any relation to the thoughts of the atoms? Those who try to account for mental processes by the combined action of atoms do so, not by the thoughts of the atoms, but by their motions.

Dr. Stoffkraft explains the origin of consciousness at p. 77 and at p. 107. We recommend to his attention Mr. Herbert Spencer's statement in his "Principles of Psychology," § 179, where he shows in a most triumphant manner how, under certain circumstances, "there must arise a consciousness." Such statements, carefully studied, may contribute to the further progress of science in the path which we have been describing, by showing more clearly that consciousness cannot be the result of a plexus of nervous communications any more than of a congeries of plastidule souls.

Personality is often spoken of as if it were another name for the continuity of consciousness as reproduced in memory, but it is impossible to deal with personality as if it were something objective that we could reason about. My knowledge that I am is quite independent of my recollection that I was, and also of my belief that, for a certain number of years, I have never ceased to be. But as soon as we plunge into the abysmal depths of personality we get beyond the limits of science, for all science, and, indeed, every form of human speech, is about objects capable of being known by the speaker and the hearer. Whenever we pretend to talk about the Subject we are really dealing with an Object under a false name, for the first proposition about the Subject, namely, "I am," cannot be used in the same sense by any two of us, and therefore can never become part of science at all.

The progress of science, therefore, so far as we have been able to follow it, has added nothing of importance to what has always been known about the physical consequences of death, but has rather tended to deepen the distinction between the visible part, which perishes before our eyes, and that which we are ourselves, and to show that this personality, with respect to its nature as well as to its destiny, lies quite beyond the range of science.

J. CLERK MAXWELL

SCIENCE CLASS-BOOKS

The London Science Class-Books. Edited by G. Carey Foster, F.R.S., and Philip Magnus, B.Sc. *Biological Series.* 1. *Botany—Outlines of Morphology and Physiology.* 2. *Botany—Outlines of Classification of Plants.* By W. Ramsay McNab, M.D. 3. *Zoology of the Vertebrate Animals.* 4. *Zoology of the Invertebrate Animals.* By Prof. A. Macalister. (London: Longmans, Green and Co., 1878.)

THE editors of this series of Elementary Science Class-Books intend that the works shall all be composed with special reference to school teaching; that they shall be suited to the capabilities and comprehension of boys and girls during their school course, while they shall at the same time afford trustworthy and accurate information presented in such a way that it may serve as a basis for more advanced study. In thus announcing their scheme the editors would seem to indicate that they have learned to appreciate the very great want that exists in all our public schools of just such a series of class-books as they undertake to supply; and though the standard at which they aim cannot be regarded as a high one, still most judges will agree that it is both a suitable and a proper one, and it is one that we wish the editors every success in their carrying of it out. If the natural sciences are to be taught in our schools the scholars must have class-books of these sciences, and we take it as a good sign that the demand for such class-books is in this new series being supplied. The information in the present series is to be accurate and trustworthy, and the names of the authors of the four books already published of the biological series is a sufficient guarantee that this is so. The information is to be suited to the capabilities of girls and boys during their school-days, and still it is to be presented in such a way as to form the basis of a higher study. The authors' names, however distinguished, will be no necessary guarantee of this. It is not given to every one to be able to write an elementary book that may serve as the basis for a more advanced study. In the first two class-books on our list the author indeed does not even make the attempt. In his preface Prof. McNab declares that it has been thought advisable to make his class-books on Botany such as would serve as a basis for the teaching in the higher classes of schools, and such as would supply the wants of medical students and others wishing to acquire a knowledge of the subject. We think this a pity, for we certainly at once miss that strictly elementary treatment of the subject, that full statement and discussion of the fundamental facts thereof, which we were led to expect, not by the author, but by his editors; and however useful and instructive these two class-books may be, the aim that we fancy they should have kept in view is lost in the endeavour, to quote their authors' own words, that they should "serve as an introduction to the celebrated text-book of the distinguished German botanist, whose "Lehr-

buch der Botanik" has been lately translated by Professors Bennett and Thiselton Dyer, and which has been published in the Clarendon Press Series. After the class-book is mastered the manual is to be studied."

The first class-book treats of the morphology and physiology of plants. In it three chapters are devoted to the morphology, and five chapters to the physiology. Like the little text-book of Thomé, it presents to the student's view a cell of a fungal plant, as an instance of the simplest conception of a vegetable cell; but while such a structure is a cell, surely it is neither the most simple, nor certainly is it the most perfect form of a cell to be the first given to a beginner. The structure of the cell-wall and its growth are well treated of, but we do not like to find the word "absorption" used in the account of the formation of wood vessels, bordered pits, &c.; this word is very likely to mislead the student, especially when he finds it used in another chapter to denote the imbibition of moisture. So far as we know, the cellulose cell-wall is in no case decomposed molecule by molecule, and these are not taken up as they are laid down; and yet would it not require that all this should be done ere the term internal absorption could be correctly applied?

The subjects of the formation of new cells, and of the substances contained in protoplasm, are thoroughly well done; a few technical words here and there occurring, and not explained, will demand the attention of the teacher. It would have been well had the chemical formulæ for starch, and some of the other members of the cellulose group, be given, in addition to the formula for cellulose. Such formulæ would have enabled the student the better to understand the change of one organic compound, such as starch, into another, such as sugar. In passing we may observe that Sach's "Text-Book" is very defective also in this respect; indeed, the word sugar is not to be found in its index, and nowhere in the text is its composition given. Chapter II. treats of the tissues. Perhaps a few more technical words are here used than are absolutely necessary, but no doubt a great deal of exactness is attained by their use. The details are compressed into the smallest compass, but are quite up to the very newest facts; and because each word in such a chapter stands to express a good deal, we venture to take exception to the employment of the word development in the following sentence:—"Permanent tissue is formed by the further development of those cells of the meristem which have lost the power of dividing," and which have assumed some permanent form. It is just in such cells that no true development can take place. In Chapter III. the external conformation of plants is briefly treated of. In Chapter IV. we find The Nutrition of Plants. "As the chemical elements out of which the plant constructs its organic substance," the author enumerates "carbon, hydrogen, oxygen, nitrogen, and sulphur;" might he not have added iron? True, it may still be doubted whether iron forms an integral constituent of the chemical formula of chlorophyll, but without it the plant, as a plant, cannot "construct its organic substance." The processes of assimilation and metastasis are very conveniently tabulated, and the student carefully going over this chapter will, we should think, be able to understand it without any help from a teacher. It is a chapter which does the author great credit, and almost comes up to the standard of the editors. Only one comment we venture to make on it. Taking it for granted that Nägeli has somewhere said that the *ultimate* solid particles of plants are more or less crystalline, a conception beyond our powers, how then can the mode of their growth be very different from that occurring in minerals, crystals, &c.? The Fifth Chapter is concerned with the relation of the general conditions of the life of plants. The Sixth treats of the growth of plants. The author limits the term growth to an increase in bulk, accompanied by a deposit of some new constructive material. He thus considerably limits the term, as we believe correctly defined by Herbert Spencer. The subject of the tension found in growing parts is treated at a length out of due proportion to the treatment of the other subjects in the volume, and in such a class-book we think it would have sufficed to have given the results of Sachs' experiments on the periodicity of growth, and not to have copied his tabular statements in such detail. The last chapters are devoted to the subjects of the reproduction of plants and their classification.

The second class-book, also by Prof. McNab, is a continuation of the first, but is devoted to the special morphology and outlines of a classification of plants. The classification of the flowerless plants is based on that of Sachs, though this is here and there altered. We think the author acts rather prematurely in placing such families as those of the Chytridiaceæ and Chlorochytridiaceæ yet as among the Oosporeæ. The Lichens are regarded as ascomycetous fungi parasitic on algæ; this we think right. Cohn's very convenient names Bryophyta and Pteridophyta are adopted for the groups of the mosses and the Vascular Cryptogams; and as an instance that very recent facts in the life history of the Cryptogams has not escaped the attention of the author, we perceive that he mentions that the branches of the Horse-tails arise in an axillary manner, and not as believed up to Janczewski's researches, in an endogenous manner. So much space is devoted to the Cryptogams that the treatment of the flowering plants is greatly curtailed. These two little volumes have been well and painstakingly compiled; they may be safely placed in the hands of a student who knows some little of the subject about which they treat, and they cannot fail to be very useful to every teacher of the natural sciences in our upper schools.

The two little volumes on "Zoology," by Prof. Macalister, are of quite another type. They aim at presenting, in as simple a form as possible, the leading characters of vertebrate and invertebrate animals. In these, special care has been taken to dispense with all unnecessary technicalities, and when such, owing to the nature of the case, arise, they are carefully explained. We would have wished something more novel than most of the woodcut illustrations, though we acknowledge that among the invertebrates some of the illustrations are modern; but we feel glad to find each volume with a carefully drawn up index, the loss of which will doubtless be felt by many a student of Prof. McNab's two class-books; while they will be found to afford trustworthy and accurate information. This will be found given in a way quite suited to the comprehension of our average minded girls and boys.

OUR BOOK SHELF

Studies from the Physiological Laboratory in the University of Cambridge. Edited by the Trinity Prælector in Physiology. Part III. (Cambridge: Printed at the University Press, 1877.)

THIS volume of 165 pages, together with numerous elaborate plates—the largest of the Cambridge Biological "Studies" yet published—is a most pleasing indication of a vigorous spirit of research in a body which has by many been thought to be solely educational. It is not, indeed, the number of the memoirs and papers here collected, but their quality, which makes them worthy of the university whose name they bear on the title-page. In quantity they are far from commensurate with the latent means and opportunities of the colleges and University of Cambridge, but in their thoroughness and dignity they display a spirit which would do honour to any university. They represent a new feature in the history of biological science in this country, viz., the recognised official charge of biological research in high places, where it has been too long neglected. The Biological and Physiological School of Cambridge is a rare and valuable "sport" in the offspring of an organism of decided conservative tendencies: may we not hope that, ere long, Oxford will give birth to a similar healthy monster?

All the papers of this volume have been published before in the *Journal of Anatomy and Physiology*, or elsewhere; but we are not the less glad, on that account, to see the present collection. If the cause of scientific research were more secure in England than it is, the publication of special collections of memoirs of the various schools might be held to be an unnecessary luxury, or even—since rivalry may become ungenerous—a positively dangerous habit. Under our present conditions, however, it is not only pleasant to be reminded now and again of the various centres of organised research among us, but it materially strengthens the hands of English scientific workers to invest the different growing schools with somewhat of a personal and individual interest.

The volume contains physiological and anatomical papers, chiefly in zoology, but also in botany. Dr. Michael Foster and Mr. Dew-Smith contribute a most interesting paper on the effects of the constant current on the heart, which is a continuation of the work they did on the reaction of the snail's heart to electrical currents. Mr. J. N. Langley has a paper on the action of pilocarpin on the submaxillary gland of the dog. Mr. Gaskell reprints one of his papers on the vaso-motor nerves of striated muscles. Mr. F. M. Balfour contributes an important section of his now published monograph on the development of elasmobranch fishes, viz., the development of their spinal nerves; as well as a paper on the spinal nerves of amphioxus. Mr. Marshall follows with a paper on the development of the nerves in birds. Mr. Bullar has a paper, with plates, on the generative organs of parasitic isopoda; Mr. Bridge one on the cranial osteology of *Amia calva*, also admirably illustrated; and Mr. Sidney Vines a short communication on the digestive ferment of nepenthes.

The American Quarterly Microscopical Journal, containing the Transactions of the New York Microscopical Society. Edited by Romyn Hitchcock. Vol. I., No. 1. October. (New York: Hitchcock and Hall, 1878.)

COMMENCING, as this new journal does, on the lines of our own *Quarterly Journal of Microscopical Science*, and somewhat under the like auspices, we trust it may have the same worthy career, and be equally well thought of. The first number is beautifully printed on excellent paper, and contains some eighty-two pages belonging to the journal proper, while the *Transactions* of the New York Microscopical Society extend to some sixteen pages more. The six plates, on their part, are good, but not up to the same standard of execution as the letterpress, and fall a good deal below those that generally appear in our own microscopical journal. The chief contents of this part are—1. On the Sting of the Honey Bee, by J. D. Hyatt. Plates I. and II. 2. Description of some New Species of Diatoms, by H. L. Smith. Plate III. 3. Observations on several Forms of Saprolegniaceæ, by F. B. Hine. Plates IV. to VI. Only the first part of this paper is given, and the list of works referred to by the author is given at the end of the paper, so perhaps it may be premature to suggest that English writers on this subject are not altogether wanting, as he would seem to think; but has he not Dr. Lindstedt's Synopsis, and does not this refer to such? 4. The Oil Immersion Lenses of Zeiss compared with the Objectives of Spencer and Sons, by H. L. Smith. 5. On the Microscopical Examination of Fibers (fibres?), by W. H. Seaman. 6. Emigration in Passive Hyperæmia, by W. T. Belfield. 7. On a New Device for Dark-field Illumination, by W. Leighton. Among the shorter articles we may mention one reprinted, with full acknowledgment, on the Spore Formation in the Mesocarpeæ, from our own columns, and an account of the National Microscopical Congress held last August at Indianapolis, Indiana.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Locusts and Sun-Spots

As the locust (*Œdipoda migratoria*, or *Acridium perigrinum?*) is a frequent and occasionally aggravating accompaniment of drought and famine, it cannot but be interesting to notice that periodical incursions of this insect into the temperate zone are apparently regulated in some way by the terrestrial meteorological abnormalities which accompany the varying phases of the sun-spots.

Dr. F. G. Hahn, in his treatise "Ueber die Beziehungen der Sonnenfleckenperiode zu meteorologischen Erscheinungen," atfer remarking that locusts will probably only visit the temperate regions in great numbers during unusually hot and dry years (on account of the brood), and abandon them again in wet and cold years, shows, from a list furnished by Dr. W. Köppen, of Hamburg, embracing the period 1800-1862, that in Europe they begin coming about the epoch of minimum sun-spot, paying annual visits from thence up to the epoch of maximum sun-spot, after which they disappear altogether until the next following epoch of sun-spot minimum.

In the following table I give Dr. Hahn's dates for their visitations in Europe, with some additional ones on the authority of M. Camille Flammarion, and Mr. Walford of the Statistical Society, which include other regions of the north temperate zone. I also give the corresponding sun-spot epochs in each case, according to Wolf. The capital letters in parentheses attached to the dates, indicate the authorities respectively alluded to above.

LOCUSTS.	SUN-SPOTS.	
Date of Visitation in Temperate Zone.	Min.	Max.
1613 (F)	1610·8	1615·0
1690 (F)	1689·5	1693·0
1748[1]–1749 (F)	1745 0	1750·3
1800 annually up to 1806 (H)	1798·3	1804·2
1811 ,, ,, 1816 (H)	1810·6	1816·4
1820 ,, ,, 1829 (H)	1823·3	1829·9
1832 (F) 1834 (F) 1837 annually up to 1839 (H)	1833·9	1837·2
1844 annually up to 1848 (H)	1843·5	1848·1
1855 ,, ,, 1862 (H)	1856·0	1860·1
1866 (F) 1868 (W)	1867·2	1870·6
1874 annually up to 1878 (W)	1877(?)	—

[1] See *Gentleman's Magazine* for July, 1748, pp. 331 and 414.

The remarkable fact displayed in his portion of the above table, that locusts only make their appearance in great numbers in the interval from minimum to maximum sun-spot and never during the interval from maximum to minimum sun-spot, is evidently regarded by Dr. Hahn as affording some additional collateral proof in favour of the relation he had already partially established between the rainfalls of Central Europe and the sun-spots, viz., that the interval from minimum to maximum sun-spot is in general drier and warmer than that from maximum to minimum sun-spot. The added dates apparently exhibit a relation to the sun-spot epochs similar to those given by Dr. Hahn.

Whether the laws which regulate the visitations of locusts are the same in other parts of the world or not, it is at all events suggestive to notice that the dates of their general appearance throughout the world, given by Mr. Walford in a recent paper to the Statistical Society, entitled, "Famines of the World, Past and Present," exhibit the same relation to the sun-spots as that noted by Dr. Hahn in the case of their visits to Europe.

The date of apparition, the locality visited, and the epoch of sun-spot minimum corresponding, are given below.

Date of Apparition.	Locality visited.	Epoch of Minimum Sun-Spot.
1802	India	1798·3
1812–13	,,	1810·6
1833–35	,,	1833·9
1855	United States	1856·0
1868	,,	1867·2
1874–77	,,	1877(?)
1878	China, Spain, Algeria, Bosnia, India	1877(?)

In the face of such an apparent predilection on the part of locusts to swarm during the minimum epoch of sun-spots, it might, I think, be advantageous to institute an extensive comparison of all past visitations of these insects with the eleven-year cycle of sun-spots. This after due allowance had been made for any known natural cycles of incubation, might possibly bring to light a physical cycle of visitation, the size and position of the area affected by which would, perhaps, afford some indication of the corresponding limits of the rainfall variation.

E. D. ARCHIBALD

11, Granby Terrace, Glasgow

The Range of the Mammoth

THE criticism by Mr. Clement Reid of my paper recently read before the Geological Society, and not yet published, renders it necessary to remind him that his views as to the mammoth not being pre-glacial are not new, but were advanced by me ten years ago (*Pop. Science Review*, 1868, p. 275; *Geol. Mag.*, v. 7, July, 1868), and afterwards given up by the light of a wider experience. His arrangement of the complicated glacial deposits of Britain in one linear series, like all similar attempts, appears to me to be based on the fallacy that a difference in the character of the strata, in different areas, implies a difference in point of time. His divisions seem to me purely local, and mostly peculiar to the eastern counties. I see no reason for believing that, while five out of the six proposed were being accumulated east of the Pennine chain, there were no glacial phenomena in the west, in Lancashire or Cheshire, until the last phase, or the sixth of the eastern divisions; or, in other words, that the lower boulder clay of Lancashire and Cheshire is the equivalent of the last division. On the contrary, the marine sands and gravels covering the lower boulder clays, on both sides of the Pennine chain, and occupying a large area round Crewe, Stafford, and in the Trent Valley, is to me a well-marked horizon, defining the upper from the lower series of boulder clays. It is very likely that the lower boulder clay of Lancashire and Cheshire is the equivalent of the lower boulder clay series of Norfolk and Suffolk, as well as of the "moraine profonde" of Scotland. It must, however, be admitted that the correlation of the glacial deposits in various parts of Britain has not yet been satisfactorily made out. "Tot homines quot sententiæ."

To group them, as they are frequently now grouped, in one linear series, seems to me almost as useless as to construct a history of Europe in which the dynasties ruling various kingdoms at the same time are described one after another, and taken to belong to different periods because they were different dynasties.

W. BOYD DAWKINS

Fossil Floras of the Arctic Regions

THE author of the valuable paper communicated in NATURE, vol xix. p. 124, on "The Fossil Floras of the Arctic Regions," will find in an essay of mine, entitled "Thalassa," and published in the course of last year, a number of facts and arguments in corroboration of his views regarding the influence of oceanic currents upon climate. In this essay, which is mainly founded upon the observations made on board H.M.S. *Challenger*, I have endeavoured to show how a relatively trifling elevation or depression of the sea-coast or of the bed of the ocean may considerably alter the configuration of an oceanic basin; how such an alteration must affect the direction, volume and temperature of existing currents, and thus have a tendency to change the climate and ultimately the distribution of animal and plant life in the regions bordering on the oceanic basin. I also (p. 29–30) ventured to express the opinion that "it appears hardly necessary to go in search of vast cosmic changes, such as an alteration in the position of the terrestrial axis, a diminution in the amount of solar heat . . . . while we have, close at hand, an agency whose effect upon climatic conditions may be said to be a matter of daily experience, and which is sufficiently powerful to establish, in almost any region on the earth's surface, the small difference of temperature which is a decree of life or of death to numerous animal and vegetable organisms." To this I may now add, that the influence of oceanic currents upon climate and upon the distribution of life will be most felt and produce the most remarkable results in the *Arctic* and *sub-Arctic* regions, which, as we know, may at one period be swept by powerful polar currents, at another invaded by vast masses of warm water brought there by currents from the tropical regions, such, for example, as the Gulf Stream.

The facts brought to light by recent sounding-operations regarding the configuration of the sea-bottom and the distribution of oceanic depths has led me to the conclusion that our continents and oceanic basins, such as they appear at the present day, are of immense antiquity; that changes in the distribution of land and water require for their accomplishment long periods, which, for all we know, may alternate with periods of repose or even of retrocession; and that, consequently, the influence of these changes upon currents and upon climate must be equally slow and uncertain in its duration. If, therefore, the effect of oceanic currents upon climate appears as an important factor, which can no longer be neglected in any discussion on the flora and fauna of past geological epochs, on the other hand, the slow and uncertain progress of the changes above mentioned seems to afford ample scope for the operation of other causes which, besides climate and temperature, determine the existence of certain species in a given area of the earth's surface.

J. J. WILD

The Microphone

TWO subjects of interest in connection with the practical application of the microphone have lately been brought to my notice by Raja Sir T. Madava Row, K.C.S.I., Dewan of Baroda. In the hope of securing a little assistance from some of your scientific readers I hasten to lay them before you.

The first question is with reference to the use of a microphone as a stethoscope. It seems that native ladies of high position decline altogether to allow a doctor to examine the chest in the ordinary manner. Sooner than submit to such an examination they would prefer to die—certainly rather a staggering fact for those imbued with European ideas. In the cause of humanity it is therefore desirable to do something for those whose position and caste would be imperilled by *direct* examination. If the microphone could be so delicately arranged as to transmit the auscultatory sounds, a medical ear, even at a distance, would surely be able to detect the existence of any disease of the heart or lungs. In the few experiments that we have made with our limited appliances we have been able to hear the ticking of a watch at a distance of about 200 yards, and the roar of a black ant when attacked by his companion, but as yet we have heard no internal sounds from the human breast. Perhaps with better devised instruments some one may have been able to obtain that which has yet been denied to us. I am sure many native ladies would be glad to get an affirmative answer to the question, "Can the microphone be used as a stethoscope?"

The second subject seems to me to be a much more difficult one to grapple with. Sir Madava Row writes to me as follows:—

"In the undulating region of Travancore, where the water-bearing strata heave and fall according to the locality concerned, I have come across a set of professional men who are generally consulted by those who wish to sink wells in view to ascertain whether, at a given spot, a well may be sunk with the probability of finding water *near enough*. These professional men undertake to predict where the springs will be found near, and where they will be found at great depth, and their predictions are generally verified with great accuracy. I took some trouble to ascertain how these men are enabled to predict the proximity, or otherwise, of the springs underground. Brushing aside the ceremonies and incantations they perform in view to deceive others and perhaps themselves also, I found that they detect the proximity of the subterranean springs by lying down on the bare ground in the dead silence of night, with the ear in contact with the ground, and trying to *hear the sound* of the flow of water in the strata beneath. By practice the ear is made very sensitive, and the degree of distinctness with which they hear the sound of flowing water enables them approximately to predict the depth of the springs. It is in this manner that appropriate spots are selected for sinking wells.

"Now, would any of the instruments you are experimenting with magnify the sound of the subterranean flow of water so as to greatly facilitate the process I have described? If so, it may be a considerable practical gain."

To this query I have hitherto been able to return no other answer than a negative one. Both the subjects are practical ones, and I only hope that there will be before long some light cast upon them. THOS. S. TAIT

Baroda, India, November 4

Leaf-Sheaths and the Growth of Plants

THE latest results of M. Bert's researches into the growth of plants (see *Comptes Rendus*, vol. 87, p. 695, November 4), have led me to publish an observation which I made on the inner sheaths of young leaves this last spring. The spring before last I was struck with the crimson-like colour of these silky sheaths on many trees, whereas the young leaves they cover are of a tender green, and it naturally occurred to me that their purpose was not only to form a wrapper to the leaf, but also a coloured screen, which would allow the red rays of the spectrum to pass, and to a certain extent quench the blue rays. But I could not understand why the latter rays should be cut off, since they are highly actinic, and the leaves themselves are green. Last spring I carefully noted the tints of the leaf-sheaths of different trees, with the following results:—

Name of Tree.	Tint of Inner Leaf Sheath.	Tint of Outer Leaf Sheath.
Elm	Red (crimson)	Reddish-brown.
Lime	Red (crimson)	Reddish-brown.
Beech	Red (crimson)	Brown.
Sycamore	Red (crimson).	
Ash		Sooty-black.
Horse-chestnut	Red (crimson).	
Maple	Bright-red.	
Birch	Brown.	
Oak	Brownish-red.	
The Bramble	Reddish.	
The Hawthorn	Red.	
Certain Roses	Red.	
Wild Cherry	Red.	

Sometimes the bark on the stem of young sycamore shoots and the top leaflets were tinged with a deep reddish-brown. The overlapping tips of young leaves in buds were frequently reddish, and the majority of outer leaf-cases were a warm or reddish-brown. The shining buds of the horse-chestnut afforded a fine example of the phenomenon. The leaf-cases were of a strong red, like carmine, the exposed tip of each sheath leaflet being dyed a deeper red at the middle, shading off to the edges, where they overlapped. Under this protective curtain was a layer of flossy fibre, like cotton, swathing the pale young leaves underneath.

The recent researches of M. Bert throw light on this interesting subject. He finds that plants kept under green glass shades soon perish, because these intercept the red or less refrangible rays of the spectrum, and allow the blue or more refrangible rays to pass. Red glass, on the contrary, sustains life, although it becomes enfeebled by reason of the withdrawal of all the blue rays. M. Bert thinks that all the rays are necessary to the full vigour of the plant, and in the proportion of the solar spectrum; but it would appear from the care which nature has taken to redden the young leaf-cases, that for certain trees at least, the spring sunlight is richer in blue rays than she wishes, and therefore she has arranged that part of them shall be excluded, while all the red rays (or those which affect the maximum reduction of carbonic acid gas, and the building up of tissues) are allowed to pass into the leaf. I should add that this effect of colour is evidently superadded to the other functions of the sheath, as it usually appears as a more or less perfect colouring of the *outer surface* of the sheath only. JOHN MUNRO

West Croydon

Hornets

THE following fact, which I have been enabled to verify concerning a hornet, may be interesting to some of the readers of NATURE:—In a letter I received from my grandson, a very observing and accurate young naturalist, there was the following statement, dated October 13, 1878, Tunbridge Wells:—

"Last week I caught a splendid large hornet on the hall window, and last Saturday I caught a smaller one on a small oak-tree in Hurst Wood (Tunbridge Wells). He was engaged in eating some sticky, whitish stuff which had come out of the tree in several places where it had been cut or bruised. This stuff seemed to attract all the insects in the neighbourhood, especially swarms of flies. There were two red admirals (butterflies), two hornets, four wasps, and hundreds of bluebottles and other small flies, busily engaged in eating this substance, which was bored with small round holes. On Sunday, as we came home from Speldhurst Church, we passed the same tree, and on it, to my great surprise, I caught half a hornet, which was very active, running about the tree, and seemed to be quite happy and comfortable. He had no abdomen at all, except a small piece of the upper skin, which hung on; his left wings were very much battered, and he had lost his left hind leg." This half hornet was brought home and examined, keeping actively alive until the evening, when it was destroyed for the sake of preservation. I had an opportunity of examining the half hornet alluded to very soon after, and the facts detailed in the letter I have quoted were ascertained to be quite correct. This hornet was a small specimen, and I found all the abdomen gone except a small portion of the upper part of the first joint, which still remained attached to the thorax. The left wings were much broken, and one hind leg gone. In this disabled state, how long the hornet might have lived is a point which I regret was not ascertained.

Raystead, Worthing WM. WILSON SAUNDERS

Equine Sagacity

A PLEASANT story has just come to us from the Cape of Good Hope. In Graaf-Reinett, as in all the old Dutch towns in the colony, there is, in the centre of the place, a large market square, where the farmers, traders, and others, arriving with their produce at any hour of the day or night, may "out-span" the oxen or horses from their waggons, send the cattle out to the "commonage" to feed, while they bivouac at their waggons, as is the wont of African travellers to do, until the eight o'clock morning market auction.

An old horse belonging to one of these parties had wandered about in search of grass and water—vainly, no doubt, for it was during the severe drought from which the country is but now recovering. Coming to the great bare market-place, and finding a knot of men talking there, he singled out one of them, and pulled him by the sleeve with his teeth. The man, thinking the horse might possibly bite, repulsed him, but as it was not very roughly done, he returned to the charge, with the same reception; but he was a persevering animal, and practically demonstrated the axiom that "perseverance gains the day," for upon his taking the chosen sleeve for the third time between his teeth, the owner awoke to the idea that a deed of kindness might be required of him; so, putting his hand upon the horse's neck, he said, "All right, old fellow; march on!" The horse at once led the way to a pump at the further side of the square. Some coloured servants were lounging about the spot. One of them, at the bidding of the white man, filled a bucket with water; three times was the bucket replenished and emptied before the "great thirst" was assuaged, and then the grateful brute almost spoke his thanks to his white friend by rubbing his

nose gently against his arm, after which he walked off with a great sigh of relief.

A story somewhat analogous to the foregoing was told me by a friend, whose uncle, an old country squire in one of our western counties, had a favourite hunter in a loose box in the stable. One warm summer day he was "athirst," and could get no water. He tried to draw the groom's attention to the fact, but without success. The horse was not to be discouraged; he evidently gave the matter consideration. The thirst was pressing. All at once he remembered that he always had a certain halter put upon his head when led to the water. He knew where it hung. He managed to unhook it from its peg, and *carried it to the groom!* who at once, in great admiration of the knowledgable brute, rewarded him in the manner he desired. M. CAREY-HOBSON

Colour-Blindness

DR. PRIOR'S letter is almost entirely philological, and therefore does not come within my province. I have alluded to the colour-blind impression of white in my paper in the *Phil. Trans.*

I should like to know more about the eyesight of the person who says he cannot distinguish snow.

The latter part of the first paragraph of my letter on p. 120 should run, "In pigments, neutral green appears to me gray."

December 14 W. POLE

Magnetic Storm, May 14, 1878

REFERRING to a letter from the Rev. S. J. Perry in NATURE, vol. xviii. p. 617, reporting the magnetic disturbances observed at Stonyhurst, Melbourne, and Shanghai, on May 14, it may interest your readers to learn that earth-current disturbances were also noted on the Persian Gulf cables from 4 P.M. (Kurrachee time) on the 15th up to 5 A.M. on the following day.

Unusually strong earth-currents were also observed on June 3 and 4, on the cables between Bushire and Kurrachee; the current-strength at 2.40 P.M. on the 3rd, and 12.20 A.M. on the 4th, being reported as equal to fourteen Daniell's cells.

Kurrachee, November 8 HENRY C. MANCE

"Measuring the Height of Clouds"

THE electric light promising to be of great intensity at a small cost, the thought occurred to me that it might be used with advantage for the purpose of ascertaining the height of clouds. For, supposing an electric lamp sending a beam of light to the clouds, the spot where the light meets the latter, will be more or less visible, and we are obviously able to determine trigonometrically the height of the cloud.

By using two lamps, or a lamp and two reflectors, we may easily find also the rate at which clouds travel, by bringing the plane, passing through the axes of the beams of light, parallel to the direction in which the clouds move, and by noting the time it takes a cloud to travel from one beam of light to the other, having, of course, determined also the actual distance between the two spots of light on the clouds.

The above refers to observations during the night only, but by making use of coloured light, or by bringing a substance in the carbons of the lamp, the spectrum of which is easily recognisable, we might probably be able to work also during day-time.

Kew J. F. WILKE

The Weather

AFTER a week of unusually cold weather, the mean temperature having been 28°·5, and the wind constant from a northerly point, a thaw set in yesterday, and the wind became westerly, when immediately after sunset a rather unusual condition of weather occurred: viz., the rapid formation of a complete sheet of ice on the roads, though at the time, and till eleven P.M., the thermometer was 2° or 3° above the freezing-point.

As the sky was overcast at the time radiation cannot well account for it. Owing to the penetration of the cold, the surface must have retained a temperature considerably below 32° for some time after the air had become warmer and damper, so that the moisture was at once congealed.

Clifton, December 16 G. S. THOMSON

THE LAST EXPERIMENTS WITH THE 80-TON GUN

THE last experiments with the 80-ton gun at Woolwich deserve to be recorded, if only for the sake of showing that our scientific artillerists appear to be working in the proper channel. The last shot fired from the monster piece of ordnance was with the unprecedented charge of 460 lbs. of powder, and yet there was not so much strain upon the gun as that formerly exerted by charges one hundred lbs. less. The reason of this is in the main due to a change having been made in the character of the gunpowder employed; for whenever the former powder was used, even in lesser quantity, the pressure of the gas inside the gun rose at once. This would not so much matter if it could be shown that with the increase of strain, the work of the shot increased also. But such is not the case. For instance, in the case of two shots fired last week, one was sent on its way by 460 lbs. of prismatic powder, recording a velocity, we are told, of 1,626 per second, and a strain inside the gun of 19½ tons, while the other, with but 425 lbs. of cube powder, had a speed of only 1,600 feet, while it exerted a strain upon the weapon of 21 tons per square inch. The gun has been chambered—or in other words the cartridge cavity enlarged—to permit the introduction of heavier charges, as also to allow of a certain amount of air-space in the cartridge; but this modification in the weapon, beneficial as it may be, does not account, as we have shown, for the decrease upon the strain of the gun. This is due to the change in the powder.

In most of the former experiments a gunpowder of solid cubes, irregular in shape and measuring about an inch and a half, were employed; the recent results have been secured by thick six-sided prisms, about an inch across, and so accurately shaped that they may be packed together very closely. There is a single perforation in the middle of this prismatic powder, which, by the way, is of German origin, and when the cartridge has been securely packed so as to represent one solid mass, the perforations running through the whole length of the charge permit of the same being rapidly kindled. If the perforations were not there, half the charge would probably be expelled the gun before it was kindled; so that a packed cartridge of prismatic powder represents as nearly as possible a solid charge with tubes running its entire length, through which the kindling flames pass.

It has, of late, grown to be an axiom that the larger the gun the larger must be the grains of powder. A large grain of gunpowder burns slow because the fire is some time reaching the centre, and a slow-burning powder is what artillerists require for rifled guns. In a smoothbore weapon the cannon ball fits loosely, and may be expelled at a bound; but in rifled cannon the shot, so to speak, moves upon a sort of railway, and it would never do to get the shot into motion too suddenly. An undue strain would be exerted upon the gun, while the velocity of the shot would not be increased. For a rifled gun, therefore, a slow-burning charge is absolutely necessary, and this is to be secured only by reducing the surface to be kindled. In the case of the prismatic powder, the grains, if they may be called by that name, are so closely packed that no fire can get between them, and hence the action of kindling is still further reduced.

Not only is the shape and density of powder grains now attracting particular attention, but the percentage of moisture contained in the material has also lately been under study. The amount of water in gunpowder to the minute extent existing in ordinary samples is found to influence combustion in a very marked degree, and nothing but an exhaustive series of trials can give sufficient data for practical application of so important an element in the science of explosives. In the meantime chemists are pointing out yet another source of uncertainty in the combustion of gunpowder, to which, notwithstanding

their repeated warnings, but little attention has hitherto been given. We mean the composition of the charcoal. According to the manner of preparing this, the method adopted for charring and the material employed, so does the chemical composition of the charcoal differ. Some samples, for instance, prove on analysis to contain 85 per cent. of carbon, while others have 20 per cent. less; it is scarcely to be expected that gunpowder made from the two kinds will have the same burning qualities, and yet with gunpowder manufactures charcoal is charcoal, no matter how much its component parts of carbon, hydrogen, oxygen, and ash may differ. It is of little use, therefore, paying any particular attention to the physical qualities of gunpowder so long as its chemical composition is almost entirely ignored.

The manner in which the strain upon the gun and the velocity of the shot are measured at Woolwich are worthy of explanation. The means employed are of the simplest kind. The maximum pressure of the gases inside the gun as the shot is being expelled is recorded by what is termed a "crusher gauge." This is no more than a tiny pillar of copper. The pillar is placed loosely in a tube, the end of which, made of steel, stands firm and fast no matter what the pressure. So that the soft copper pillar, when subjected to the action of the gas, gets compressed, or crushed, and assumes something of a barrel shape. The pillar and its case, being affixed to the base of the shot, gets the full pressure of the gunpowder gases, and its length afterwards denotes how much this pressure has been. To secure more trustworthy pillars of the metal it is the practice to compress them first of all to a certain degree, to remove any honeycomb or imperfection, and, thus uniformly compressed, they may be relied upon to record the strain with accuracy. Comparison of the fired pillar, with other pillars which have been subjected to known pressures, at once reveals the degree of force to which the former has been subjected in the gun. The maximum pressure, or strain, to which the 80-ton gun should be subjected, is set down as 25 tons on the square inch, and it is with the aid of this "crusher-gauge" that the strain exerted in the various experiments has been ascertained.

The initial velocity of a shot, or, in other words, the rapidity with which a projectile flies at the outset of its career, is now measured by an electrical instrument, the invention of Major le Boulengé, a Belgian officer. As in the case of other instruments of a like nature, the shot is made to break through two wire screens, placed at some distance from one another. The interval is usually about 100 feet. The screen is simply a wooden framework with fine wires zigzagging across, and it is these fine wires which the shot cuts. One screen is near the muzzle of the gun, and the other at the distance we have mentioned. No. 1 screen is in connection with an electro-magnet in the instrument-house, and No. 2 screen with a second, the two magnets hanging close together. While the wires in front of the screen are perfect, an electric current passes without interruption, and the electro-magnets in connection with them are endowed with power, but this power ceases as soon as the shot cuts the wires of the screen. Before the gun is fired there is suspended to the magnets two rods of iron, which remain, however, only so long as the magnets are magnets. When the shot is fired, No. 1 screen is torn, and down falls the rod suspended to No. 1 magnet; an instant afterwards, when the shot has reached No. 2 screen, No. 2 magnet also loses it virtue, and down falls the second rod. The time between the falling of the two rods is so small, that ere the first has fallen half its length the second has dropped upon a trigger, which trigger darts out and strikes the side of No. 1 rod. When the latter is picked up, the first thing is to examine the surface for the mark of the trigger, for the position of this mark, whether high or low, tells the operator what he wants to know. The rod being of a given weight, always takes the same time to fall, and according whether it has fallen half or quarter its length, so the time taken by the shot to travel between the screens has been long or short. In a word, the rod has only to be compared with a prepared scale in order to read off the number of feet per second at which the shot has gone on its way.

THE REGISTRARSHIP OF LONDON UNIVERSITY

LAST week we referred to Dr. Carpenter's intended resignation of the Registrarship of the University of London. We have before us his letter intimating his desire to resign his post on May 31 next, and the resolution of the Senate in connection therewith. By the date mentioned Dr. Carpenter will have completed his twenty-third year as Registrar, and, including his previous nine years as Examiner, his connection with the University has extended over four-fifths of its term of existence, and over a corresponding proportion of his own professional life. There is no doubt that the success of this great institution is to a great extent owing to the energy and faithfulness with which Dr. Carpenter has discharged the duties of his post. It has been fortunate for the University as well as for science that a man of so eminent a scientific position has been so long and so intimately connected with it, and it will be extremely difficult to find one capable of taking up adequately Dr. Carpenter's work. We have pleasure in publishing the resolution of the Senate, to which we have referred.

"In accepting the Registrar's resignation of the important office he has held since 1856, the Senate desire to record their sense not only of the ability, judgment, and fidelity with which he has uniformly discharged its duties, but also of the zeal and efficiency with which he has on all occasions exerted himself both within and beyond the limits of his official obligation, for the promotion of the best interests of the University.

"The Senate would further record their conviction that it has been of special advantage to the University, during the twenty years of its most rapid development, to have had the services of a Registrar who, besides being an excellent administrator of its affairs, has attained, by his scientific labours, a position which has given him a just weight and influence over those with whom he has been brought officially into contact.

"The Senate strongly recommend the Registrar to the favourable consideration of the Lords of Her Majesty's Treasury as having acquired, by 'special services,' a claim to a larger superannuation allowance than that to which he is entitled by mere length of service."

ABOUT FISHES' HEADS

IN a former number (vol. xvii., p. 286), in a note "About Fishes' Tails," we called attention to some recent observations of Alexander Agassiz on the young stages of some fishes, in which he showed the wonderful changes that, as development went on, took place in their caudal fins; yet strange though these changes are, they seem as nothing to those that take place in some fishes' heads, and the facts first noticed by Steenstrup, and the theory which, by a marvellous power of intuition, he built up thereon, as to the eye in a flounder passing from the right side of its head to its left, have been in a great measure confirmed, and perhaps in a greater measure added to, by the painstaking observations quite recently published, of Alexander Agassiz,[1] from which it would now seem very certain that even the most shapeless adult fishes begin their life as quite symmetrical young creatures. No more

[1] *Proceedings* of the American Academy of Arts and Sciences, vol. xiv., July, 1878.

unsymmetrical fish can probably be found than an adult flounder with its unsymmetrical tail, with its twisted head, with its two goggly eyes—brought together on the one side of that head—and yet examine a flounder while yet young. "The one I captured," writes Agassiz, "was so transparent as to rival the most watery of jelly fishes. When placed on a flat glass dish it could only be distinguished by allowing the light to strike it in certain directions, otherwise all that was visible were the two apparently disembodied bright emerald eyes moving more or less actively. It was over an inch in length, the position of the eyes was perfectly symmetrical, and they were placed at a considerable distance from the anterior extremity of the snout; the dorsal fin extended almost to the nostrils." From this beautiful symmetry how then did the strange want of it in the adult fish arise? Long ago (1863) this question presented itself to Steenstrup. He had a small number of very young flounders preserved in alcohol, and from an examination of these he answered the query thus:—The young flounder, after a short time, takes to lying on its right side, why no one can tell, but with this result that the eye of that side begins to turn inwards, and passing through the tissues of the head, transfers itself to the left side. So strange seemed this explanation, that Malm's observations, in which he seemed to show that this apparent transference was really due to a torsion or twisting of the entire head, appeared to some to be, perhaps, the most probable explanation of the extraordinary phenomenon described by Steenstrup, and yet in Steenstrup's paper he very clearly showed that any ordinary torsion of the head of a flounder on its axis was wholly insufficient to explain the final position of its eyes. Since 1863 a good deal has been written upon the subject of the want of symmetry in the heads of the so-called flat fishes, more especially by Sir Wyville Thomson, Dr. Ramsay Traquair, Dr. Schiodte, Dr. Klein, Professors Reichert and Canestrini; but the most important and the latest memoir is the one just published by A. Agassiz, which forms a second part of his memoir, "On the Young Stages of Osseous Fishes," and is devoted chiefly to the development of the flounders. This memoir is accompanied by eight excellent plates, some of which show very well the changes of form through which some of the young flounders pass. The young flounders of some species attain a considerable size ere they show the least tendency to favour one side more than another, and before there is any change in the position of the eyes. They then swim vertically, at least when they come up to the surface to feed. This they will do on bright sunny days, about ten o'clock in the morning, while the water is very smooth, and they will then be seen to devour greedily swarms of embryo crustaceans of all orders. Some will after a while settle down on their left sides, which then in time become colourless and blind, these would be called dextral, while in some just the reverse takes place; but no matter on which side they take to resting on, the exchange is the same. First there is a slight advance of the eye of the blind side towards the snout, then this rises higher and higher towards the medial line of the head; it now becomes more and more visible from the coloured side, until at last it quite passes over. This transfer commences, in eight species observed by Agassiz, very early in life, while all the face-bones of the skull are quite cartilaginous, and, by a combined process of rotation and translation, it is completed long before these have become ossified. So far these observations of A. Agassiz were completely in conformity with the observations of Malm, who, it will be remembered, did not trace the changes undergone during the process; and they seemed to be completely antagonistic to the idea of Steenstrup, that the eye from the blind side passed through the tissues of the head and came out on the coloured side. But in the late summer of 1875 a little shoal of some fifteen quite transparent flounders were captured by Agassiz, on a quiet and brilliant morning, on the surface of the water at the mouth of the harbour of Newport. They were swimming vertically, and violently rushing after the minute entomostraca which swarmed on the surface. They were at once transferred to shallow glass jars, in which they would remain at the bottom on their right sides, for hours immovable. When disturbed they were rapid in their movements, frequently jumping out of the water. When swimming vertically they usually moved obliquely, the tail being carried lower than the head. When one of these was looked at in profile, its right eye could be seen through the head, slightly in advance and a little above the left eye; owing to the great transparency of the body, the right eye was then nearly as useful as if placed on the left side. Gradually it rose, until in about six days it was well above the left eye; shortly after, wonderful to relate, it was seen to sink into the tissues at the base of the dorsal fin between this and the frontal; slowly it sank until the huge orbit became reduced to a mere circular opening. Little by little this became smaller and smaller, the eye pushed its way deeper into the tissues, until an additional opening was formed on the left side. At this stage there were three orbital openings, though of course but two eyes. The original or right-orbital opening soon became closed and the coloured side had its two eyes. Thus was the suggestion of Steenstrup proved to be correct by careful observation on a living form, and what is of even greater interest, A. Agassiz is, from having thus, as it were, seen all round the subject, enabled to suggest that the difference between these two methods of the transference of these eyes is not so great as would at first appear, the eye that sinks through the tissues, only taking a slightly shorter cut to arrive at its destination than the one that travels round the frontal bone. He is also able to hint at facts and suggest thoughts thereon, that seem to us to be as full of interest as of novelty. Only a few of these can we allude to, such as the great length of the optic nerve, which allows slack to be taken during the transfer of the eye, and yet does not cause the sight to be interfered with, and the direct and very active circulation taking place to and from the heart and the orbital cavity, constituting almost an ocular heart.

The causes usually assigned for the development of fishes with a binocular side are all more or less unsatisfactory. It is known that in experiments thereon similar conditions constantly fail to produce similar results. Of the causes assigned the chief are: that the great width of the body in flounders makes the resting on the one side the most natural position; but there are many fishes of far greater width which swim vertically. The absence of a swimming bladder has also been assigned as a good reason, but some flounders have a swim bladder. Alexander Agassiz hints that the true cause may perhaps be that some broad fish may find it much easier to pursue their prey while swimming close to the bottom. They are protected from detection by their coloured side resembling sand, mud, and gravel. This would gradually lead to the exclusive use of one side (should the fish lie on either side) and would result in the atrophy of the eye, unless the fish were able to transfer his eye to the other side and so retain it. But then it will be asked, why do we not find flat fish among the broad forms of every family of fishes? and, remembering that flounders are only found in the most recent geological deposits, why were they not as common in earlier times as at the present day? and, above all, why was the tendency of the eye to change transmitted from generation to generation and not the binocular state itself?

May not, suggests Agassiz, Giards' idea come to our help here. Giards hints that the fundamental cause of all asymmetry in the animal kingdom is due to a difference in the strength of the organs of sense, and he gives in support of this idea some most ingeniously explained

cases. At any rate, the action of light upon the sense organs, which in all embryos are developed out of all proportion to their ultimate conditions, must remain an all-important element in its effect upon the nervous system. In embryos so transparent as those of many young fishes are, which might be said to be nothing but eyes, brain, and notochord, the action of light must be infinitely more potent upon their nervous system than it can possibly be in older stages, when the muscular system has assumed the control.

The pigment cells appear early in the egg. In some fishes, immediately before the little fish is hatched, two colour elements are to be found, black and yellow; but in the majority of cases the black alone is present, the yellow element appearing subsequently, and, last of all, the red. Pouchet's experiments seem to show that the blue pigments are only a dimorphic condition of the red pigments. This, by the way, would account for why a lobster turns red when cooked. The proper mixture of the three colours—black, red, yellow—enables the flounders to imitate most admirably the general effect of their feeding-grounds; so much so that often it requires a most practised eye to detect them. The rapidity with which they can change their colour is also quite striking. Agassiz frequently removed a jar containing a young flounder, which he figures, from a surface imitating a sandy bottom to one of a dark chocolate colour, and in less than ten minutes the black pigments would obtain a preponderance.

The question of the form and development of the pigment cells is also discussed in the memoir. As to the causes of colour in the animal kingdom we would seem to be only on the threshold of an interesting and novel field of inquiry, and it would seem, says Agassiz, very hazardous to infer from a physiological point of view, as has been frequently done on philological grounds, that Homer's colour descriptions indicate a gradual development of the sense of colour in the early races of mankind.

E. PERCEVAL WRIGHT

Since writing the above we have received from Prof. Japetus Steenstrup "Fortsatte Bidrag til en rigtig Opfattelse af Oiestillingen hos Flyndrene," with four plates. This supplemental memoir is in Swedish, and gives a *résumé* of what has been written on the subject since the paper in which the illustrious author first called attention to it, with criticisms thereon. An advance sheet of Agassiz's paper also enabled him to quote the chief details of his observations. The memoir also contains a description with beautiful figures of a Plagusia form, which was captured while its eye was just about to traverse the head obliquely and to take its place on the other side as the upper eye. It also gives a series of figures which make clear the connection that exists between certain frequently met with monstrous forms of flat-fish and the normal forms. One of these thus illustrated is the "malformed brill" figured in Yarrel's "British Fishes."

THE BROWN INSTITUTION

IT is now just seven years since the Brown Institution was opened, under the auspices of the Senate of the University of London, as a place for the study of the diseases of animals. It was at that time placed by the Senate under the direction of a committee comprising the most eminent members of the medical profession, with Dr. Sharpey as their chairman. Dr. Burdon-Sanderson was appointed superintendent, with Dr. Klein—who had then recently migrated from Vienna to London—as his coadjutor. A hospital had been built for the reception of diseased anmals, and placed under the care of a highly qualified veterinarian, Mr. Duguid, and in connection with it a good and sufficient laboratory had been erected for the purpose of carrying out pathological and therapeutical experiments. No provision could be made from the funds of the Institution for the expenses of such investigations, it having been found necessary to devote the whole available income to the purely charitable purposes which the founder had associated with the investigation of disease in his testamentary statement of the objects he had in view. Pecuniary aid for research was, however, not wanting. The work done in the laboratory was, during the first three or four years, for the most part conducted at the instance of Mr. Simon, who was at that time at the head of the Medical Department of the Privy Council, and it was thus provided for by annual grants of public money. For a time all went on favourably, and it seemed possible that the Brown Institution would eventually fulfil the functions and acquire the importance of those State-supported establishments for research which have recently accomplished so much for the advancement of medical science in Germany. But, alas! clouds soon began to gather. That strange, popular agitation which culminated in the passing of the "Vivisection Act" showed itself to be specially hostile to those systematic experimental investigations which, at the present moment, are absolutely necessary for the elucidation of fundamental questions in pathology. Accordingly, the Brown Institution became a prominent object of attack. When the Act was passed it became apparent that the realisation of the hopes which had been entertained was no longer probable, for it was soon found that, in their bearing on pathological inquiries, the restrictions imposed really amounted to prohibitions.

These circumstances affected the working of the institution in such a way as seriously to diminish its prospect of usefulness. Early in the present year Dr. Burdon-Sanderson, baffled in his plans, resigned his appointment. His resignation has been followed by that of Mr. Duguid, who has accepted a more lucrative position under Government; and finally Dr. Klein, who became a candidate for the vacant superintendentship, and was supported by the unanimous recommendation of the Committee, but was rejected by the Senate of the University, who thus showed that the possession of an academical title confers none of the academical spirit. At the present moment, therefore, the Brown Institution is represented only by the buildings and the endowment. The men who have done its work, and whose names have been hitherto identified with it, have retired. The prospect is discouraging, but not quite so bad as it seems.

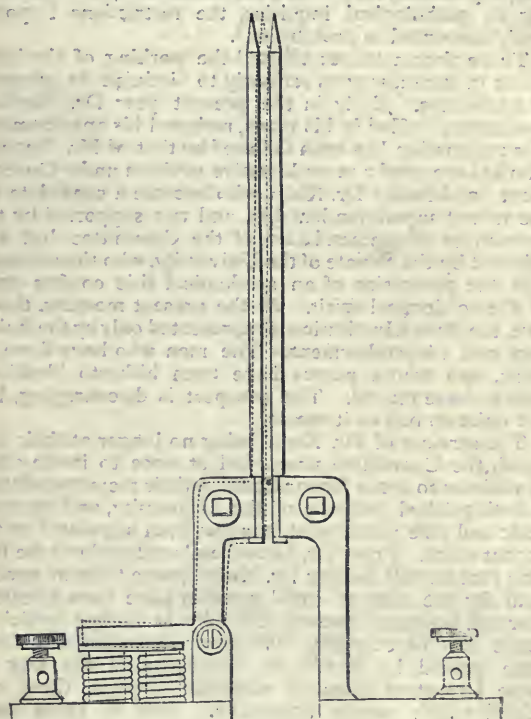
The services of Dr. Klein being no longer at their disposal, the Committee proceeded at once to invite other candidates to come forward, and on their recommendation a distinguished graduate of the University, and an energetic and able pathologist, has just been appointed to the vacant office. From Dr. Greenfield's antecedents we feel sure that he will (failing Dr. Klein) prove to be as good a man for the post as could possibly have been selected. Nor will he experience any difficulty in finding sufficient scope for his energies. Whatever obstacles may have been placed by ill-advised legislation in the way of some important lines of scientific inquiry, there are others which remain accessible. One of these lines was opened by Dr. Burdon-Sanderson three years ago. In the beginning of 1876 a grant of 500*l*. was made by the Royal Agricultural Society for the carrying out of scientific investigations at the Brown Institution, as to the nature and origin of some of the destructive contagious diseases of animals which prevail in this country. The results of these inquiries have already been, in part, printed, and others are in course of publication. In consequence of the resignation of Dr. Sanderson and of his veterinary coadjutor Mr. Duguid, the progress of his work has been temporarily arrested. But it is gratifying to be able to state that at the Annual Meeting of the Royal Agricultural Society which took place on

the 12th inst.; an additional sum of 250*l.* was voted for their prosecution, and that they will be actively resumed as soon as Dr. Greenfield has completed his arrangements.

The interval of inactivity has been used by the committee for carrying out important improvements of the premises in Wandsworth Road, so that Dr. Greenfield will enter on his new duties with many advantages in his favour—an excellent laboratory, sufficient resources, fruitful work already in progress, and a committee including such men as Busk, Gull, Paget, Quain, Sharpey, and Simon to back him. We feel confident that the wisdom of the appointment will be justified by the result, and that the new chapter in the history of the Brown Institution which will begin with the year 1879, will be a successful one.

ON SOME IMPROVED METHODS OF PRODUCING AND REGULATING ELECTRIC LIGHT[1]

IN a former communication to the Society I directed attention to the fact that when the electric light is produced from the ends of two carbon pencils placed parallel to each other, if the strength of the electric current, the thickness of the carbons, and the distance between them are rightly proportioned, the carbons will burn steadily downwards until they are wholly consumed, without any insulating material between them. To initiate the light by this method, it is necessary to complete



the electric circuit between the carbons by means of some conducting substance, which volatilises on the passage of the current, and establishes the electric arc between the points.

When a number of such lights are produced simultaneously from the same source of electricity, any interruption in the continuity of the current extinguishes all the lights in the same circuit, and each pair of carbons requires to be reprimed before the lights can again be established. This defect, as will be obvious, would cause great inconvenience when the lights are not easily accessible, or are at considerable distances apart.

In the course of my experiments it was observed that when the electric circuit was completed at the bottom of a pair of carbons close to the holders, the arc immediately ascended to the points, where it remained so long as the current was transmitted. My first impression of this peculiar action of the arc was, that it was due to the ascending current of hot air by which it was surrounded. This, however, was found not to be the cause, as the arc travelled towards the points in whatever position the carbons were placed, whether horizontally or vertically in an inverted position. Moreover, when a pair of carbons were held in the middle by the holders, the arc travelled upwards or downwards towards the points, according as the circuit was established above or below the holders. The action was, in fact, recognised to be the same as that which determines the propagation of an electric current through two rectilinear and parallel conductors submerged in contact with the terrestrial bed, which was described by me in the *Philosophical Magazine*, August, 1868.

In all the arrangements in general use for regulating the electric light, the carbon pencils are placed in the same straight line, and end to end. When the light is required, the ends are brought into momentary contact, and are then separated a short distance to enable the arc to form between them. The peculiar behaviour of the electric arc when the carbons are placed parallel to each other, suggested to me the means of lighting the carbons automatically, notwithstanding the fact that they could only be made to approach each other by a motion laterally, and to come into contact at their adjacent sides. To accomplish this object, one of the carbon holders is articulated or hinged to a small base plate of cast iron, which is so constructed as to become an electro-magnet when coiled with a few turns of insulated wire. The carbon holder is made in the form of a right-angled lever, to the short horizontal limb of which is fixed an armature placed over the poles of the electro-magnet. When the movable and fixed carbon holders are brought into juxtaposition, and the carbons inserted in them, the upper parts of the two carbons are always in contact when no current is transmitted through them, as shown by the dotted lines in the engraving.

The contact between the carbons is maintained by means of an antagonistic spring inserted in a recess in one of the poles of the electro-magnet, and reacting on the under side of the armature. One extremity of the coil of the electro-magnet is in metallic connection with the base of the carbon holder, while the other extremity of the coil is in connection with the terminal screw at the base of the instrument from which it is insulated. The coils of the electro-magnet are thus placed in the same circuit as the carbon pencils.

When the alternating current from an electro-magnetic induction machine is transmitted through the carbons, the electro-magnet attracts the armature and separates the upper ends of the carbons, which brings them into their normal position, and the light is immediately produced. When the circuit is interrupted, the armature is released; the upper ends of the carbons come into contact, and the light is produced as before. When several pairs of carbons are placed in the same circuit, they are, by this arrangement, lighted simultaneously.

H. WILDE

[1] Supplement to Paper read at the Manchester Literary and Philosophical Society, November 26 (see NATURE, vol. xix. p. 78). Communicated by the Author.

INFLUENCE OF THE STRAITS OF DOVER ON THE TIDES OF THE BRITISH CHANNEL AND THE NORTH SEA[1]

THE conclusions are:—

1. The rise and fall of the water-surface and the tidal streams throughout the North Sea north of the

[1] Abstract of a paper read at the Dublin meeting of the British Association.

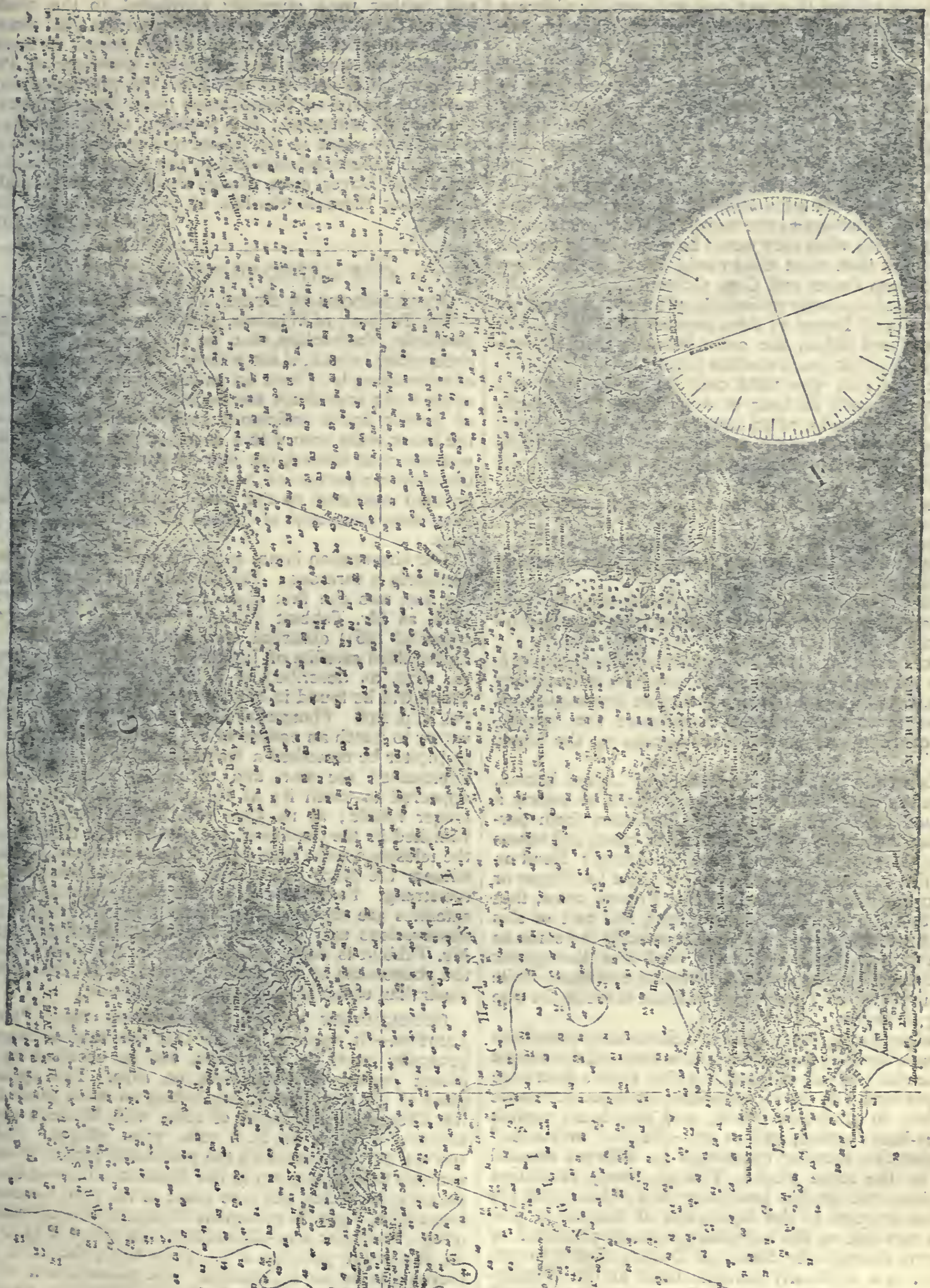


CHART OF THE ENGLISH CHANNEL.

parallel of 53° (through Cromer, in Norfolk) and the north coast of Holland and Hanover, are not sensibly different from what they would be if the passage through the Straits of Dover were stopped by a barrier.

2. The main features of the tides (rise and fall and tidal streams) throughout the British Channel west of Beachy Head and St. Valery-en-Caux do not differ much from what they would be if the passage through the Straits were stopped by a barrier between Dover and Cape Grisnez (Calais).

3. A partial effect of the actual current through the Straits is to make the tides throughout the Channel west of a line from Hastings to the mouth of the Somme more nearly agree with what they would be were there a barrier along this line than what they would be if there were a barrier between Dover and Cape Grisnez.

4. The chief obviously noticeable effect of the openness of the Straits of Dover on tides west of Beachy Head is that the rise and fall on the coast between Christchurch and Portland is not much smaller than it is.

5. The fact that the tidal currents to the westward commence generally an hour or two before Dover high-water and to the eastward an hour or two before Dover low-water instead of exactly at the times of Dover high- and low-water, is also partially due to the openness of the Straits of Dover.

6. The facts referred to in Nos. 4 and 5 are wholly due to three causes :—

(1) The openness of the Straits of Dover.

(2) Fluid friction (in eddies along the bottom and in tide-races).

(3) Want of absolute simultaneity in the time of high-water across the mouth of the Channel from Land's End to Ushant.

It is certain that (1) is very sensibly influential; it is probable that (2) is also so; it is possible, but scarcely probable, that (3) is so. Without farther investigation it would be in vain to attempt to estimate the proportionate contributions of the three causes to the whole effect.

7. It is certain that were the Straits of Dover barred, and were the water frictionless, there would be nearly a perfect *nodal line* [with but a small deviation from perfect nodality because of the influence of cause (3)] across the Channel from somewhere near St. Alban's Head on the English coast to somewhere near Cape La Hague or Cherbourg or Cape Barfleur, on the French coast, that west of this line the time of low-water, and east of this line the time of high-water, would be exactly the same as the time of high-water at Dover; and that throughout the Channel the water would be flowing eastwards while the tide is rising at Dover, and westwards while the tide is falling at Dover.

8. (Understanding from Fourier's elementary principles of harmonic analysis that all deviations from regular simple harmonic rise and fall of the tide within twelve hours are to be represented by the superposition of simple harmonic oscillations in six-hours period, and four-hours period, and three-hours period, and so on—like the "overtones" which give the peculiar characters to different musical sounds of the same pitch.) The six-hourly oscillation which gives the double low-water at Portland and the protracted duration of the high-water at Havre[1] is probably in part due to the complex-harmonic character of the current through the Straits of Dover; that is to say, definitely, to a six-hourly periodic term in the Fourier-series representing the quantity of water passing through the Straits per unit of time, at any instant of the twelve hours.

The double high-water experienced at Southampton, and in the Solent, and at Christchurch and Poole, and still further west, generally attributed to the doubleness of the influence experienced from the tidal streams on the two sides of the Isle of Wight, seems to have a continuity of cause with the double low-water at Portland, which is certainly allied to the protracted high-water of Havre—a phenomenon quite beyond reach of the Solent's influence. It is probable, therefore, that the double high-water in the Solent and at Christchurch and Poole is influenced sensibly by the current through the Straits of Dover, even though the common explanation attributing them to the Isle of Wight be in the main correct.

WILLIAM THOMSON

[1] At Havre, on the French coast, the high-water remains stationary for one hour, with a rise and fall of three or four inches for another hour, and only rises and falls thirteen inches for the space of three hours; this long period of nearly slack water is very valuable to the traffic of the port, and allows from fifteen to sixteen vessels to enter or leave the docks on the same tide.

OUR ASTRONOMICAL COLUMN

OCCULTATIONS OF STARS BY JUPITER'S SATELLITES.—Mr. Tebbutt, of Windsor, N.S.W., writes to the *Astronomische Nachrichten*, that on October 5 he made "an observation, which, if not without a parallel in the annals of astronomy, is at least an extremely rare one." A star of the ninth magnitude was occulted by the first satellite of Jupiter, under sufficiently good definition to allow of the latter being seen with a round disk: the occultation was not quite central, the star appearing to pass behind the northern portion of the disk. From the approximate position assigned to the star by Mr. Tebbutt, it must have been No. 20236 of Oeltzen's Argelander, called 9·10 mag.

The observation is not quite without a parallel, though doubtless a rare one; Flaugergues of Viviers (who, by the way, was the first discoverer of the great comet of 1811, as Mr. Tebbutt was also discoverer of the grand comet of 1861) observed an occultation of a small star by the third satellite of Jupiter on the morning of August 14, 1821, as described in a letter to Baron de Zach, which will be found in his *Correspondance Astronomique*, vol. v. p. 456. Flaugergues had proceeded to his observatory to watch an eclipse of the satellite, and on looking at Jupiter he remarked a small star near it; the satellite approached the star, and at 1h. 47m. sidereal time, appeared to touch it; at 1h. 56m. 52s. the star was no longer visible; at 1h. 59m. 10s. the satellite in its turn vanished in the shadow of the planet. He continued at the telescope some time after its disappearance, hoping to witness the star's emergence, but twilight soon became too strong. Perhaps now that the phenomena of Jupiter's satellites are more closely watched than formerly, such observations may become somewhat less exceptional; Mr. Tebbutt is doing good service in the observation of the phenomena of the Jovian system, as is also another Australian observer, Mr. Todd, at Adelaide.

OCCULTATION OF 64 AQUARII BY THE PLANET JUPITER.—It appears certain that the star 64 Aquarii, generally rated $6\frac{1}{2}$ magnitude, will be occulted by the planet Jupiter on September 14, 1879. The apparent place of the star for that day, taking its mean place from the Greenwich catalogue of 1864, with Mädler's proper motion, will be in R.A. 22h. 32m. 58·45s., N.P.D. 100° 39′ 0″·6, whence, with the position of Jupiter from Leverrier's tables, as given in the *Nautical Almanac*, the apparent conjunction will take place at 1h. 53m. Greenwich mean time, when the geocentric difference of declination is 9″·8. The polar semi-diameter of the planet is 23″·0 and its horizontal parallax 2″·2. It is clear, therefore, that there must be an occultation. The phenomenon will be most favourably witnessed at the Australian observatories; at Melbourne, for instance, the planet will be only a quarter of an hour from the meridian and 27° from the zenith.

THE CONJUNCTION OF MARS AND SATURN, JUNE 30, 1879.—The *Nautical Almanac* notifies a conjunction of these planets on June 30, 1879, at 8h. G.M.T., with Mars only 1′ to the north of Saturn. It is not without interest

to examine this near approach more closely, particularly as Bouvard's Tables of Saturn were used for 1879. According to Leverrier's Tables, the position of Saturn from Bouvard requires corrections of about + 1·79s. in Right Ascension, and − 0″·4 in Declination; whence, with Leverrier's place of Mars the conjunction in right ascension occurs at 8h. 3·7m. G.M.T., and at this time the geocentric difference of declination is 1′ 29″·0. The apparent semi-diameter of Mars (taking 9″·45 for the diameter at mean distance) is 4″·46, and the apparent polar semi-diameter of Saturn, 7″·83; the horizontal parallaxes, 8″·36 and 0″·93 respectively. Hence it is evident that there will be no approach to an occultation. At conjunction the planets will be below the horizon in this country, but will be near the meridian at our Australian observatories; there, however, the least distance between the south limb of Mars and the north limb of Saturn will, according to the Tables, exceed a minute of arc. Mr. Marth has pointed out that the last close conjunction of Saturn and Mars took place on April 18, 1817; the *Berliner Jahrbuch* for that year gives the time of conjunction at 7h. M.T. at Berlin, with Mars 1′ S. of Saturn.

An occultation of Saturn by Mars, so far as we know, has not yet been put upon record, nor suspected before the invention of the telescope. The earliest mention of a near approach of the two planets is found in the Chinese annals during the latter days of the 10th moon, A.D. 27; on this occasion Mars, Jupiter, and Saturn were all situate within about 2° from the bright star Regulus; and the same annals record that on July 23, A.D. 143, Mars was very near to Saturn.

BIOLOGICAL NOTES

NATURAL SELECTION AMONG LARVAL SALAMANDERS.—Every case illustrating survival of the fittest has its own interest, as well as its bearing on general laws. The New England salamanders lay large numbers of eggs attached to water plants, and the larvæ are very interesting to watch. In a group that was studied recently, cannibal tendencies soon developed, the stronger eating off the gills of the weaker, at the same time being able to protect their own, within a week or ten days after hatching; these cannibals were fifty per cent. larger than their brethren, and, soon waxing bolder, they began to swallow them bodily. After ten days of the results of such feeding, they were ten or twelve times the size of such weaker brethren as were still left alive. Thus they rapidly developed and passed out of the gill-bearing stage. See Mr. S. F. Clarke, in *American Naturalist* for September.

THE MUSCLES OF THE MAMMALIAN FOOT.—Dr. D. J. Cunningham (*Journal of Anatomy and Physiology*, October, 1878), after dissecting the manus and pes of a large number of mammals, finds that the typical arrangement of the intrinsic muscles of the foot is the same as in the hand, and that this is best seen in certain marsupials. In these animals the muscles are disposed in three layers (1) a plantar layer of adductors; (2) an intermediate layer of short flexors; and (3) a dorsal layer of abductors. Deviations from the type may take place by suppression or by fusion of certain elements of the different layers. Fusion of the members of the intermediate and dorsal layers is very common. The presence of an opponens muscle is not accounted for in the foregoing disposition. When present Dr. Cunningham regards it as derived most commonly from the short flexor, but in many of the carnivora it proceeds from the plantar layer. Further, it is found that in many animals the relation of the intrinsic muscles to the metatarsal bones, both as regards their origin and position, corresponds with transitory conditions in the foot of the human embryo. The adult dog agrees exactly with the first stage of the human fœtus in the relation of the intrinsic pedal muscles to the metatarsals; the bones are closely compressed together, and the muscles are entirely plantar in position.

SENSITIVE ORGANS IN ASCLEPIADACEÆ.—Robert Brown gave it as his opinion, based on experience, that fertilisation in this family of plants depends largely upon insect agency. Dr. J. G. Hunt has recently published observations on *Stapelia asterias*, whose flower has an extremely disagreeable and animal odour, which appears to attract many flies. Under observation flies were seen eagerly applying their tongues all over the petals and essential organs, apparently eating, with an almost intoxicating relish, the excretion covering those parts. This banquet was indulged in with safety until their tongues came in contact with one of five black spots situated near and alternate with the stamens, when, with amazing quickness, the fly was seized and firmly held by the tongue—a hopeless prisoner. Now a struggle commenced, and if the fly was small and not vigorous, he was retained; if large and strong he escaped, dragging away the black spot and also the pollen-masses, two of which are attached to each trap. The adhesion of the fly's tongue is not caused by any viscid liquid, but by a capital pair of blades, which, when touched lightly by a fly, or even a hair, close instantaneously, and secure the object. Two species of Asclepias have been examined by Mr. Edward Potts, and in these he finds that each anther has a pair of sacks or cases in which the pollen masses are suspended so as to make their withdrawal easy. They are closely adherent to the stigma. The sensitive glands are placed in shallow depressions upon the perpendicular columnar ridges of the stigma. The fact of the removal of the pollen masses by insect agency is well known; the question to determine was whether the glands had anything to do with the removal. Mr. Potts caught house-flies and held them by their wings above the flowers, allowing their feet to scramble over them. Almost immediately one or more of these would become ornamented with groups of the glands and pollen-masses, which clung so closely that their later struggles and rubbings failed to detach them. When separate hairs were directed on to a gland, the latter instantly contracted and clung to the hair, tearing itself loose from the stigma, and carrying away the pollen masses with it. On one of the species of Asclepias Mr. Potts noticed three flowers which, in addition to its own complete anthers, had one other sensitive gland and its attached pollen-masses, inserted under the edge of a normal anther, and against the sloping lower surface of the stigma. The development of these adventitious pollen-masses was traced till they put forth a profusion of pollen-tubes into the stigma, and the ovaries began to increase in size. Dr. Asa Gray mentions self-fertilisation as occurring in this genus by a similar growth of bundles of pollen-tubes penetrating the stigma at its lower extremity. But here in the presence of the foreign pollen-masses none of the home-grown ones had put forth pollen-tubes. It is conjectured that the maturity of the pollen-masses is reached so late that the stigma of the same flower is frequently unsusceptible. But if the pollen-masses from earlier flowers are removed by insects and lodged upon another just opened, they develop pollen-tubes, and cross-fertilisation ensues. Thus the sensitive glands are not for capture of insects, but to favour cross-fertilisation. (*Proceedings*, Acad. Nat. Sci. Philadelphia, 1878).

THE INHALATION OF PHOSPHURETTED HYDROGEN.—Dr. T. B. Henderson, of Glasgow (*Journal of Anatomy and Physiology*, October, 1878), has investigated the physiological effects of the inhalation of phosphuretted hydrogen gas, by inclosing an animal in an air-tight chamber of known capacity, and subsequently introducing into this a given quantity of the gas. In the first experiment a strong rat was placed in an atmosphere consisting almost entirely of phosphuretted hydrogen, and death

occurred in about ten minutes. An atmosphere containing one per cent. of the gas was found to prove fatal within half an hour. In the case of a large female rabbit, 0·2 per cent. caused death in thirty-three minutes. In these cases the most marked symptom was that of great increase in the number of respirations. Before death, respiration became slow and laboured, and convulsions resembling those of opisthotonus occurred. The ventricles of the heart became most powerfully contracted. Where the strongest dose was administered, the effect on the heart was most marked, and the lungs appeared unaffected. When small quantities of the gas were used, within a very short time the animals began to show signs of suffering from intense irritation of the skin, scratching and biting at it incessantly. Afterwards the creatures seemed to become drowsy, and assumed a very peculiar attitude, sitting down on all-fours, the back bent upwards, and nose pushed backwards between the fore-paws, so as to bring the forehead against the floor of the cage; a rat in this position looked very much like a curled-up hedgehog. A fatal result occurred when the quantity of gas was so small as 1 to 5120. In no case could the odour of the gas be detected in any organ of the body after death. The gas did not appear to exert any local action on the skin.

STRUCTURE AND AFFINITIES OF CHARACEÆ.—This difficult problem has been the subject of recent discussion in the pages of Trimen's *Journal of Botany*. The first paper was in the July number, by Mr. A. W. Bennett, who gave his reasons for dissenting from some generally accepted views of the structure of *Chara*, and from its assignment by Sachs to a place among the Carposporeæ. He objects in the first place to the use of the term "pro-embryo" (Vorkeim) for the immediate product of the germination of the spore, the homologue of the protonema of a moss, and not of the pro-embryo nor suspensor of Selaginellaceæ and Phanerogams. The term sporangium is also frequently misapplied to the nucule, which is in reality an archegonium. The so-called "sporocarp" is formed before and not as the result of fecundation. Finally, Mr. Bennett maintains that Characeæ differ from all the other higher cryptogams in the absence of any alternation of generations, the nearest affinity being with Muscineæ, which they approach in their organs of reproduction. In the September number Prof. Caruel expresses his agreement with Mr. Bennett in removing the Characeæ from the Carposporeæ, but differs in his interpretation of the structure which is the immediate product of germination, the homology of which with the protonema of mosses he contests. He places them in a separate class of their own, intermediate between phanerogams and vascular cryptogams. Finally, in the number for December, Mr. S. H. Vines has a very elaborate essay on the subject. He agrees with both the previous writers in separating the Characeæ from the Carposporeæ, and with Caruel in disputing the homology of the "pro-embryo" with the protonema of a moss, but on the other hand again considers their nearest affinity, though remote, to be with Muscineæ. His principal object is to show that the "pro-embryo" is in reality the embryo of the plant, and that it constitutes in itself the non-sexual generation or sporophore, homologous with the sporogonium of mosses, notwithstanding the apparently anomalous fact that it never produces spores. For such a structure he proposes the term "aposporous sporophore," and compares it to the "apogamous" oophore or prothallium of *Pteris cretica* and some other ferns, which are anomalous in not producing sexual organs of reproduction.

GEOGRAPHICAL NOTES

THE fifty-sixth supplement to Petermann's *Mittheilungen* has just been published, and consists of a masterly treatise on Deltas, by Dr. G. A. Credner, of Halle. The author shows the importance of deltas in reference both to geography and geology, and discusses carefully the real import of the term. He then, in the first part of his work, treats of the Formation, Structure, Growth, and Distribution of Deltas under the heads of (1) Limit and Form of the Delta; (2) Formation and Condition of the Delta Surface; (3) Size of the Delta; (4) Its Power; (5) Its material; (6) Architecture; (7) Rate of its Growth; (8) Results of its Growth; (9) The Age of Deltas; (10) Number and Geographical Distribution of Deltas; (11) Classification of Deltas. The second part treats of the various causes of the origin of deltas, the causes and conditions of their formation, in which the author discusses various processes of great geological interest. Three sheets of maps accompany this most important paper, showing, among other points, the various deltas of the world.

WE are glad to learn of the early appearance of a work published in Russia under the editorship of M. Semenoff, President of the Geographical Society at St. Petersburg. The title is "Illustrated Russia," and it will give a geographical, historical, ethnographical, and statistical description of the country. We notice among the very numerous collaborators all the names well known in the Russian geographical world. The work will contain four folio volumes of sixty to seventy sheets each, and it will be accompanied with numerous illustrations, engraved by the best European firms. Another work of the same kind is undertaken by M. Mordovtseff—"The Ukraine (Little Russia): its History and its People." It will be on the same plan as the well-known work on "Bohemia: its History and its People."

WE are also glad to notice the appearance of the last volume of the "Works of the Ethnographical Expedition sent by the Russian Geographical Society." This volume deals with the south-western provinces of Russia. The expedition was undertaken in 1869, finished in two years, and the printing of the reports, which occupy seven large volumes, has taken since 1872.

WE find in the *Izvestia* of the Russian Geographical Society a notice of the journey of M. Mayeff in Southern Bokhara, last August. After having reached Karshi with an embassy sent to the Emir by the Governor-General of Tashkent, M. Mayeff visited the mountain pass, Ak-bash, which goes from Tenga-khoram to the Kerchak River, and to the great and wealthy village, Kuitan: thence he proceeded by the pass Tenga-daval to Shir-abad. The Kerchak River and its tributary, Kuitan-daria, both mighty mountain streams, were previously quite unknown. The Tenga-daval cleft cuts through the whole mass of the Kuityn-tau, the south-western part of Hissar ridge. From Shir-abad M. Mayeff, going further south, crossed the great Pashkhund ridge, reached the Surkhan river at Kakaity, and traced its banks down to Regar and Sary-djuy. Thence he returned to Shahri-sabs by a very bad route, hardly practicable even on horseback, along the rocky banks of the wild stream, Sengri-dagh. The surveys made during this journey are a most important acquisition for the geography of Central Asia; the highlands of Bokhara, quite unknown until now, will soon receive on our maps an outline in accordance with nature.

THE last number of the *Izvestia* of the Russian Geographical Society contains a report, by Capt. Sidensner, on the possibility of a water communication between the tributaries of the Obi and Yenissei; a very interesting paper, by M. Miclucho Maclay, on the Pelew archipelago, being a description of the people, its customs, administration, and religion; a necrology of M. Chaslavsky; and several notes:—On M. Mayeff's journey to Southern Bokhara, on the Russian cruises to the Obi and Yenissei, and especially statistical ones on printing in Moscow, on trade, ports, and telegraphs in Japan, and on the population and manufactures in governments Tula and Nijni-Novgorod.

DR. EDWIN R. HEATH, to whom we referred some time ago as intending to take up the work of South American exploration commenced by Prof. Orton, and interrupted by his untimely death, left New York on November 18, on the *William H. Keeney*, for Pará, expecting to proceed by steamer to San Antonio, to remain there during the winter, and from that point as a centre to carry on his further investigations. In these he expects to be greatly assisted by Messrs. T. and P. Collins, of Philadelphia, the contractors for the Mamore and Madera Railroad.

THE Woodruff half-educational, half-pleasure expedition round the world has been again organised on a new basis. A steamer of 3,000 tons has been purchased in Europe, and the inclusive fare is 2,500 dollars.

MR. S. E. PEAL communicates to the *Indian Tea Gazette* some remarks on the subject of an overland route to China, *viâ* the Assam Valley, which are of interest, as there is no attempt to under-rate the great difficulties to be encountered. He justly observes that the merits of the various passes out of Eastern Assam are not well understood, and that it is comparatively easy to draw a line on the map from some point in Upper Assam to the Yangtoze or Likiang-fu, and to say, "Why cannot this be done?" Wilcox and others since have sufficiently shown the difficult nature of the country, *viâ* the Tengapani and the Brahmaputra Valley; what lies beyond the point explored is reputed still worse, and as crossing snow. Northwards again, *viâ* the Dihong, or Dihong Gorges, the outlet is equally uninviting. The Upper Subansire, or Lopra-cha-chu, is not well explored, though routes not far from it are known to pass for three-quarters of a mile along the face of a precipice 1,500 feet above an abyss, and on a path of stone slabs resting on iron piers let into the face of the rock. The Towang route is, again, difficult, and too far west. South of the Tengapani we have Dapha Búm, 14,500 feet high, with a pass to the south, in which Wilcox's experiences prove how unsuitable it would be for a trade-route. South of Daphapani is the upper portion of the Buri Dihing, and whether there is a route up it crossing Eastern Patkai and going down the Sitka, is not known, though Singphús travel that way. It is one of the routes to be explored, and may give a pass at, perhaps, 5,000 feet elevation or less. The Namrup basin, however, would so far seem to be the only reliable and easy outlet from Eastern Assam: and here is a pass at an elevation of about 1,000 ft., leading *viâ* Hukong and the Shoemai direct to Western Yünnan, a route which is at present in use. By the Patkai route past Nongyang Lake to the Hukong Valley, Mr. Peal says, we cross at the highest an elevation of 1,500 to 2,000 ft., and fall at once into a beaten track to Yünnan. By this route alone also can the huge snow-clad ridges be turned that stand as barriers east and west of the Upper Irrawadi or Shoemai, and that stretch down from the north to about the parallel of 27°. In point of fact the east-south-east is the only direction in which it is possible to get out of Assam in the direction of China at less than 2,000 ft. above the sea-level.

THE COMPOUND NATURE OF THE ELEMENTS

WE have not yet received from the Royal Society the paper read last Thursday by Mr. Lockyer, in which he brought forward facts indicating the compound nature of the chemical elements. In the meantime the following article from yesterday's *Times* may be of interest; it is evidently written by a chemist who was present when the paper was read:—

"At a crowded meeting such as is seldom witnessed of the Royal Society, on Thursday evening last, Mr. J. Norman Lockyer, F.R.S., read a lengthy paper, in which he discussed the evidence derived from spectroscopic observation of the sun and stars and from laboratory experiments, which has led him to the conclusion that the so-called elements of the chemist are in reality compound bodies. In order that the line of argument followed by Mr. Lockyer may be understood, it will be necessary briefly to refer to the results of previous researches. As a rule, in observing spectra, the substance to be examined is volatilised in a gas flame or by means of sparks from an induction-coil, and the light is allowed to fall on the slit of the spectroscope; the spectrum is then generally one in which the lines run across the entire field, but by interposing a lens between the spark apparatus and the slit of the spectroscope, Mr. Lockyer was enabled to study the various regions of the heated vapour, and thus to establish the fact, already noted by some previous observers, but to which little attention had been paid, that all the lines in the spectrum of the substance volatilised did not extend to equal distances from the poles. He then showed by the aid of this method that in the case of alloys containing different proportions of two metals, if the one constituent were present in very small quantity, its spectrum was reduced to its simplest form, the line or lines longest in the spectrum of the pure substance alone appearing, but that on increasing the amount of this constituent its other lines gradually appeared in the order of their lengths in the spectrum of the pure substance. Similar observations were made with compound bodies. It was also noticed that the lines furnished by a particular substance varied not only in length and number, but also in brightness and thickness, according to the relative amount present. Armed with these facts, and with the object of ultimately ascertaining, more definitely than has hitherto been possible, which of the elements are present in the sun, Mr. Lockyer, about four years ago, commenced the preparation of a map of a particular region of the spectra of the metallic elements, for comparison with the map of the same region of the solar spectrum. For this purpose about 2,000 photographs of spectra of all the various metallic elements have been taken, and, in addition, more than 100,000 eye observations have been made. As it is almost impossible to obtain pure substances, the photographs have been carefully compared, in order to eliminate the lines due to impurities; the absence of a particular element as impurity being regarded as proved if its longest and strongest line was absent from the photograph of the element under examination. The result of all this labour, Mr. Lockyer states, is to show that the hypothesis that identical lines in different spectra are due to impurities is not sufficient, for he finds short line coincidences between the spectra of many metals in which the freedom from mutual impurity has been demonstrated by the absence of the longest lines. He then adds that, five years ago, he pointed out that there are many facts and many trains of thought suggested by solar and stellar physics which point to another hypothesis—namely, that the elements themselves, or, at all events, some of them, are compound bodies. Thus it would appear that the hotter a star the more simple is its spectrum; for the brightest, and therefore probably the hottest stars, such as Sirius, furnish spectra showing only very thick hydrogen lines and a few very thin metallic lines, characteristic of elements of low atomic weight, while the cooler stars, such as our sun, are shown by their spectra to contain a much larger number of metallic elements than stars such as Sirius, but no non-metallic elements; and the coolest stars furnish fluted band-spectra characteristic of compounds of metallic with non-metallic elements and of non-metallic elements. These facts appear to meet with a simple explanation if it be supposed that as the temperature increases the compounds are first broken up into their constituent "elements," and that these "elements" then undergo dissociation or decomposition into

"elements" of lower atomic weight. Mr. Lockyer next considers what will be the difference in the spectroscopic phenomena, supposing that A contains B as an impurity and as a constituent. In both cases A will have a spectrum of its own. B, however, if present as an impurity, will merely add its lines according to the amount present, as we have above explained; whereas if a constituent of A it will add its lines according to the extent to which A is decomposed and B is set at liberty. So that as the temperature increases the spectrum of A will fade if A be a compound body, whereas it will not fade if A be a true element. Moreover, if A be a compound body, the longest lines at one temperature will not be the longest at another. The paper chiefly deals with a discussion from this point of view of the spectrum of calcium, iron, hydrogen, and lithium, as observed at various temperatures; and it is shown that precisely the kind of change which is to be expected on the hypothesis of the non-elementary character of the elements has been found to take place. Thus each of the salts of calcium, so long as the temperature is below a certain point, has a definite spectrum of its own, but as the temperature is raised the spectrum of the salt gradually dies out and very fine lines due to the metal appear in the blue and violet portions of the spectrum. At the temperature of the electric arc the line in the blue is of great intensity, the violet H and K lines, as they are called, being still thin; in the sun the H and K lines are very thick, and the line in the blue is of less intensity than either, and much thinner than in the arc. Lastly, Dr. Huggins's magnificent star photographs show that both the H and K lines are present in the spectrum of *a* Aquilæ, the latter being, however, only about half the breadth of the former; but that in the spectrum of *a* Lyræ and Sirius only the H line of calcium is present. Similar evidence that these different lines may represent different substances appears to be afforded by Prof. Young's spectroscopic observations of solar storms, he having seen the H line injected into the chromosphere seventy-five times, the K line fifty times; but the blue line, which is the all-important line of calcium at the arc-temperature, was only injected thrice. In the spectrum of iron two sets of three lines occur in the region between H and G which are highly characteristic of this metal. On comparing photographs of the solar spectrum and of the spark taken between poles of iron, the relative intensity of these triplets is seen to be absolutely reversed; the lines barely visible in the spark photograph being among the most prominent in that of the solar spectrum, while the triplet, which is prominent in the spark photograph, is represented by lines not half so thick in the solar spectrum. Prof. Young has observed during solar storms two very faint lines in the iron spectrum near G injected thirty times into the chromosphere, while one of the lines of the triplet was only injected twice. These facts, Mr. Lockyer contends, at once meet with a simple explanation if it be admitted that the lines are produced by the vibration of several distinct molecules.

"The lithium spectrum exhibits a series of changes with a rise of temperature precisely analogous to those observed in the case of calcium.

"In discussing the hydrogen spectrum, Mr. Lockyer adduces a number of most important and interesting facts and speculations. It is pointed out that the most refrangible line of hydrogen in the solar spectrum, *h*, is only seen in laboratory experiments when a very high temperature is employed, and that it was absent from the solar protuberances during the eclipse of 1875, although the other lines of hydrogen were photographed. This line also is coincident with the strongest line of indium as already recorded by Thalén, and may be photographed by volatilising indium in the electric arc, whereas palladium charged with hydrogen furnishes a photograph in which none of the hydrogen lines are visible. By employing a very feeble spark at a very low pressure the F-line of hydrogen in the green is obtained without the blue and red lines which are seen when a stronger spark is used, so that alterations undoubtedly take place in the spectrum of hydrogen similar to those observed in the case of calcium. In concluding this portion of his paper Mr. Lockyer states that he has obtained evidence leading to the conclusion that the substance giving the non-reversed line in the chromosphere, which has been termed *helium*, and not previously identified with any known form of matter, and also the substance giving the 1,474 or coronal line, are really other forms of hydrogen, the one more simple than that which gives the *h*-line alone, the other more complex than that which gives the F-line alone.

"There can be no question that the facts brought forward by Mr. Lockyer are of the highest importance and value, and that they will have much influence on the further development of spectrum analysis, to which he has already so largely contributed. But his arguments are of a character so totally different from those ordinarily dealt with by chemists that they will hesitate for the present to regard them as proof of the decomposition of the elements until either they are assured by competent physicists that they cannot be explained by any other equally simple and probable hypothesis, or until what Mr. Lockyer has foreshadowed as taking place to such an extent in other worlds has been realised beyond question or cavil in our own laboratories. It has been suggested that the same molecule may be capable of vibrating in different ways at different temperatures, and thus of yielding different spectra, just as a bell may give out different notes when struck in different ways; and although Mr. Lockyer has replied to this objection, it can scarcely be regarded as finally disposed of. The fact, however, as Mr. Lockyer has pointed out, that the change from the spectrum of a compound to the lowest temperature spectrum of its metallic element is of a similar character to and even less in degree than the change from the lowest temperature spectrum of the metal to the spectra which it furnishes at higher temperatures does not appear to favour such an hypothesis, and from the similarity in the phenomena it is difficult to deny that in both cases decomposition does not equally take place. Prof. Young's observations on the injection of particular lines into the chromosphere during solar storms are also difficult to reconcile with this view, and if the conclusions drawn from previous researches are correct, it also does not account for the short line coincidences which led Mr. Lockyer to his hypothesis.

"Chemists are careful to teach that what are at present regarded as elements are not necessarily simple bodies, but merely substances which they are unable to decompose or which they have no special reason to regard as compound bodies. The remarkable relations, both in atomic weight and properties, existing between many of the elements, tend, indeed, to show that they are related in the manner Mr. Lockyer supposes. We sincerely hope that he will continue his researches in this direction, and we trust that at no very distant time he may be able to bring forward evidence sufficiently clear to convince even the most sceptical."

NOTES

MR. J. M. WILSON, Mathematical Master at Rugby School, has been elected Master of Clifton College, in place of Dr. Percival, elected President of Trinity College, Oxford. Mr. Wilson has done very much for science at Rugby, and, although Dr. Percival is a difficult man to follow, and has done more probably than any head master for the teaching of science in schools, still we hope that Mr. Wilson will prove a worthy successor to him.

We are indebted to the kindness of Prof. Hayden for the following observation made by his party on the day of the last eclipse of the sun:—"Washington, D. C., November 18, 1878. Observation on total eclipse of the sun. Camp No. 4, Little Sandy, Wyoming Terr., July 29, 1878:—

	m.	sec.
Time of total eclipse as taken by A. D. Wilson with telescope and theodolite	2	27·5
Mr. James Eccles, with smoked glass	2	26·0
W. H. Holmes, with smoked glass and telescope of theodolite	2	27·0

	°	′	″
Approximate latititude	42	8	25
,, longitude	109	9	52

The weather was very clear and quite calm during eclipse, but was preceded by a heavy wind."

At the recent meeting of the United States National Academy, we learn from *Science News* (the third number of which has reached us), Prof. Davidson incidentally announced that he hoped to be able to observe the intra-Mercurial planets without waiting for eclipses.

The *Japan Mail* states that an astronomical observatory is to be established within the precincts of the Geographical Bureau of Tokio. The same journal also announces that telegraphic insulators, made at the village of Imari, in the province of Hizen, are of such good quality that they find large sale in Europe.

A scheme for the extension of meteorological observations throughout Russia is now being discussed by the Russian government. The Central Meteorological Institution will be located at St. Petersburg. Provincial meteorological institutions will be created in connection with each Russian university, and these provincial institutions will have under their superintendence all meteorological stations of their respective districts, which stations will be largely increased in number.

We are informed that the Government of New South Wales has requested Mr. William Forster, Agent-General for the Colony, Prof. Liversidge, of the University of Sydney, and Mr. E. Combes, M.P., C.M.G., to collect information in the United Kingdom and on the Continent relative to the working of English and foreign technological museums and colleges, with a view to forming similar institutions in Sydney. A sum of money has been placed on the Estimates by the Government of the Colony, to enable the Committee to purchase suitable specimens. We have no doubt that the Agent-General for New South Wales (3, Westminster Chambers, S.W.) will be extremely glad to receive from such institutions, or from any other source, reports or any information which would assist the Committee in its inquiries.

A correspondent writes:—A singular project is on foot at Paris. M. Camille Flammarion, having published a number of articles to prove that the moon is not destitute of inhabitants, has been led to the idea of constructing a refracting telescope which will be powerful enough to see them. He is now busy organising a committee to collect the necessary funds.

An unexpected difficulty has stopped the construction of the works for the mounting of the great refractor at the Paris Observatory. This instrument is to be erected on the grounds which the Municipal Council had let to M. Leverrier for the nominal sum of 100 francs a year for a period of ninety-nine years. But there is a law that the Government funds cannot be spent for building purposes, except on grounds belonging to the Government; and the Municipal Council, who were so liberal as to the rent, want a high price for the ground.

M. Lockroy, the editor of the *Rappel*, and representative in the French Chamber of Deputies, has introduced a bill to dispose of the money intended to be spent on the rebuilding of the Tuileries Palace, for the completion of the isolation of the French national library.

General Myer has sent orders to Sergeant Jennings to leave in Europe the collections of the works published by the Signal Corps Central Office at Washington, exhibited at Paris. This valuable set has been deposited in the hands of M. Jarry, 46, Boulevard Magenta, Paris, one of the most active members of the Meteorological Society of France, who will be ready to give any information relating to them. It is expected that the series of publications of the United States Signal Corps will be exhibited next spring at the Palais de l'Industrie, in the scientific exhibition, and an improved weather indicator will be sent from Washington to be practically tested by predictions adapted to the peculiarities of the French climate.

We notice the appearance of a Russian work, by M. Starchevsky—"Guide for the Russian in Central Asia"—being a collection of vocabularies of the languages, viz., Turkish (Djagatay, or Uzbeck), with an indication of the variations afforded by the Kashgar, Khiva, and Turkoman idioms; Kirghiz; Tartarian (Kazan and Orenburg idioms); Sart, or the town Uzbeck; and Tadjik (Bukhara idioms). The vocabularies also contain sketches of the grammars of these languages, the words being given in their Russian transcription. A second volume, containing the Russian, Uzbeck, Kirghiz, Tartar, and Tadjik part, will appear shortly.

It is probably now only a question of time for the electric light to become an everyday institution in our large towns. For several nights satisfactory experiments have been made on Holborn Viaduct, and, under most unfavourable atmospheric conditions, part of the Thames Embankment was illuminated the other evening.

In Mr. A. S. Wilson's "Experiments on Turnip Seeds" (reprinted from the *Transactions* of the Botanical Society of Edinburgh), he follows out Darwin's idea that "heavy and fine seeds tend to yield the finest plants." Mr. Wilson's conclusions are altogether in harmony with those of Darwin. The mean of a large number of experiments gave a product of 2 lbs. 7 oz. per seed in the case of large seeds, as against 2 lbs. 1¼ oz. in the case of small seeds.

In his just published report on the trade of Chinkiang, on the Yangtsze-kiang, H.M.'s Consul mentions that there are rumours that the coal, iron, and plumbago mines in that neighbourhood are to be opened without delay, under the auspices of the Viceroy, Li Hungchang. A British engineer in the employ of the Chinese Government has recently visited these mines and reported on them to the Viceroy Shên, at Nanking.

The limit of permanent snow in the Caucasus is very variable, this mountain-group, of 156 geographical miles, lying between two seas and several steppe-regions, being consequently subject to the most opposite meteorological conditions. The average height of the snow-line on the Elburs, the highest point of the Caucasus, is 10,885 feet. The average height of the lower limits of the glaciers on the Elburs is 8,216 feet. The Kasbek is the centre of another region of glaciers and permanent snow-fields, in which the true situation of the snow-line is not yet accurately ascertained. A third region comprises the high ranges of the schist-system of Perikitel and Bogoz in Daghestan. The fourth region is the Schathdag, south of Daghestan, ten geographical miles from the Caspian Sea. In this last region the snow-limit reaches to 10,374 English feet above the sea-level. The average height of the snow-limit within the Caucasus is 10,600 feet. Local variations upwards and downwards are frequent, and there may be a difference of 3,200 feet between the maximum and the minimum. In the West Caucasian regions these conditions

resemble those of South Europe; in the Eastern region they offer an analogy with those of Asiatic mountain-groups, influenced by a continental climate. During the last few years some glaciers in the western half of the Caucasus have been retrograde, just as it has been observed with those of the Alps during these last fifteen years. In 1849 several Caucasian glaciers were evidently advancing through ancient forests. The glacier of the Kasbek, especially of Deftoraki, after having followed for some time the general retrograde movement, lately began to move forward again. Experience has proved, that, whenever this movement reaches a certain amount, the end of the glacier is broken away, and may cause serious catastrophes, as it did by stopping the chief military road from Tiflis along the Terek valley to Wladikawkas. Such observations are thus of high practical importance. The Deftoraki glacier may be paralleled with the Rosenthal and Vernagt glaciers of the North Tyrol, considering their variable periods and extreme alternations of progression and retrogression. For further details we refer to Dr. Abich's paper in *Proced. Imper. Geol. Instit. Vienna*, March 5, 1878.

REPORTS come to *Science News* of a remarkable and very extensive series of caves discovered in Page county, Virginia, which, it is said, a scientific expedition will probably soon examine. Their great area, variety, curious formation, and natural ornaments, if the stories about them be true, are sufficient to place them among the wonders of the world.

WE have received Decade I. of a "Prodromus of the Zoology of Victoria, or Figures and Descriptions of the Living Species of all Classes of the Victorian Indigenous Animals," by Prof. F. McCoy (London: Trübner and Co.). The plates in this first part are most beautifully coloured, and do infinite credit to the skill of the colony. There are three plates of snakes, three of fish, one of the giant earth-worm (*Megascolides australis*, McCoy), one devoted to three species of the day-moth, and the last two to two species of diurnal lepidoptera.

THE Commission for the Survey of New York State has been reappointed with an appropriation of 14,300 dollars a year. A large amount of preliminary work has been done under Mr. J. S. Gardner.

ACCORDING to a note published by the *Norddeutsche Allgemeine Zeitung*, Berlin time will become the only one in use in the whole of the German Empire. The difference in time is thirty minutes minus on the coast of the German Ocean, and thirty-seven in advance in the eastern parts of Bavaria. This resolution has been fostered by a similar reform lately established in Sweden.

M. BARDOUX has re-organised the French Central Society of Agriculture, which will be styled the National Society of Agriculture. It will be composed of 82 ordinary members, 10 foreign members, 150 corresponding members in France and Algeria, and 50 foreign corresponding members. The President of the Republic is to be *ex officio* the patron of this Society, and the Minister of Agriculture and Trade the honorary president.

AN earthquake was felt in Cologne and vicinity on December 10 at 11.35 A.M. A similar commotion was felt in the provinces of Luxemburg and Namur, principally on the borders of the Ardennes forest; on the same day at 11.28 Brussels time. The duration of the shock was eight seconds, and it was accompanied by a well-defined noise, which awoke the inhabitants. On the following morning a meteor was observed at six o'clock in Alsatia, from Mulhausen to Colmar. A fire-ball travelling from north-west to south was seen exploding, exhibiting a display of natural fire-works. No noise was heard by any observer.

AT the last meeting of the Manchester Anglers' Association, Mr. F. J. Faraday, F.L.S., in reading a paper on the "Mind of Fishes," recounted an instance of apparent intelligence in a skate, observed by the author while officiating as curator of the Manchester Aquarium. On the occasion in question a morsel of food thrown into the tank fell directly in the angle formed by the glass front and the bottom. The skate, a large specimen, made several attempts to seize the food, but owing to the position of the mouth on the under-surface of the head, and the closeness of the food to the glass, he was unable to do so. He lay quite still for a while "as though thinking;" then, suddenly raising himself in a slanting posture, the head inclined upwards and the under-surface of the body towards the food, the creature waved his broad expanse of fins and thereby created an upward current or wave in the water, which lifted the food from its position and carried it straight into his mouth.

AT the Royal Institution a Course of Six Lectures (adapted to a juvenile auditory) on a Soap Bubble, will be given by Prof. Dewar, M.A., F.R.S., at 3 o'clock, on December 28, 31, January 2, 4, 7, 9 (1879).

ON the New Jersey bank of the Delaware River, the skeleton of a man has recently been found buried in a standing position in a red sandy bluff overlooking the stream. A few inches below the surface the neck bones were found, and below these the rest of the skeleton except the bones of the hands and feet. The skull being wanting, it could not be determined whether the remains were those of an Indian or a white man; but the burial was peculiarly aboriginal. It was found that around the lower extremities were placed a number of large stones, showing traces of fire, together with charred wood; and there was no doubt that the bones of the feet had been burnt. Probably the man had been executed as a prisoner of war, being placed erect in the pit with a fire around his feet. He would appear to have been then buried, with the exception of his head. The skeleton when complete must have been six feet high.

THE new instalment of the *Transactions* of the Asiatic Society of Japan contains several papers of considerable interest. Mr. E. M. Satow, the Japanese Secretary of H.B.M.'s Legation at Tôkiô, who was one of the earliest labourers in the field of Japanese literature, contributes articles on the "Korean Potters in Satsuma," and the "Use of the Fire-Drill in Japan;" Mr. Aston one on "Hideyoshi's Invasion of Korea," and Mr. R. W. Atkinson "Notes on the Manufacture of *Oshiroi*" (white lead). There are also two contributions on earthquakes in Japan, and notes on some of the volcanic mountains of the empire.

PEAT FUEL is much used at Bremen and in other parts of North-western Germany, and increased attention has been paid of late years to its production and preparation. We learn from Consul Ward's Report that the vast tracts of marshy moors which are to be found in many parts of the German Empire, and more especially between the River Elbe and the Dutch frontier, are regarded as containing an immense amount of wealth in the form of peat fuel. With the view of developing and improving the present means of producing and manufacturing this article, and of extending its consumption to districts where fuel is dear, an association was formed at Königsberg a few years ago, and was reconstituted last year at Schwerin. Their intention is to diffuse technical knowledge throughout the country with regard to peat production and manufacture.

THE Annual Report of the Belfast Naturalists' Field Club for 1876–77, contains a variety of matter, some of it of considerable scientific interest. There is a brief account of the excursions in connection with the Club, embracing a good deal of topographical, antiquarian, and other information. At the winter session a variety of papers were read, some of which are reported at greater or less length. In connection with Mr.

William Gault's detailed "Observations on the Geology of the Black Mountains," a coloured diagram is given. The Appendix contains papers by Mr. Joseph Wright on "Recent Foraminifera of Down and Antrim," and by Messrs. Swanston and Lapworth on the "Correlation of the Silurian Rocks of Co. Down."

THE Twentieth Report of the East Kent Natural History Society is, on the whole, satisfactory. It contains abstracts of several good papers read at the meetings. The Society has ninety-three members.

EXCAVATIONS in the "Dragon Cave" at Mixniz, Styria, have been already noticed (NATURE, vol. xviii. p. 618). The diggings made in June, 1878, by the Anthropological Society of Gratz, have brought to light some bones bearing indistinct marks of cutting and percussion. Above the stalagmitic layer over the hearth-stuff some bones were found, in loam, well preserved, but probably derived from an older site. They are greenish, and partly of an intense bluish-green tint; and Prof. C. Doelter finds that their composition approaches that of turquoise [bone-turquoise?]. A full account by Prof. R. Hoernes will be found in the *Proc.* Imp. Geol. Instit. Vienna, August 31, 1878.

THE additions to the Zoological Society's Gardens during the past week include a Yellow Baboon (*Cynocephalus babouin*), from West Africa; two Ring-tailed Lemurs (*Lemur catta*), from Madagascar, presented by Mr. G. A. Shaw; a Green Monkey (*Cercopithecus callitrichus*), from West Africa, presented by Mr. J. Williams; a Common Fox (*Canis vulpes*), British, presented by Mr. Sntton Sharpe; a Woodcock (*Scolopax rusticola*), European, presented by Messrs. E. and W. H. Davis; a Common Swan (*Cygnus olor*), European, presented by Capt. Marx; a Ring-tailed Lemur (*Lemur catta*), from Madagascar, deposited; an Ocelot (*Felis pardalis*), from America; a Cereopsis Goose (*Cereopsis novæ-hollandiæ*), from Australia; three Yellow-winged Blue Creepers (*Cœreba cyanea*), from South America, purchased.

ON HELIOTROPISM IN PLANTS

THE heliotropic phenomena in plants form the subject of a monograph by Herr Wiesner, the first part of which has been recently communicated to the Vienna Academy. The following outline from the *Anzeiger* of the Academy will give an idea of some of the fruits of the author's researches on this important subject.

The first section treats of the history of the subject. In the second section the author studies the *influence of light* on heliotropism. The experiments were made in the light of a gas flame which burned under a constant pressure with a uniform intensity (luminous power = 6·5 spermaceti candles). The unit for the measurement of the light-intensity was the strength of this flame at the distance of one metre. It was found that in heliotropism three cardinal points of light-intensity are to be distinguished; an upper limit, a lower limit, and between the two an *optimum* of light intensity. Thus with decreasing intensity of light the strength of the heliotropic effect increases to a certain point, and beyond this point decreases. The lower limit referred to coincides with the lower limit of light-intensity for the stoppage of growth in length, while the upper limit does not coincide, or only occasionally coincides, with the upper limit of light-intensity for growth in length, for in th case of plants very sensitive heliotropically it lies higher, and in less sensitive plants lower, than the upper limit for growth in length. The mode of arrangement of the experiment in gas-light did not permit of determining in all cases the limiting values of the light-intensities; thus, for example, the upper limit for the heliotropism of etiolated shoots of *Salix alba*, and of the hypocotylous portion of the stem of *Viscum album*, and the lower limit for the heliotropism of the growing stem of vetch could not be ascertained. The former lies above 400, the latter far below 0·008. The optima were found to lie between 0·11 (the growing stem of the pea) and 6·25 (etiolated shoots of *Salix alba*). Both with gas-light and with natural light it was ascertained that beyond a certain intensity no growth in length occurs.

The third section treats of the relations between the *refrangibility* of the light rays, and the heliotropic effects. The experiments were made partly in the objective spectrum, partly in varieties of light, got by sending white light through coloured solutions. . . . It was proved that portions of plants very sensitive helictropically, *e.g.*, growing stems of *Vicia sativa*, undergo curvatures in all kinds of light, even in ultra-red and ultra-violet, with the exception of yellow. The maximum of the heliotropic force of light lies at the boundary between violet and ultra-violet; a second (smaller) in the ultra-red. From both maxima the power of the rays to produce heliotropism decreases gradually on to the yellow. Portions of plants little sensitive heliotropically, are no longer influenced by orange, or by red and green, or even (in the case of etiolated shoots of *Salix alba*) by ultra-red rays. The yellow rays quite stop the heliotropism, for, *e.g.*, in pure red a quicker and stronger heliotropism occurs than in a light which gives yellow besides red.

In the fourth section experiments are described on the joint action of (positive and negative) heliotropism and (positive and negative) geotropism. It is here shown, *inter alia*, that, in the case of plants very sensitive heliotropically, the geotropism is, at the optimum of light-intensity, apparently extinguished, even in strongly geotropic organs; further, that in many organs (growing stem of the pea), the heliotropic and geotropic powers of curvature disappear simultaneously; in others, however (stems of cress), the younger portions of the stem are more strongly heliotropic than the older, and the oldest after-growing portions of stem no longer show bendings in the light, but, through drawing action on one side (the heliotropic overhanging point of the stem), show apparently heliotropic curvatures chiefly due to growth, which are then counteracted by negative geotropism.

The arguments which go to prove that heliotropism is due to the phenomenon of unequal growth upon unequally-lit sides of an organ are set forth in the next section, and proof is offered that, for heliotropism as well as for growth in length, free oxygen is necessary.

The last chapter furnishes proof that the conditions for heliotropism remain constantly the same during its course, and coincide with the conditions for growth in length; further, that heliotropism (and the same holds good for geotropism) occurs as a phenomenon of induction. In this chapter it is also shown that when light induces heliotropism in an organ, a fresh heliotropic or geotropic induction meets with resistances, and can only come into action after extinction of action of the first; and that successive impulses of light and gravity, of which each by itself is capable of producing certain effects, do not have their actions added together when the effects that should be obtained separately are in the same direction, *e.g.*, one and the same side of the organ is helped in its growth in length.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 5.—"On a Machine for the Solution of Simultaneous Linear Equations," by Sir William Thomson. Let B_1, B_2, ... B_n be n bodies each supported on a fixed axis (in practice each is to be supported on knife-edges like the beam of a balance).

Let $P_{11}, P_{21}, P_{31}, \ldots P_n$ be n pulleys, each pivoted on B_1;
  $P_{12}, P_{22}, P_{32}, \ldots P_{n2}$,, ,, B_2;
  $P_{13}, P_{23}, P_{33}, \ldots P_{n3}$,, ,, B_3;
  ....
,, $C_1, C_2, C_3 \ldots C_n$, be n cords passing over the pulleys;
,, $D_1, P_{11}, P_{12}, P_{13}, \ldots P_{1n}, E_1$, be the course of C_1;
,, $D_2, P_{21}, P_{22}, P_{23}, \ldots P_{2n}, E_2$, ,, ,, C_2;
  ....
,, $D_1, E_1, D_2, E_2 \ldots D_n, E_n$, be fixed points;
,, $l_1, l, l_3, \ldots l_n$ be the lengths of the cords between D_1, E_1, and D_2, E_2, ... and D_n, and E_n, along the courses stated above, when B_1, B_2, ... B_n, are in particular positions which will be called their zero positions;

Let $l_1 + e_1, \ldots l_2 + e_2, \ldots l_n + e_n$ be their lengths between the same fixed points, when B_1, B_2, ... B_n are turned through angles $x_1, x_2, \ldots x_n$ from their zero positions;

(11), (12), (13), ... (1n),
(21), (22), (23), ... (2n),
(31), (32), (33), ... (3n),
....

quantities such that

$$\left.\begin{array}{l}(11)x_1 + (12)x_2 + \ldots + (1n)x_n = e_1\\ (21)x_1 + (22)x_2 + \ldots + (2n)x_n = e_2\\ (31)x_1 + (32)x_2 + \ldots + (3n)x_n = e_3\\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\\ (n1)x_1 + (n2)x_2 + \ldots + (nn)x_n = e_n\end{array}\right\}\ldots\ldots\ldots\ldots\ldots(\mathrm{I}).$$

We shall suppose $x_1, x_2, \ldots x_n$ to be each so small that (11), (12), ... (21), &c., do not vary sensibly from the values which they have where $x_1, x_2, \ldots x_n$, are each infinitely small. In practice it will be convenient to so place the axes of $B_1, B_2, \ldots B_n$, and the mountings of the pulleys on $B_1, B_2, \ldots B_n$, and the fixed points D_1, E_1, D_2, &c., that when $x_1, x_2, \ldots x_n$ are infinitely small, the straight parts of each cord and the lines of infinitesimal motion of the centres of the pulleys round which it passes are all parallel. Then $\frac{1}{2}$ (11), $\frac{1}{2}$ (21), ... $\frac{1}{2}$ (n1) will be simply equal to the distances of the centres of the pulleys $P_{11}, P_{21}, \ldots P_{n1}$, from the axis of B_1;

$\frac{1}{2}$ (12), $\frac{1}{2}$ (22) ... $\frac{1}{2}$ (n2) the distances of $P_{12}, P_{22}, \ldots P_{n2}$ from the axis of B, and so on.

In practice the mountings of the pulleys are to be adjustable by proper geometrical slides, to allow any prescribed positive or negative value to be given to each of the quantities (11), (12), ... (21), &c.

Suppose this to be done, and each of the bodies B_1, B_2, ... B_n to be placed in its zero position and held there. Attach now the cords firmly to the fixed points $D_1, D_2, \ldots D_n$ respectively; and, passing them round their proper pulleys, bring them to the other fixed points $E_1, E_2, \ldots E_n$, and pass them through infinitely small smooth rings fixed at these points. Now hold the bodies $B_1, B_2, \ldots$ each fixed, and (in practice by weights hung on their ends, outside $E_1, E_2, \ldots E_n$) pull the cords through $E_1, E_2, \ldots E_n$ with any given tensions[1] T_1, T_2, T_n. Let $G_1, G_2, \ldots G_n$ be moments round the fixed axes of $B_1, B_2, \ldots B_n$ of the forces required to hold the bodies fixed when acted on by the cords thus stretched. The principle of "virtual velocities," just as it came from Lagrange (or the principle of "work"), gives immediately, in virtue of (I),

$$\left.\begin{array}{l}G_1 = (11)\ T_1 + (21)\ T_2 + \ldots + (n1)\ T_n\\ G_2 = (12)\ T_1 + (22)\ T_2 + \ldots + (n2)\ T_n\\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\\ G_n = (1n)\ T_1 + (2n)\ T_2 + \ldots + (nn)\ T_n\end{array}\right\}\ldots\ldots\ldots(\mathrm{II}).$$

Apply and keep applied to each of the bodies, $B_1, B_2, \ldots B_n$ (in practice by the weights of the pulleys, and by counter-pulling springs), such forces as shall have for their moments the values $G_1, G_2, \ldots G_n$, calculated from equations (II) with whatever values seem desirable for the tensions $T_1, T_2, \ldots T_n$. (In practice, the straight parts of the cords are to be approximately vertical, and the bodies $B_1, B_2, \ldots, B_n$ are to be each balanced on its axis when the pulleys belonging to it are removed, and it is advisable to make the tensions each equal to half the weight of one of the pulleys with its adjustable frame.) The machine is now ready for use. To use it, pull the cords simultaneously or successively till lengths equal to $e_1, e_2, \ldots e_n$ are passed through the rings $E_1, E_2, \ldots E_n$, respectively.

The *pulls* required to do this may be positive or negative; in practice, they will be infinitesimal, downward or upward pressures applied by hand to the stretching weights which remain permanently hanging on the cords.

Observe the angles through which the bodies $B_1, B_2, \ldots B_n$ are turned by this given movement of the cords. These angles are the required values of the unknown $x_1, x_2, \ldots x_n$, satisfying the simultaneous equations (1).

The actual construction of a practically useful machine for calculating as many as eight or ten or more of unknowns from the same number of linear equations does not promise to be either difficult or over-elaborate. A fair approximation being found by a first application of the machine, a very moderate amount of straightforward arithmetical work (aided very advantageously by Crelle's multiplication tables) suffices to calculate the residual errors, and allow the machines (with the setting of the pulleys unchanged) to be re-applied to calculate the corrections (which may be treated decimally, for convenience): thus, 100 times the amount of the correction on each of the original unknowns, to be made the new unknowns, if the magnitudes thus falling to be dealt with are convenient for the machine. There is, of course, no limit to the accuracy thus obtainable by successive approximations. The exceeding easiness of each application of the machine promises well for its real usefulness, whether for cases in which a single application suffices, or for others in which the requisite accuracy is reached after two, three, or more of successive approximations.

[1] The idea of force here first introduced is not essential, indeed is not technically admissible to the purely kinematic and algebraic part of the subject proposed. But it is not merely an ideal kinematic construction of the algebraic problem that is intended; and the design of a kinematic machine, for success in practice, essentially involves dynamical considerations. In the present case some of the most important of the purely algebraic questions concerned are very interestingly illustrated by these dynamical considerations.

Mathematical Society, December 12.—Mr. C. W. Merrifield, F.R.S., president, in the chair.—Prof. W. S. Jevons, F.R.S., was elected a Member.—The following communications were made to the Society:—Mr. H. Perigol, on a kinematic paradox (the rotameter); Mr. S. Roberts, F.R.S., on the forms of numbers determined by continued fractions; Prince Camille de Polignac, on a graphic construction of the powers of a linear substitution.

Linnean Society, December 5.—Prof. Allman, F.R.S., president, in the chair.—Dr. I. Bayley Balfour demonstrated the peculiarities of a rare Myxomycetes, which species of Heterodictyum he showed bore characters intermediate between Cribraria and Dictydium.—Mr. G. Murray called attention to a peculiar greenish-yellow fungus (*Hygrophorus Wynniæ*, Berk. ?) from Bridlington, Yorkshire.—Examples of a moss new to Britain, the *Aulacomnion turgidum*, were shown by Mr. E. M. Holmes, who stated that they were found by Mr. West and Dr. F. Arnold Lees in Yorkshire; a comparison between the above and the common *A. palustre* was made.—Mr. F. H. Waterhouse read a paper on some Coleoptera collected by Charles Darwin, of geographical interest. These had lain undetermined for a long series of years, and now prove new to science. *Phytosus Darwinii*, from the Falklands, has unusually long, slender claws; *Choleva falklandica* is elliptical-shaped and strongly punctated. *Elmis brunnea* and *Anthicus Wollastoni*, from St. Helena, are noteworthy, inasmuch as Mr. Wollaston ("Coleop. St. Hel.") does not record either genus as existent there. *Scaphisoma elongatum*, from Rio de Janeiro, is the first species of the genus known to inhabit South America; and Prosthetops (*P. capensis*) is a novel genus with two ocelli, from South Africa.—Mr. C. B. Clarke, in a note on *Gardenia turgida*, stated that in books the flower calyx of males was alone described, while all herbaria specimens are diœcious, and males and females have hitherto been referred to different genera. The precise characters of each were denoted.—Dr. F. Day gave a summary of his (third) concluding paper on the geographical distribution of the Indian fresh-water fishes, in this dealing with the families Scombresocidæ, Cypriodontidæ, Cyprinidæ, Notopteridæ, and Symbranchidæ. Among the eighty-seven genera two only are African, thirty-two extend to the Malay Archipelago, and twelve are common to Africa and Malaya; of 369 species two are African, twenty-seven Malayan, and two common to both regions. In short, the fresh-water fish affinities preponderate to the Indo-Chinese and Malayan sub-regions; thus supporting Mr. Wallace's opinion as opposed to the view held by Mr. Blandford, who gives greater weight to African relationships, at least so far as mammals are concerned. Dr. Day, moreover, contends that the Indian fresh-water fishes point to three subordinate separate faunas—1. That belonging to the Ghauts, Ceylon, the Himalayas, and Malay Archipelago; wherein may be distinguished two fish races, a Palæarctic and a Malayan. 2. A fauna of the plains west of the Indus, with an African element in it. 3. That (by far the largest) spread over the plains east of the Indus, and which appears to have a Burmese connection.—The abstract was read of a second contribution on the mollusca of the *Challenger* Expedition, by the Rev. R. Boog Watson. This consisted of descriptions of species of Trochidæ belonging to four genera, viz., *Sequinzia*, *Basilissa*, *Gaza*, and *Bembix*; the three last being new and otherwise remarkable.—Messrs. Dowdeswell, Arthur Hammond, Thos. Hanbury, Joseph Sidebotham, Wm. Thomson, and Chas. A. Wright were elected Fellows of the Society.

Zoological Society, December 3.—Mr. Robert Hudson, F.R.S., vice-president, in the chair.—Mr. H. Seebohm, F.Z.S., exhibited a series of specimens of the hooded and carrion crows, and made remarks on their intermediate forms and geographical distribution.—Col. L. H. Loyd Irby, F.Z.S., exhibited and made remarks on the nests, eggs, and young of *Cypselus pallidus*, taken at Gibraltar.—Mr. Howard Saunders, F.Z.S., exhibited and made remarks on some eggs of Indian Laridæ (*Sterna bergii* and *Larus hemprichii*), which had been taken by

Capt. Butler, of H.M.'s 83rd Regt., on the Mekran Coast.—Dr. Day, F.Z.S., exhibited and made some remarks on some jaws of Indian sharks belonging to the genera *Galeocerdo* and *Carcarias.* —The Secretary called attention to an error which had been made in reference to the collection of butterflies from Billiton, reported on by Messrs. Godman, Salvin, and Druce, in the last part of the Society's *Proceedings*. The collection had been made and forwarded to England by Herr J. G. F. Riedel, of Koepang. —Mr. Sclater communicated some further particulars respecting the occurrence in Lancashire of the specimen of the black-throated Wheatear (*Saxicola stapazina*) exhibited at the last meeting of the Society.—Prof. A. H. Garrod, F.R.S., read a paper on the conformation of the thoracic extremity of the trachea in the birds of the order Gallinæ.—A communication was read from Dr. A. Günther, F.R.S., containing the description of some reptiles from Midian, collected by Major Burton. Amongst these were two new snakes proposed to be called *Echis decorata* and *Zamenis elegantissima*.—Mr. H. Seebohm pointed out the character of a new Sylvia from Abyssinia, proposed to be called *Sylvia blanfordi*, after Mr. Blanford, by whom it was obtained during the Abyssinian Expedition.—Mr. H. Seebohm also read notes on the identity of the birds which had been named *Hororni fortipes*, *Neornis assimilis*, *Horeites robustipes*, *H. brunneus* and *H. pallidus*, and proposed to reduce them to one species under the name *Cettia fortipes*.—Mr. Martin Jacoby read descriptions of some new species of Phytophagous Coleoptera from Central and South America.

Anthropological Institute, November 26.—Mr. John Evans, F.R.S., president, in the chair.—The Rev. John Robbins, D.D., was announced as a Member.—Mr. Worthington G. Smith exhibited a series of flint implements from the valley of the River Lea.—Mr. A. L. Lewis read a paper on the evils arising from the use of historical national names as scientific terms. The propositions which he endeavoured to establish were: 1. That there were at the first population of Europe certain primitive races, of which three are particularly described. 2. That these races are so mixed that at the present day the representatives of them appear not only in most European nations, but in the same families and among children of the same parents. 3. That notwithstanding this mixture and the effects which it must permanently have, racial characters display an astonishing permanence. 4. That this mixture, being so slow in its effects and yet having become so general, has probably been at work for a very great length of time—so great that the peoples to whom the earliest history introduces us were probably nearly as much mixed as those of the present day. 5. That it is desirable to discontinue the use of political names of those peoples as ethnic names, and to employ others, based on the physical characteristics of the individual. 6. That while physical characteristics are the only basis for a true division into races, yet in the practical application of this division the influence upon individuals of different races of a community of language, custom, history, or tradition must not be lost sight of, although these things do not prove community of race, but only the contact at some time or other of the races to whom they are now common.—The director read a paper by Prof. Daniel Wilson, LL.D., on some American illustrations of the evolution of new varieties of men. In the mingling of different races in America, so complex and varied, all subjected to the influences of climate and social habits, and all mingling in blood in a greater or less degree with the native red races, hybridity had resulted on a great scale. The process had already been developed sufficiently long to afford important indications of the evolutions of permanent hybrid varieties. A specimen is to be seen among the tribes of the half-breeds in Manitoba, as it were in the process of evolution; while sheltered within the remote Arctic regions man can be studied among the Esquimaux in conditions closely analogous to those which are ascribed to a post-pliocene, if not to a pre-glacial period. In the abrupt collision of the civilised races of Europe with the American aborigines, it had always been taken for granted that the latter were doomed to inevitable extinction, and that the land would be peopled with the purely civilised races of the world. There is no question, however, that from an early date there have been intermarriages between Europeans and the American races. A growing feeling is manifesting itself in the United States and Canada that the Indian population is not doomed to extinction, and that a much larger amount of healthy intermarrying and consequent absorption has existed than unobserving critics had any conception of, and the native Indian element is a factor in the population of the New World destined to exercise an enduring influence on the ethnical character of the Euro-American races.

CAMBRIDGE

Philosophical Society, November 4.—The following communications were made to the Society:—The physical constants of hydrogenium, by Prof. Dewar, Part 2. This paper is a continuation of an investigation into the physical constants of hydrogenium. The first part appeared in the *Transactions* of the Royal Society of Edinburgh, vol. xxvii., and had reference to the specific gravity, specific heat, and coefficient of expansion of the occluded hydrogen. These observations led to the conclusion that the specific gravity was independent of the amount of condensed gas, and had a mean value of 0·62. This result has been confirmed by the subsequent experiments of Troost and Hautefeuille, and what is very remarkable, they deduce an identical value for the density of hydrogen from observations on the hydrides of potassium and sodium. The specific heat, relatively to palladium, of the condensed hydrogen, appeared to vary inversely as the charge, but taken relatively to successive charges was nearly constant, and had the value 3·4, which is identical with that of gaseous hydrogen at constant pressure. The coefficient of the cubical expansion of the alloy is about twice that of palladium, and that of the hydrogen in its compressed state not more than three times that of mercury. This communication deals with the thermo-electric relations and conductivity of hydrogenium. It is shown that the electro-motive force of a junction of hydrogenium palladium is at ordinary temperatures nearly equal to that of an iron copper junction, and that it increases with the temperature according to the general parabolic law, the rate of the increase being, however, greater than iron copper and subject to a regular variation on account of successive heatings. The formation of thermo-electric piles, and of neutral points in a uniform wire of this substance, along with the continuous formation of thermo-electric currents through the application of a hydrogen flame were explained and shown. Experiments on the electric resistance show that it increases directly as the amount of condensed gas.—Studies in spectrum analysis, by Professors Liveing and Dewar. The authors describe the reversal of characteristic lines of rubidium and cæsium when the chlorides are heated with sodium in glass tubes in an atmosphere of hydrogen or nitrogen, and a bright light is viewed through the vapours. They remark that the violet lines of rubidium, and the most refrangible of the cæsium lines are first seen, and broaden out the most when the temperature rises, contrary to what might have been expected from the analogy of other cases. The absorption lines observed coincided with the bright lines of the metals heated in a flame, not with the lines which they give in a dense electric spark; but the authors obtained spectra similar to the flame spectra by passing sparks from an induction coil without a Leyden jar, between beads of fused chlorides of those metals, although simpler spectra were produced by the more abrupt discharges produced by interposing a Leyden jar. The authors further described absorption spectra produced by magnesium vapour when mixed with hydrogen, potassium, and sodium respectively. That produced by magnesium and hydrogen consisted of a line a little less refrangible than the *b* group, and a band rather more refrangible than the *b* group, fading away towards the blue. The constant appearance of these absorptions when the vapour of magnesium in hydrogen was observed in a hot iron tube, led to the endeavour to obtain the corresponding luminous spectrum. This they succeeded in doing by taking sparks from an induction coil, without a Leyden jar between magnesium wires in a tube full of hydrogen. It appears that the compound to which this spectrum is due is formed only within a certain range of temperature, and is dissociated at higher temperatures—for the spectrum is scarcely seen at all when a large Leyden jar is used, which may be supposed to have the effect of shortening the time of discharge and increasing the temperature. Further, this compound does not seem to be formed when the pressure of the hydrogen is much reduced. In the case of sodium and magnesium they observed an absorption line in the green not observed in either vapour separately; and when potassium and magnesium were used, a characteristic pair of lines in the red always appeared, and sometimes another line in the blue. The authors have not yet seen these as bright lines. In the course of observations on the spectra of sundry rarefied gases the authors have been led to conclude that electric sparks take a selective course in a mixture of gases, and that the differences in the spectra

observed in different parts of the same tube are probably due to the existence of more than one gas in the tube. Tubes of nitrogen which did not show the lines of hydrogen at all when sparks from an induction coil without a Leyden jar were passed through them, gave strong hydrogen lines when a large jar was interposed. A bulb tube with magnesium wires filled with hydrogen at low pressure gave in one half scarcely any spectrum but the F-line of hydrogen, while the other half gave the spectrum of acetylene. They generally found hydrogen lines and flashes of sodium (no doubt from the glass) in tubes very much exhausted; and they conclude that impurities enter such tubes from sources hitherto unsuspected. Tubes filled with oxygen obtained from silver iodate have been found to give the spectrum of iodine, pointing to the conclusion that chemical reactions occur at very low pressures which are not produced under other circumstances. Generally the authors conclude that the spectrum of a gas in a rarefied state affords the most delicate test of its purity, and that it is to the chemical problem of obtaining pure gases that attention needs to be specially directed.

PARIS

Academy of Sciences, December 9.—M. Fizeau in the chair.—The following papers were read:—New method for determining the flexion of telescopes, by M. Loewy. The principle is to produce in the field, besides the images of the eyepiece and objective (whose position may vary), a third image emanating from the axis of rotation, which, completely independent of the flexion of the tubes, undergoes only a slight displacement due to the auxiliary lens. This image serves as a means of estimating the relative displacement of the two others. (A concavo-convex lens is placed in the axis of the central cube, and on its axis of rotation.)—Examples of calculation of the torsion of prisms with mixtilinear base, by M. de Saint-Venant.—On the binary form of the seventh order, by Prof. Sylvester.—Study on ordinary and compound steam engines, steam jackets, and superheating, according to experimental thermodynamics, (extract), by M. Ledieu. Some observations are here made on neutral spaces and their influence, the restriction of these having been one direction of recent improvement.—On the works of the Saint Gothard tunnel, by M. Colladon. After recounting obstacles which have retarded the work—among others, greatly increased and violent infiltration, and the swelling of a plastic mass of decomposed felspar and gypsum on contact with moist air, exerting tremendous pressure on supports—he gives information about the air compressing and ventilating apparatus and the boring machines. It is expected that about eight years wlll suffice for the completion of the work. The difference between the first estimated and actual expense will, it is thought, be nearly 100 million francs.—On a series of soundings undertaken by M. Roudaire in view of the formation of the African interior sea, by M. De Lesseps. These soundings will cover about 500 leagues, and will occupy M. Roudaire about six months, after which it will be possible to estimate fairly the expense of the project. M. De Lesseps describes what he saw of that region.—Report on a memoir of Prof. Lawrence Smith on the native iron of Greenland and the dolerite it contains. The reporters recommend insertion of this interesting memoir in the *Recueil des Savants étrangers*.—Diseases of plants caused by *Peronospora*; attempted treatment; application to the lettuce-disease, *P. Gangliiformis*, Berk. Memoir by M. Cornu.—M. Werdermann replied to M. Reynier's reclamation of priority with regard to the electric lamp. He maintains that his (W.'s) lamp depends not on the effect of incandescence of a heated carbon, but on an extremely small voltaic arc; the incandescence of a small part of the electrode is merely an inevitable consequence.—On an automatic regulator of currents, by M. Hospitalier. This consists of a one-layer resistance bobbin, having a portion of its wire laid bare, and in contact with a slightly convex distributor connected with an armature before an electro-magnet which is affected by the current to be regulated.—On a small telephonic apparatus, by M. Boudet de Paris (sealed packet opened). This refers to a telephone in the form of a watch, which, with a microphone, gives speech well. M. du Moncel referred to a very advantageous arrangement of a speaking microphone (by M. Boudet de Paris), which he would shortly describe.—On the reduction, in continuous fractions, of a pretty extensive class of functions, by M. Laguerre.—On a point in the history of mathematics, by M. Desbovas.—Theorems on prime numbers, by M. Proth.—On a remarkable specimen of siliciuret of iron, by Prof. Lawrence Smith. This piece is remarkably rich in silicium (about 15 per cent.), and is evidently the product of a blast furnace. There are no such furnaces where it was found, but there are some a few miles away, and about 100 miles from the spot was one which supplied iron having 8 per cent. silicium, and gave up working because of want of demand for such iron. Prof. Smith thinks the piece may have been (exceptionally) produced there. M. Daubrée said industry has never been known to produce an alloy of iron with nearly so much silicium. The highest proportion at the Exhibition was 10 per cent.—On a new acid obtained from camphor, by M. Haller.—On the formation of hexamethylbenzine by the decomposition of acetone, by Mr. Greene.—On normal ethyloxybutyric acid and its derivatives, by M. Duvillier.—On the presence of ytterbine in the sipylite of Amherst, by M. Delafontaine.—Existence of baryta and strontian in all rocks constituting primordial strata; metalliferous veins with gangue of baryta, by M. Dieulafait. He infers from the facts that baryta and strontian have the same origin, viz., the primordial rocks; hence the metalliferous ores (manganese, lead, zinc, &c.), for which baryta serves as gangue, has also this origin.—On the dangers in use of borax for preservation of meat and the reasons why some substances cause meat to lose its nutritive properties, by M. Le Bon. He prescribes, in principle, the use of chemical substances, even the apparently inoffensive salt, for preservation of meat. The most nutritive part of meat is the juice, and this, when the meat is put in saline solution or covered with a salt in powder, makes rapid exchange of its nutritive principles through endosmose. He hints at a new mode of preservation, however, other than cold.—On an artificial pyroxene (diopside), by M. Gruner.—Influence of atmospheric electricity on fructificatien of plants, by M. Grandeau. It greatly stimulates the phenomenon.—On a disease of the coffee-tree observed in Brazil, by M. Jobert.—On the diffusion of heat by leaves, by M. Maguenne.—On the power of absorption of water by wood, by M. Maumené. The property varies (for different woods) between 9·37 and 174·88 per cent. of the absolutely dry wood; the latter figure was obtained with chestnut.—On a scientific balloon ascent of October 31, by M. Tridon.

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THURSDAY, DECEMBER 26, 1878

THE ASSOCIATION OF LOCAL SOCIETIES

LOCAL Societies: What is their aim and what purpose do they serve? How may this aim be most surely gained? How can this purpose be most effectively carried out? These are questions which naturally arise when considering the subject of local societies.

The *aim* of every local society should be to raise the intellectual status of the locality. The *purpose* to do so in that way most generally useful. It is the mind of the community which has to be raised by affecting the minds of the individuals. Individual minds are to be affected by contact with material surroundings. These surroundings influence us through the powers of observation, hence *careful and accurate observation* must exist among the members of a society fulfilling its proper functions. The greater the number of members exercising such observation the greater the usefulness of the society. It is almost needless to instance other mental qualities as necessary for success, because experience shows that when once the observing faculty has received its due share of attention, the power of using the observations made follows in due course. The faculty of observation must be drawn out and cultivated by contact with matter in relation to man, and by contact with matter considered apart from man as existing in a state of nature. And just as it is important that in the culture of the individual a one-sidedness should be specially avoided, so in raising the culture of a community it is equally important that opportunities or suggestions for mental improvement *all round* should be afforded. Hence we are inclined to think it advisable that especially in the case of small country towns scientific studies, or suggestions for such, should proceed from the same platform as those studies which are often spoken of as more purely literary. Of course *literature* includes the records of science, but still for general purposes the meaning is clear when a literary institute or society is spoken of as distinguished from a scientific. Among the lower types of animals there is a want of specialisation of parts; very different functions may be performed by the same part or the whole of the body; in the higher, specialisation prevails, each function has its own organ, and the function is performed more efficiently. In large towns science may be pursued apart from general literature, and even each special science may stand on its own platform, but in small towns this is out of the question, and I believe unadvisable, for the over-performance of one function in the lowly organised society s checked by the claim of the general body. Moreover, the tastes of a community being naturally various, it becomes essential to present intellectual food of various kinds. Hence we cannot but think that small local societies should be both literary and scientific. The two aspects of culture will support and strengthen each other, and the introduction of a new clique, or party, or sect be avoided. For it must be remembered that one of the distinct collateral advantages of such societies is that a common platform is provided upon which men of all political or religious beliefs can stand and work together. No one who is acquainted with the social conditions of our small towns can underrate the importance of this.

But how are such societies to work? I would reply, from within, outwards. Not, in the first place, by calling in extraneous help, by engaging eminent men to give courses of lectures, but by arousing the spirit of inquiry and observation amongst the townsfolk. Let but a few natives come forward with short papers on any subjects with which they may be especially acquainted, the subjects being treated in such a way as to elicit a discussion or inquiries, a spirit of interest will soon be aroused, and minds put into a proper attitude for the reception of truths before quite unknown to them, and for the prosecution of some special subject as a study. In practice I would strongly advise the following course to be pursued by any embryo literary and scientific society. Have two classes of meetings: one the *ordinary meeting*, at which members alone (and therefore townsfolk) should read short papers, upon which a discussion should afterwards be encouraged; and *public lectures*, given mainly by non-residents, and to which the general public should be admitted on the payment of a small fee. At the ordinary meeting the local talent and observation is drawn out, and at the public lecture new subjects are introduced to the notice of members. At the former, notices of local phenomena and history, or the occasional original investigations of members, are recorded; at the latter, new lines of thought are often indicated, or systematic instruction given in some one subject.

A society established on some such basis is then in a position to encourage the collection of objects of *local* natural history, to establish a *local* museum, and carry out field excursions during the summer months. Moreover, the experience of many years past has shown me that the life—and therefore the growth of culture—in such a society is far greater than in those cases where only a yearly course of *lectures* is organised, the greater part of them being given by strangers. Next comes the oft-repeated question, But how long will such a society last? Many are ready to say, We have tried some such plan, and success has attended our efforts for one or two years, and then the society has died out. On this part of the subject a few words will now be said, and the remarks made are founded upon experience gleaned amidst the practical working of local societies in Cumberland during the past nine years.

How, then, can permanence be ensured? In a small town or district local resources and talent are apt to become exhausted or unavailable. A time will surely come when the intellectual movement will wane and the society be on the brink of non-existence. But the very usefulness of such a movement must consist in its stability; there should be a growth, not a bare existence. To insure this stability I suggested some years ago that the four societies then existing in the Lake District and West Cumberland should be united for general purposes, while each society should retain its individuality. After many preliminary difficulties were overcome, the union was effected, and since that time each society has grown stronger, four new societies have been formed, and the total number of members increased from a few hundred to nearly 1,200.

The objects to be attained by this association of societies are as follows:—1. Increased strength to be derived from mutual help, encouragement, and a spirit of

honest emulation. 2. The union affords greater facilities towards publishing transactions and securing the services of eminent lecturers. 3. An annual meeting of the associated societies affords an opportunity for the discussion of principles of working and promotes the general life. 4. The annual meeting being held in a fresh town each year helps to keep the country alive to the Association work, and encourages the formation of new societies.

The constitution of the Cumberland Association is as follows:—The president to be a man of local note and high culture, and to serve for a period not greater than two years.[1] The Presidents of individual societies to be vice-presidents of the Association. The council of the Association to consist of two delegates from each society, chosen annually. The treasurer and secretary (honorary) to be one and the same person, and fully acquainted with the county in all its aspects.

The working of the Association is carried on thus: The Association secretary keeps a record of all papers and lectures brought before the individual societies. Before the commencement of each winter session he communicates with all the local secretaries, and from his knowledge of available intellectual stores in the county, helps each in the drawing up of the winter programme in whatever direction help may be specially needed. It is his duty also to help forward the establishment of local classes where such are possible. At a council meeting held in the autumn some public lecturer is decided upon who shall go the round of the associated societies during the winter, and a grant is made towards his expenses from the Association funds (of which more anon), the rest being made up by each society served.

The annual meeting takes place at Easter or in May, and lasts two or three days. The Association President delivers his annual address, reports from the several societies are read and discussed, original papers are read, lectures given by one or more eminent men, and field excursions made.

At the close of each winter session the local secretaries send into the Association secretary any papers which have been selected by the local committees as worthy of publication. If the Association council approve these papers they are published in the *Transactions* at the Association expense. The funds of the Association are gathered thus: Each society pays an annual capitation grant of 6*d*. per head on all its members. There is also a class of Association members, residing at a distance from, and not belonging to, any local society, who pay an annual subscription of 5*s*., and are virtually considered members of all the societies, and have the privileges of such. The *Transactions* are sold to the societies and Association members at the price of 1*s*., the public being charged 2*s*. 6*d*. Some of the societies purchase copies to the full number of their members, and present them, others take only a limited number of copies (determined by the local society committee) and re-sell to those of their members who care to possess them. In this way the greater part of an edition of 800 copies of the *Annual Transactions* is disposed of. Authors are allowed extra copies of their own papers at a moderate charge, and when all expenses are met, a fair balance is left to carry on to the next year.

It should be noted that of the eight societies in Cumberland, now associated, the local annual subscriptions of members in each society is generally 5*s*.; in one case, however, it is 3*s*. 6*d*., and in another 2*s*. 6*d*. It is a rule of the Association that members going from one society to another to afford help in the carrying out of the various programmes, should have their expenses paid by the society helped. Such is the general constitution and mode of working of the Cumberland Association, which has undoubtedly succeeded in its aim, so far as the keeping up of existing societies and the formation of new ones is concerned. The *Annual Transactions*, too, include many papers of local value, and some of general interest, while among the eminent men who have kindly come forward to lend their services at the Annual Meetings, are the Astronomer-Royal, the Bishop of Carlisle, Prof. Shairp, Prof. Wm. Knight, and I. Fletcher, M.P., F.R.S. At present, however, the Association is but in its infancy, and may be considered more or less of an experiment, yet that some such method of union is desirable amongst local societies in the various counties or districts of England few will deny. Time will show how the system may be improved and varied to suit special circumstances, but I cannot but think that the plan of association to carry out the larger objects of societies, and the annual meeting of the associated societies in successive towns of a county, must economise labour and promote the healthy culture of the county in which the work is carried on.

Amongst the difficulties presenting themselves in the early days of the association, the following occurred. For several previous years a Cumberland and Westmoreland Antiquarian and Archæological Society had flourished, and it was feared that the new County Association would clash with its existence. The Antiquarians thought it best not to amalgamate with the associated society, its constitution being in many points different from theirs, but it was resolved that whenever papers, bearing on local antiquarian or archæological subjects were read before any of the associated societies, these papers should be offered by the Association council to the Antiquarian and Archæological Society for publication in their *Transactions* if deemed worthy. Moreover, some of the officers of the Association are active members of the Archæological Society, and so far from their being any antagonism, the two decidedly help one another forward in the general work of gleaning local knowledge and diffusing culture.

As hon. secretary of the Cumberland Association, I should feel very grateful for any hints or suggestions from the readers of NATURE. What is wanted in every county is more culture, and that carried on in a *natural* way, and with a true love of nature in all her aspects.

J. CLIFTON WARD

[1] The Lord Bishop of Carlisle acted as president for two years, and I. Fletcher, M.P., F.R.S., is now in his second year of presidency.

NEWCOMB'S LUNAR RESEARCHES

Researches on the Motion of the Moon, made at the United States Naval Observatory, Washington. By Simon Newcomb, Professor U.S. Navy. Part I. (Washington, 1878.)

THE author prefaces his work with the remark that for several years after the publication of Hansen's Tables of the Moon, there was a very general belief that

the motion of our satellite could be followed by their means with the same accuracy as that of the other heavenly bodies, after having been made the subject of astronomical and mathematical research for two thousand years. This expectation was soon proved to be far from borne out. Prof. Newcomb showed in 1870 that the accuracy of the Tables since 1750 had been secured only by sacrificing the agreement with observations previous to that epoch, and that about 1700 the Tables deviated more widely from the observations than the previous ones; and it may be added that those who had been engaged in examining the old eclipses were aware that Hansen's Tables did not represent the phenomena only as far back as the commencement of the eighteenth century, so well as Burckhardt's or Damoiseau's, which had been used for our ephemerides up to the date of their publication.

A new investigation of the subject for the purpose, if possible, of ascertaining the cause of these unexpected deviations was entered upon at the Naval Observatory, Washington, and was made a part of our author's official duties in that establishment. In the present volume we have the results of researches on the discordances in question, based upon observations before the year 1750. This portion of the work was originally intended to follow the study of the mathematical theory of the inequalities of long period in the moon's mean motion, but for reasons explained in the Introduction, that part of the inquiry is still incomplete. The year 1750 was fixed upon as the terminal point in the investigation in this first part, as it is the epoch when exact meridian observations commenced, and that which separates the period within which we are in possession of observations reduced on modern data, from the period during which neither really accessible observations nor tables of reduction are available.

Prof. Newcomb supplies an historical introduction in which the discovery of the secular acceleration, and the examinations of ancient eclipses with the view to fix its correct amount by observation are briefly noticed, as also Ferrel's paper, published in 1853, containing "the first known attempt to calculate from theory the retardation produced by the action of the moon upon the tidal wave," and the researches of Adams and Delaunay. He proceeds to give a summary of the data now at our disposal for determining the apparent secular acceleration of the moon from observation alone. These include the statements of ancient authors from which it has been inferred that total solar eclipses have been witnessed at certain points of the earth's surface at dates approximately indicated, and the author points out the uncertainty attending our interpretation of such records. He considers that the circumstance which we should regard as most unequivocally marking the totality of an eclipse is the visibility of stars, though he thinks that even this criterion is hardly to be admitted as conclusive, because Venus may be seen during a considerable partial or an annular eclipse, and at certain times when there is no eclipse at all, there is also another difficulty in some cases, in determining the precise localities where the phenomena were observed. We have also the series of lunar eclipses upon which Ptolemy founded his theory and which are recorded by him in the Almagest. These are followed by the observations of the Arabian astronomers, chiefly contained in an Arabic manuscript belonging to the University of Leyden, a translation of which was made by Caussin and published by the French Government in 1804, under the title "Le Livre de la Grande Table Hakémite." Prof. Newcomb remarks that this work contains what are entitled to be considered the earliest astronomical observations of eclipses which have reached us, for although some of the data furnished by Ptolemy, Theon, Albategnius, and others, may have been the results of astronomical observations, in no case have the quantities actually observed been handed down to us. The entire number of eclipses in this collection is twenty-eight, and the times of concluded beginning and ending were usually determined by noting the altitudes, which were recorded sometimes in whole degrees only, at others "in coarse fractions of a degree." There must remain a doubt how nearly such times apply to those of actual contact, but Prof. Newcomb suggests in this part of his work that by the mean of all the observed times the error in the moon's mean longitude can be reduced to not more than a minute of arc. The Arabian observations are followed by those of European observers prior to the invention of the telescope, including Regiomontanus, Bernard Walther, and Tycho Brahe: of the latter, the author remarks, "It is wonderful if so indefatigable an observer never observed an occultation of a star or planet by the moon, yet I have never succeeded in finding any such;" he made a careful examination of Tycho's observations during periods in which the bright star Aldebaran must have been occulted, to no purpose. The observation of eclipses and occultations with the aid of a telescope, Prof. Newcomb remarks, may be considered as commencing with Bullialdus and Gassendus, but they had no clock, and only fixed the time by noting the altitude of the sun or a star. The application of the clock commences with Hevelius, and in the scarce volume of his "Machina Cœlestis" are found a number of occultations thus observed. Then follow the observations of Flamsteed and the astronomers at the observatory of Paris, the Cassinis, La Hire, and Delisle, the latter of whom also observed at St. Petersburg. Prof. Newcomb, during a visit to Paris, while Delaunay was in charge of the Observatory, was fortunate in having all the archives of that establishment unreservedly placed at his disposal. He found amongst them most of the original note-books of the French observers since the year 1675, in which were contained a great number of occultations that had been quite forgotten, those which had appeared in the *Memoirs* of the Academy, forming but a small fraction of the whole. Again, on visiting Pulkowa, M. Struve gave him access to the records of Delisle's observations, 1727 to 1747, forming a useful supplement to those of Paris, which had diminished in number after 1720.

Prof. Newcomb concludes this historical notice with remarks on observations since the time of Bradley, and granting certain fundamental premises, suggests that the secular acceleration of the moon may admit of nearly as accurate determination from the modern observations, as from their combination with the ancient ones.

Having thus briefly recapitulated the data available for investigation, the author adverts to ancient eclipses, presumed to have been total from the narrative of the historians, at certain points of the earth's surface. The

eclipses he admits in this category are eight in number, from that of Thales, B.C. 585, to the eclipse of A.D. 364, described as total at Eoos. Looking for mention of distinct indications of totality, he has not included such an eclipse as that of B.C. 763, the record of which was discovered by Sir Henry Rawlinson on one of the Nineveh Tablets in the British Museum, where importance appears to be attached to it by the description being underlined; hence its presumed totality. The eclipses of the moon recorded by Ptolemy in the Almagest are thoroughly examined, with satisfactory results, except in the case of the eclipse of B.C. 383, December 22, where there appears to be a mistake as to its having been really observed at Babylon; and he concludes that during the eight centuries preceding the Christian era the mean longitude of the moon in Hansen's Tables requires a correction of about 18′. The Arabian eclipses, solar and lunar, twenty-five in number, between A.D. 829 and 1004 are then compared with the Tables.

In the next two sections Prof. Newcomb supplies a full description of the method adopted for deducing the errors of the lunar elements from the eclipses and occultations, and for determining the effect of changes in the elements upon the path of the central line of an eclipse, adopting, in the latter case, formulæ originally given by Bessel.

In a following portion of the volume, occupying about a hundred pages, are given, mostly as originally recorded, the observations of occultations and eclipses by Bullialdus and Gassendus, Hevelius, and, as the author terms them, the astronomers of the French School, between 1670 and 1750, preserved in the archives at the observatories of Paris and Pulkowa; it is needless to say that this section possesses a great value, supplying as it does the particulars of so many observations hitherto unpublished. We have then the positions of the moon from Hansen's Tables, used in the comparison of the preceding observations with theory, in the calculation of which, and for the numerical work generally, Prof. Newcomb was assisted by a grant from Congress, sufficient to enable him to employ two computers. Certain modifications of the strict form of application of the Tables were considered allowable for the older observations, their degree of accuracy rendering an exact computation of Hansen s Fundamental Argument of no advantage, and he considers such modified plan of employing Hansen's Tables, preferable to the use of the older Tables, which might be adopted for the sake of saving labour. A "Tabular exhibit of Reduction of the Occultations," 286 counting immersions and emersions separately, is then given. The equations of condition for the occultations, and a provisional solution follow.

In the next section the author presents an elaborate discussion of eclipses from 1620 to 1715, which it may be remembered is the last total eclipse that was methodically observed in this country. In this series is included the eclipse of 1639, June 1, observed by Gascoigne and Horrox amongst others, and 1706, May 11, which was total in the south of France, but of which Prof. Newcomb takes no other observations into account than those of La Hire, of Paris: the fact, indeed, is that the observations along the belt of totality appear to be strangely and unaccountably discordant, so far at least as regards the beginning and ending of the total phase. The eclipse of 1715 is very fully discussed, and remarking that by Halley's organisation of a numerous body of observers throughout the path of the moon's shadow across England, valuable observations for determining its limits were procured, the author deduces from them a correction to the motion of the moon's node, which he finds to be $10'' \times T$ (T being counted in centuries from 1850), the argument of latitude being diminished by this amount; this result he considers nearly certain with respect to its algebraical sign, but observes that it must be affected by any corrections of Hansen's value of the moon's parallax.

We now reach the main conclusions to which Prof. Newcomb is led, by the laborious and masterly discussion of observations prior to 1750, of which necessarily little beyond an outline has been given here. The theoretical value of the secular acceleration of the moon's mean motion due to the cause discovered by Laplace, has been fixed with accuracy by Prof. Adams and Delaunay: the latter geometer, carrying his approximation to a greater number of terms than had been included by Prof. Adams, assigned $6''\cdot18 . T^2$. But this value, as is well known, has not been found to accord with the older observations, and the difference between the theoretical value and that which observation seemed to require, has been generally attributed to a retardation of the earth's axial rotation; thus, as Prof. Newcomb remarks, "the apparent secular acceleration will be made up of two parts—the one a real acceleration, the other an apparent one, due to the change in our measure of time. But further, he says it will be found that the hypothesis of a constant tidal retardation does not account for the observed mean motion of the moon, and either the retardation must be supposed variable, even to becoming at times an acceleration, or it must be admitted that her mean motion is affected by changes not hitherto explained. He then proceeds to inquire what deviations of the moon's mean motion remain unaccounted for, and with this object he first collects into tabular form the individual corrections to Hansen's mean longitudes, derived from the discussion of eclipses and occultations from 1621 to 1728, with their probable errors, the latter being necessarily somewhat arbitrary, for want of data for their rigorous computation. The older results do not exhibit larger discordances than might be expected. In order to complete the investigation of anomalies in the moon's mean motion unexplained, it was necessary to have the errors of Hansen's Tables from 1750 to the present epoch, or to 1875, and these were partly obtained from Hansen's paper in the *Monthly Notices* of the Royal Astronomical Society, and, since 1850, from Part III. of the publications of the American Transit of Venus Commission, where it was shown by Prof. Newcomb that at the epoch 1875·0 the meridian observations at Greenwich and Washington agreed in indicating a correction to the tabular mean longitude of $-9''\cdot7$. The occultations about the same time giving a correction nearly two seconds less, it is assumed that the true correction at this epoch was $-8''$. Hansen introduced in his tables a term depending on the argument, 8 times the mean motion of Venus *minus* 13 times the mean motion of the earth, which has not been theoretically explained, and is to be regarded as empirical. This term is therefore removed from the theory, before examining, as Prof. Newcomb proceeds to

do, how nearly theory alone, without any empirical correction, will represent the observations. It is then at once apparent that the residuals cannot be represented by corrections to the epoch of mean longitude, mean motion, and secular acceleration, and any approximation to a mean value of the latter would have different values, according to the mode of using the data. To obtain the best result from the ancient and modern observations combined, it was deemed advisable to assign a minimum probable error of 4″ or 5″ to each residual for the modern observations. Equations of condition for correction of epoch, mean motion and acceleration are formed, and extend from B.C. 688 to A.D. 1875, or over a period of 2,500 years, and the resulting corrections to Hansen's values for 1800, are, for mean longitude, $+3''\cdot90$, mean motion $-19''\cdot03$, and for secular acceleration $-3''\cdot36$; Hansen's adopted value of the latter being $12''\cdot17$, the value which best satisfies the observations discussed by Prof. Newcomb is found to be $8''\cdot8$. Though he considers this correction to the tabular acceleration to be clearly indicated, the residuals for the modern observations are yet of such magnitude as to be wholly inadmissible, and therefore the theory in its present state will not represent observations with any value of the secular acceleration, and respecting the cause of the magnitude of these remaining differences, he makes two hypotheses: (1) that they are only apparent deviations caused by inequalities in the earth's axial rotation, (2) that they arise from one or more inequalities of long period in the actual mean motion of the moon. Examining the effect of the first hypothesis, he arrives at the conclusion that if it be correct "the problem of predicting the moon's motion with accuracy through long intervals of time must be regarded as hopeless since it cannot be expected that variations in the earth's axial rotation will conform to any determinable law," and, he adds, "success in tracing the deviations in question to the moon itself and to the theory of gravitation is therefore a consummation to be hoped for." With regard to the second hypothesis, it is seen that the residuals of the equations of condition indicate that the modern observations may be nearly represented by a term having a period of between 250 and 300 years, and hence Prof. Newcomb inquires how closely an empirical correction to Hansen's first term depending upon the action of Venus, the period of which is 273 years, will accord with the modern observations, and he finds a very satisfactory agreement. An additional diminution of 10″ in the secular mean motion of the moon results, which at the present epoch involves a further diminution in the secular acceleration of $0''\cdot5$, that the ancient observations may be well represented; thus the acceleration becomes $8''\cdot3 \,.\, T^2$. A table is given exhibiting the corrections to Hansen's mean longitude from 1620 to 1900 for every tenth year; in 1880 it is $-11''\cdot2$, and in 1900 $-24''\cdot6$.

This important volume concludes with some remarks upon the bearing of the value of the moon's secular acceleration deduced from the investigations, of which we have endeavoured to give a general outline here. Prof. Newcomb thinks it is apparent that one of two propositions must be accepted: "Either the recently accepted value of the acceleration and the usual interpretation of the ancient solar eclipses are to be radically altered, the eclipse of -556 not having been total at Larissa, and that of -584 not having been total in Asia Minor; or the mean motion of the moon is, in the course of centuries, subjected to changes so wide that it is not possible to assign a definite value to the secular acceleration." It is certain that there will be a difference of opinion upon his main conclusions, and for this he expresses himself fully prepared. If a definite theory of the apparent inequalities of long period in the moon's motion cannot be formed, or if the moon's mean motion is subject to such changes from age to age that no invariable and well-defined value of the secular acceleration can be deduced, then he urges it is not certain that the question whether Hansen's tabular mean longitude during centuries preceding the Christian era does or does not require a considerable negative correction can ever be conclusively settled, since no conclusions can be drawn except from observations made near the period in question, and he advocates the necessity of a further investigation into the eclipses and other data on the two hypotheses, first that Hansen is correct during the period named above, and second, that a correction of $-16'$ is required, and suggests that the question should be examined in this manner by some independent authority. If, on the other hand, it is not possible to form a perfect theory of all the inequalities in the moon's mean motion independently of observations, he thinks it will be practicable to arrive at a value of the secular acceleration from the modern observations, reliable within $0''\cdot5$.

ROSCOE AND SCHORLEMMER'S CHEMISTRY

A Treatise on Chemistry. By H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S., Professors of Chemistry in Owens College, Manchester. Vols. i. and ii. (Macmillan and Co.)

THIS work is a most valuable contribution to the literature of chemistry. Its aim, as stated in the Preface, is to place before the reader a fairly complete, and yet a clear and succinct statement of the facts of modern chemistry, whilst at the same time entering so far into a discussion of chemical theory as the size of the work and the present transition state of the science permit, special attention being also paid to the accurate description of the more important processes in technical chemistry, and to the careful representation of the most approved forms of apparatus employed.

The manner in which this design has been carried out is such as might have been expected from the high reputation of the authors. The work commences with a very interesting historical introduction, in which the progress of chemistry is traced from the early times, in which it was merely an art subservient to alchemy, medicine, and a few branches of manufacture, to the time when, by the gradual accumulation of observations, and the discussion of them by men of philosophic mind, it rose to the rank of a science. A clear and impartial discussion is given of the relative merits of the various workers by whose labours the system of chemical philosophy now accepted was developed, showing how the phlogistic theory of Becher and Stahl first established a common point of view from which all chemical changes could be regarded,

and enabled chemists to introduce something like a system by which analogous phenomena could be classified and referred to a common cause; further, how the experiments of Black, Cavendish, and Lavoisier first showed the importance of attending, in the study of chemical changes, to the alteration in weight of the substances concerned; and how Lavoisier was ultimately led to the true theory of chemical combination, which regards it as consisting simply in the addition of one element to another, the weight of the product being exactly equal to the sum of the weights of the combining bodies.

Next follows a sketch of the labours of Bergmann, Richter, Cavendish, and others, which led up to the establishment by Dalton of the great doctrine of combination in multiple proportions, on which he founded the "atomic theory." The early experiments of Dalton are briefly described; his table of the relative weights of the atoms of certain elementary and compound bodies is given; and the Introduction ends with an account of the manner in which the exact values of the atomic weights were determined by Thomas Thomson, Wollaston, and more especially by Berzelius; of the discovery of the compound nature of the alkalis by Davy, and of a number of new elements by various chemists; and lastly, of the development of Organic Chemistry and its true relations to the chemistry of inorganic bodies; and the final establishment—chiefly by the researches of Liebig—of the fact that the science of Physiology consists simply in the physics and chemistry of the living body.

The Historical Introduction is followed by a chapter on the General Principles of Chemical Science, in which the methods by which the laws of chemical combination have been established are more fully described, especially that by which Lavoisier demonstrated the nature of combustion and the indestructibility of matter. This part of the subject is well illustrated by diagrams of the apparatus used in these important investigations. A list of the elements with their combining weights is then given, and a table exhibiting the arrangement of the elements in groups, chiefly, but not entirely, according to their combining capacity or quantivalence. Next follows a section on the laws of chemical combination, the methods of analysis and synthesis, the manner in which the law of equivalents and the law of multiple proportions were established, and the explanation of these laws by Dalton's atomic theory. This theory is adopted by the authors as the basis of all their explanations of chemical phenomena, and in this we think they are right: for without insisting on this theory as a matter of absolute certainty, we cannot but regard it as the only theory yet proposed which gives any rational and connected view of the laws of chemical action as established by experiment. There are, indeed, chemists of great eminence, who do not admit it, but hold out expectations of much more satisfactory explanations founded on dynamical views of chemical action. But these views have not yet been sufficiently developed to form a connected theory, and meanwhile we must make what we can of the theory of atoms, which, after all, is not necessarily inconsistent with any dynamical laws, or in other words, with any relations of matter to heat and electricity, that future experiment and observation may develop. There are, indeed, some philosophers who would have us believe in motion without matter, or in other words, in the movement of nothing at all; but this is high transcendental ground, on which we must humbly confess our inability to tread.

The consideration of the volume-relations of gases in combination, as established by Gay-Lussac, leads to the statement of Avogadro's law, according to which *equal volumes of all gases contain the same number of molecules.* This the authors rightly put forward as a hypothesis, the truth of which—like that of the law of gravitation—must be established by its accordance with the whole range of observed phenomena: for as such it must be received by the ordinary student, who is scarcely prepared to understand the manner in which it may be shown to follow as a necessary consequence of the kinetic theory of gases. An exposition is then given of the physical properties of gases, the continuity of the liquid and gaseous states, as demonstrated by Andrews, also a sketch of the kinetic theory of gases; and the chapter concludes with an explanation of the principles of Chemical Nomenclature and Notation.

The remainder of vol. i. treats of the Non-metallic elements. The preparation and properties of these bodies and of their compounds with one another, together with their industrial applications, are carefully described, and excellent figures are given of the apparatus employed for investigation and lecture illustration, also of manufacturing "plant." Especially worthy of notice are the illustrations connected with the manufacture of bleaching powder, sulphuric acid, and coal-gas. The volume concludes with a chapter on Crystallography, copiously illustrated with diagrams. The notation used is that of Naumann, which, for descriptive purposes, is perhaps the clearest and most graphic yet devised.

Vol. ii., part I is devoted to the general properties and classification of the Metals, and to the special description of those belonging to seven out of the twelve groups in which they are arranged by the authors. In this part of the work we find the same clearness and accuracy of description and explanation which are conspicuous in the first volume, both in the purely scientific portions and in those which relate to industrial applications. Excellent descriptions and figures are given of the manufacture of alkali and of glass, and of the metallurgy of zinc, copper, lead, silver, and mercury.

The book is well printed, and remarkably free from typographical errors. The few that we have noticed are not likely to mislead, and it is therefore not worth while to specify them, with the exception, perhaps, of one, occurring on p. 38 of vol. i., where it is said that the specific heats of the several elements are "universally" (instead of "inversely") proportional to their atomic weights.

The work, when finished, will afford the most complete systematic exposition of the existing state of chemical science that has yet appeared in the English language; and chemists will look forward with pleasure to the appearance of the second part of vol. ii., which will contain a description of the Iron manufacture, and to that of vol. iii., which will be devoted to the ever-growing subject of ORGANIC CHEMISTRY.

OUR BOOK SHELF

Manuals of Elementary Science—Crystallography. By H. P. Gurney, M.A. (Society for Promoting Christian Knowledge, 1878.)

THIS excellent little manual satisfies a want long felt, for, up to the present time, there was no book in which a general knowledge of the system of crystallography, first developed by Prof. Miller in his "Treatise on Crystallography," 1839, could be obtained. Prof. Miller's treatise and Tract are mainly occupied with the methods of calculation, and require a considerable knowledge of trigonometry. The manual before us aims at doing for these books what the crystyllographic introduction to Naumann's "Mineralogie" does for his "Lehrbuch der Krystallographie." It therefore avoids all the analysis used in the calculation of crystals, and limits itself to explaining the elementary geometrical principles involved in the representation of the faces by indices.

The method of development of systems of symmetry, rendered so familiar to us by Prof. Maskelyne, has been almost necessarily followed, and the author has consequently inverted the usual order of discussion of the different systems, beginning with the Anorthic, that of simplest symmetry, and proceeding through the different types of symmetry up to the cubic system, that of most complex symmetry. In the different systems the characteristic forms are shown to flow so simply from the conditions of symmetry that a moderately bright student ought to be able to deduce them himself after following Mr. Gurney's exposition in the first two or three systems. In his discussion of the rhombohedral system the author follows Prof. Miller. The hexagonal system, of which the rhombohedral is a hemisymmetrical development, is so imperfectly manifested by crystals that its discussion is only of theoretic interest and is unsuited to an elementary manual. In his discussion of merohedrism the author has not attended to the limiting condition, pointed out by von Lang, that the merohedral form should not be identical with the characteristic form of a system of lower symmetry, although here, likewise, he has the sanction of Prof. Miller's authority. The condition, however, is justified by the most recent observations, which have placed most of the minerals displaying such merohedrism in the systems of lower holohedral symmetry. We can heartily recommend the book to students even if they be able to study the more advanced text-books.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

On the Ancient Pitch of Organs

As I am obliged to intermit my researches on organ pitch for a few months, owing to pressure of other work, I wish to make a note of the point to which I have advanced, after consulting many old books, and actually measuring pitch and length of many old organ-pipes, for which I am obliged to the kind politeness of organ-builders, organists, and friends. When my researches are complete, they will appear with details in a paper to be read before the Society of Arts on the History of Musical Pitch, about a year hence. The delay arises from the difficulty of getting information from the Continent.

In England we have no organs older than the Restoration, 1660, as the Puritans smashed all church-organs in 1644-46.

The principle used by organ-builders was to make a certain pipe of the length of some multiple or easy sub-multiple of the standard length of measurement in their own country, and determine the other notes from its tone, according to the mean-tone or unequal temperament in universal use for organs everywhere till 1830, but beginning to be disused in France in 1834 and in England in 1854. There is an apparent exception in St. Jacobi Kirche at Hamburg, where equal temperament is claimed for 1720, when J. S. Bach played on that organ, and possibly in other old German organs. In England I have found the old unequal temperament still existing at St. George's Chapel, Windsor Castle, Kew Parish Church, St. Katherine's, Regent's Park, All Hallows the Great, Upper Thames Street, Maidstone Parish Church, St. Mary's, Shrewsbury, and several other organs which have been very recently re-tuned. The first equally tempered organ by Messrs. Gray and Davison was sent out in 1854.

The pitch note used from 1500 to 1650, at least in Germany, seems to have been F, for which a 13-foot pipe was employed for our F in the 16-foot octave. But the foot varied so much in Germany, being 3 per cent. longer than the English on the Rhine, and in Austria, and much shorter than the English in Central Germany, that the pitch thus determined varies by one to two equal semitones. The Brunswick foot, in 1620, where we have Prætorius's reference, possibly gave a tone of 35 vib. for the 13-foot pipe, an octave below the ordinary violoncello C.

In England Tomkyns (before the Commonwealth) fixes the F as 5 feet, which gives the A as 4 feet, and the double octave of this as 1 foot, and hence comes under the next category. The 13-foot F gives a 13-inch treble C, which, for Rhenish feet, would have a pitch of 425, whereas Handel's A was 423, having a pitch a minor third higher. This minor third constantly recurs. In Hamburg the St. Jacobi organ is a minor third sharper than the St. Michaelis organ, the first being a tone sharper and the latter a semitone flatter than French pitch. And strangest of all, the St. Jacobi organ had formerly one of its stops tuned to the low St. Michaelis pitch.

The old reason for fixing the pitch seems to have been to put the ecclesiastical tones within easy fingering for the organist, without using the chromatic notes (which *Arnold Schlick*, 1512, naïvely says is not convenient for most players), at the same time that they were within easy reach of a baritone voice. This is a point I have not yet worked out completely.

In England the foot-rule seems to have been generally adopted in early organs as the means of giving a standard, and it is not till about Green's time—a century ago—that I find it varied from this by a small fraction of an inch—not exceeding two-fifths.

The pitch of an open metal cylindrical flue-pipe used for the open diapason stop (but not "a show-pipe"), measuring 12 inches from the lower lip to the open end, varies from 472 to 475 vibrations in a second at 60° F. The variations are due to the size of the diameter, the force of wind, the opening at the foot, and the method of voicing. I have known such a pipe raised two vibrations in a moment by a slight alteration in voicing. This is the old standard pitch in England. Varieties depend upon the name of the note which it represents, and the classes of organs which I have met with in books or in reality, have hence been called by me the A foot, B flat foot, B foot and C foot organs.

1. The A foot organ has A 472 to 475. This was *Tomkyns's* pitch, as shown by Sir F. Gore Ouseley in his edition of "Orlando Gibbons," and seems to have been the pitch for which that composer wrote his Church music. It gives the mean tone C 565 to 570. As the French diapason normal is really A 435·875 (as determined originally by M. Cavaillé-Colt, and verified this year by Mr. Hipkins in Paris, by means of a Scheibler 440), this makes Tomkyns's pitch about three-quarters of a tone sharper than French pitch. This is the present existing pitch of St. Katherinen Kirche at Hamburg. The St. Jacobi organ, and also that in the Cathedral of St. Marie, at Lübeck, is a whole tone higher than French pitch. The great Franciscan organ at Vienna, 240 years old and untouched, gives A 460, which is only a semitone sharper than French pitch. These are the sharpest existing organs I have met with. The Franciscan organ is only used for the old ecclesiastical tone singing of the monks. This was also possibly the pitch recommended by *Prætorius* for church organs, the drawing in his book (1618) giving the B pipe one Saxon foot in length, with strong pressure of wind, and the Saxon foot being 7 per cent. shorter than the English.

It is as well to mention in passing that the tones and semitones here spoken of for measuring purposes, if not otherwise qualified, are *equal* semitones, and that, near enough for such purposes, an equal semitone and tone higher have 6 and $12\frac{1}{4}$ per cent. more vibrations, and thus a quarter and three-quarters of a tone higher have 3 and 9 vibrations more per cent. For unequal

or mean tone temperament, a *small* semitone has $4\frac{1}{2}$, a *large* one 7, and a tone 12 per cent. more vibrations. These numbers are very convenient for rough estimations.

The old French foot is 6 per cent. longer than the English, hence the one-foot pipe will be a semitone lower than the English, or about 443 to 446 vibrations. I have not met with a case of a French organ with A 443, or the one-foot pipe on A. But *Mersenne*, 1636, places the one-foot pipe on G, and this gives mean-tone A 496 and C 593. Now the St. Jacobi organ had actually A 491 and C 584 (equal temperament, making the C lower), as determined by forks tuned to the pitch and then measured. Hence, Mersenne's pitch, which even M. Cavaillé-Coll considered must be a mistake, actually exists at the present day.

2. The B flat foot organ, or B flat 472 to 475. This gives A 442, C 528, on the mean-tone temperament, that is, actually the pitch desired by the Society of Arts and not attained. This pitch was used by *Thomas Harris* in the Worcester Cathedral organ of 1666, by *Berhard Schmidt* (or Father Smith, as he has been called), in Durham Cathedral, 1683, Hampton Court, 1690, St. Paul's Cathedral, 1694-7, Trinity College, Cambridge, 1708, as I have ascertained, and probably in all his organs. It seems to have been occasionally used by the *Jordans*, who seem also to have built an A foot organ; but my inquiries are not yet complete. It is the favourite pitch of modern English organ builders, as I have ascertained by measuring the pitch-pipes of seven of the principal builders in London, which vary from C 524 to 528, at 60° F., to which all pitches are reduced.

3. The B foot organ, or B 472 to 475. This gives in England A 422 to 425, and C 506 to 512. This pitch was in general use, from at least 1700 to 1820, over England and over Germany. I found it in *Renatus Harris's*, All Hallows, Barking, 1675-7; St. Andrew Undershaft, 1696; and St. John's, Clerkenwell (date unknown); in *Harris and Byfield's*, St. Mary's, Shrewsbury; in *Byfield, Jordan, and Bridge's* two Great Yarmouth organs, 1733-40; in *Byfield and Green's*, St. Lawrence, Reading, 1771, and St. Mary's, Islington, 1772; in *Glyn and Parker's*, All Hallows the Great, Thames Street, 1749; in *Schnetzler's*, German Chapel Royal, St. James's Palace (date uncertain); in *Green's*, St. George's Chapel, Windsor, 1790; Winchester College Chapel, 1780; St. Katherine's, Regent's Park, 1778; and Kew Parish Church (date unknown). Glyn and Parker built the organ which Handel gave to the Foundling Hospital, 1750, and Handel, after conducting a performance of the "Messiah" there, in 1751, left his tuning-fork behind him. This fork is now in the possession of Rev. G. T. Driffield, Rector of Bow, and shows A 423, which is presumably the pitch of that organ. Mozart's clavier-maker, Stein, at Vienna, 1780-90, used a fork one vibration lower, A 422, which was undoubtedly the pitch of Haydn and Beethoven, and hence of Church music generally. It is a quarter of a tone flatter than French pitch. This was the pitch used when the Philharmonic Society was started in London, 1813, and was retained to 1826. Silbermann's organ at the Roman Catholic Church, Dresden, was about a comma flatter, or A 415.

4. The C foot-organ, or C 472 to 475 and A 495. The only instance known to me in England is Trinity College, Cambridge, as recorded in 1759 by the celebrated Dr. Robert Smith, its master, in his "Harmonics." But this was after its pitch (which was originally that of a B flat foot-organ) had been lowered a mean tone, by shifting the pipes, which, as he tells us, made it agree with the Roman pitch-pipes of 1702. But the French foot being a semitone flatter than the English, the Versailles B foot-organ (1786) had a pitch of A 396, C 474, as shown by the fork preserved in the Conservatoire in Paris, and hence precisely agreed with the altered Trinity College organ and the Roman pitch-pipe. Delezenne, in 1854, was fortunate enough to find an old dilapidated organ at the Hospice Comtesse, near Lille, which gave C 448, as near as he could measure, agreeing well with C 443, the calculated pitch of the French C foot organ.

This seems to be the first attempt at systematically finding the pitch of organs. The pitch of the pipes was in all cases found, when they could be actually heard, by beats with tuning-forks made for me, to the extent of an octave, on the basis of Scheibler's 256, 435, 440 (which I have reason to believe perfectly accurate), by Valantine and Carr, 76, Milton Street, Sheffield, and I have also reason to believe that these latter forks are not more than half a vibration wrong with Scheibler in any case. But before my complete paper is ready I shall have verified them by eighteen other forks of Scheibler now being very carefully copied at Crefeld. To hear the beats I stand thirty or forty feet away from the organ, and hold the fork over a resonance jar tuned to its pitch by pouring in water. The bellows is first filled, and no pumping is allowed during the ten seconds that I count. The beats are beautifully distinct, and I consider the result to be correct within one-fifth of a vibration.

The correction for temperature, which is most important (as at C 500 it is more than half a vibration per degree Fahr., to be added for higher and subtracted for lower temperature), is found by the following rule:—Add four per cent. to the number of vibrations observed, divide result by 1,000, and multiply by the number of degrees required. I have thus harmonised measurements made between 73° and 45° F.

The rule for finding pitch from measurement was given by M. Cavaillé-Coll (*Comptes Rendus*, 1860, p. 176), and, reduced to English measures, is as follows:—

Let L be the length, in English inches, of an open flue cylindrical metal diapason from the lower lip to the open end, and D its internal diameter, also in inches. The latter measure is frequently difficult to make, on account of the jagged, or "coned," or compressed, extremity. Then use the outer circumference, by wrapping a piece of paper round the pipe where it is truly circular; calculate the diameter as $\frac{7}{22}$ circumference, and throw off $\frac{1}{20}$ inch for the thickness of the pipe, to find D, which has to be known with considerable accuracy.

Let V be the number of double vibrations in the pipe, at 60° F., then

$$V = \frac{20080}{3L + 5V}.$$

I tried this formula with a whole octave of pipes at Green's St. Katherine's organ, and found that the error rarely reached one comma (or 1 in 80), which many persons can't hear, and never reached two commas (or 1 in 40). Since a quarter of a tone is 3 per cent. (or 1 in $33\frac{1}{3}$), and a semitone is 6 per cent. (or 1 in $16\frac{2}{3}$), this gives a far better knowledge than we can obtain by ordinary estimation of ear, without counting beats by measured forks.

It would confer a great favour on me if any one could give me these dimensions of old, unaltered organ pipes for the pipe which is nearest to twelve English inches in length, anywhere, especially abroad, naming the place and the note, and, if possible, date and builder, or would point out any existing unaltered old organs.

ALEXANDER J. ELLIS

25, Argyll Road, Kensington, W.

The Formation of Mountains

MR. ALFRED R. WALLACE asks one of our "great" physicists to enlighten us about the possibility of the interior of the globe "cooling more rapidly than the crust." If he will turn to a chapter on Conduction in such a work as Maxwell's "Theory of Heat," he will find an explanation of the principle. At p. 247 is a passage especially relating to the loss of heat by the earth.

But perhaps even a little physicist may help our great naturalist as the mouse did the lion.

In the first place it is of course understood that whenever it is said that "the interior of the globe cools more than the crust," it is not meant that it ever becomes cooler than the crust, but only that the interior, from age to age, goes on getting cooler than it was before, whilst the crust keeps at nearly a constant temperature.

An illustration, which I think gives a good idea of this process, may be taken from the dispersion of a crowd of persons in the street. Suppose each person to represent a certain quantity of heat. Then the number of persons in any space may be considered to represent its temperature, so that the crowded part will represent a very hot space. As the people disperse they move off the more quickly the further they get from the dense mass.

Now draw two lines near together across the street at some small distance from the densest part of the crowd, and let the space between these two lines represent the crust of the earth, while the space occupied by the crowd represents the earth's interior, and all beyond the outer line represents infinite space. Then the number of people passing outwards between the two lines at any particular moment will represent the quantity of heat in, and so the temperature of, the crust. At the

same time the number of persons remaining in the crowd will represent the quantity of heat in, and so the temperature of, the interior. Then it will be obvious that as the crowd disperses the number of persons at any one time between the lines may continue about the same (although the individuals will be changed), whilst those in the central crowd become fewer and fewer. This illustrates how the temperature of the crust may continue nearly uniform in spite of the continued loss of heat from, and cooling of, the interior.

I believe that I have long ago proved that the mere cooling of a solid earth would not give the amount of contraction needed to account for the observed inequalities of the surface, and I surmise that a diminution of the earth's volume has been caused by the escape of steam and gases from volcanic vents during past ages. This view has, however, attracted more attention in America than at home. O. FISHER

Harlton, Cambridge, December 13

Magnetic Storm, May 14, 1878

I AM inclined to think that Mr. Mance's observations (vol. xix. p. 148) upon the earth currents observed at Kurrachee must be incorrectly reported. To agree with the observations in China, Stonyhurst, Greenwich, and Haverfordwest, they should have commenced at 4 A.M. on May 15, and terminated at 5 P.M. on the same day (Kurrachee time).

It is a pity that electricians do not record these currents in absolute units. To say that the current was equal to fourteen Daniell cells means nothing unless the resistances present are also given. If an earth current is observed upon a cable it is easy to reproduce this current upon the same galvanometer with a known resistance and a known electromotive force, and then to express its value in webers or milliwebers. Thus if at Kurrachee 50° were noted on a galvanometer, and one Daniell cell reproduced this deflection through a total resistance of 125 ohms, then the current would be equal to $\frac{1}{125}$, or ·008 weber or 8 milliwebers, a magnitude which every electrician would understand. Moreover, if the length, resistance, and general direction of the cable or wire were given, as well as the direction of the current itself, the difference of potential of the earth at the two ends would be known. This if the cable were 246 miles long, and lay due east and west, and its resistance were 5ω per mile, then in the above case

$$\frac{E}{1230} = \cdot 008$$
$$E = 9\cdot 84 \text{ volts},$$

which is the difference of potential of the two ends.

If simultaneous observations were made in this way at numerous stations on the earth's surface, we should be able to plot out the distribution of potential on the globe, and arrive at some better knowledge of the cause of earth-currents than we have at present. W. H. PREECE

December 20

The Derivation of Life from the North

ATTENTION has been called by the President of the Royal Society to the labours of Mr. Dyer, as pointing in the case of plants to the conclusion that their various forms have been developed and dispersed from the north. I presume it is recognised that similar conclusions have been arrived at by Mr. A. R. Wallace in the case of animals. Mr. Wallace points to the palæarctic region as the great centre of their development or creation. On reading "The Geographical Distribution of Animals" when it first appeared, I was so much struck with the evidence adduced, that I was tempted to write and ask him if his work might not be said to occupy the following position in the history of unravelling what was formerly the mystery of geographical distribution. Mr. Darwin and others, including Mr. Wallace himself, had found a causal nexus in the case of islands, had shown that the faunas of islands had been derived from that of the nearest mainland, and in a character and degree varying concomitantly with the degree of their present disconnection therewith. They had thus completed the necessity for "centres of creation." Did not "The Geographical Distribution of Animals" afford the requisite evidence for carrying this commencement to its logical conclusion: for showing that in their turn the great continents themselves, or, more precisely, those which are outlying to the central mass (which is in the north, around the Pole), have a similar dependence, and have borrowed their own faunas from that northern mass, in a character and degree proportional to the dates and degree of their connection or separation from it, the islands might then be said to be the satellites, and the great zoological regions the planets of this system, all having borrowed their life directly or indirectly from a *single* "centre of creation."

To render this still clearer to my own mind I had a map of the world designed on a polar projection, the northern hemisphere being projected to somewhat beyond the southern tropic. By this means the manner in which the land surface of the globe is built around the pole is clearly seen, and the extremities of America, Africa, and Australia, extending into the great oceans of the world, are embraced, or nearly so. When the subdivisional regions (zoologically) of each of these great projections, and of the whole, are marked in colours, a succession of zoological strata, to speak rather inaccurately, appears. By carrying an ideal section from the supposed centre of creation in the north through either of these three great extremities, and from thence to the nearer, and afterwards the more remote, dependencies of those extremities (remote not in point of actual distance, as in degree of connection), we pass in each case through zoological strata of different types, until we arrive at those where no land-mammals are to be found at all. And this succession in space, as evidenced by geography, corresponds in a rough way with the succession in time, as revealed by geology. 1. As we recede in distance we meet with increased dissimilarity. 2. This dissimilarity partakes of a recession in type. 3. Some of these geographical districts seem to have their counterparts in geological periods. The Ethiopian region, as Mr. Wallace shows, presents us with the exiled miocene fauna of Europe in the most striking manner. Eocene forms may be seen in its dependency of Madagascar, or in the West Indies. Highly isolated Australia with its marsupials, &c., appears as if it were still in the secondary age. Oceanic islands, such as New Zealand, with a more beautiful climate, and more extensive surface than Great Britain, give us no land mammals at all. In others the reptiles "possess the land."

Mr. Wallace's plan is an excellent illustration of the comparative method, and shows how a careful classification leads to the solution of historical questions connected with the causes of that classification. Those causes are in this case comprised in the inference that a succession of waves of life has been propagated from the north, not all of which have had an equal extension, nor all encountered similar modifying circumstances.

If these inferences are not correct, perhaps Mr. Wallace would kindly set me right. J. W. BARRY

1, Duncombe Place, York

Glaciation of the Italian Lakes

HAVING spent some time lately on the border-land between Switzerland and Italy, it has occurred to me that a note on some glacial features of that region may prove of interest to the readers of NATURE.

The Lake of Lugano is a rock-basin. I believe it to have been scooped out by the glaciers which have formerly descended from the Alps. Of this there is abundant proof. The crystalline rocks in their lower reaches possess the easily recognisable outlines of *roches moutonnées*, but the stratified mesozoic rocks have lost these characters. Above Lugano and Agno these features are very well marked, and in these localities striation is tolerably frequent, the direction of the striæ being southerly. Along both sides of the southern extension of Mount St. Salvatore to Moreate, striæ can be seen in a few places near the lake-level, and the same is the case on both the Pianbello and Generoso shores. At the southern extremities of the lake are abundant moraine-mounds. Erratics are also present, most being gneissose or granitic, but a few have fallen upon the moving ice from nearer localities, as they are of dolomite. The moraine masses are cut through by the northerly flowing streams, but, after passing the parting between the waters flowing towards Lake Lugano and those running into Lake Como, there is the appearance of great destruction of the moraines. Unfortunately I had a mere cursory glance down the Val della Tresa, through which the drainage of the lake flows to Lago Maggiore. It has often been remarked that in this South Alpine lake district, the *débris* left by the glaciers is exceedingly small when contrasted with similar regions north of that moun-

tain chain. To every observer, indeed, this must be obvious. Two reasons have been adduced in explanation—1. That the glaciers have been smaller from the cold of the glacial period not having extended over Italy, as indicated by the absence of the reindeer and other sub-arctic species from the drift; 2. That denudation has been enormous. In spite of the southern exposure it hardly appears probable that there could have been sufficient difference in the size of the northern and southern glaciers to cause this contrast, as long as the glacial period lasted, for the vapour-laden winds from the sea must have deposited much of their moisture on the southern slopes as snow. But, as the ice-age passed away, the southern aspect of the Alps would doubtless be freed from its influence sooner than the northern, and hence, while snow and ice reigned in the latter area, the regions south of the mountains underwent denudation, probably, for a vast period. I would therefore suggest that the comparatively small amount of glacial *débris* on the southern side of the Alps may arise from that region having been exposed for a much longer time to sub-aërial waste, and in particular to the floods caused by the more rapid melting of the snows on the southern slopes of the mountains. GEO. A. GIBSON

10, Old Square, Birmingham, December 16

Electric Lighting

IN the *Proceedings* of the Philosophical Society of Manchester Mr. Wilde has described a new electric lamp, in which the carbons are placed parallel to each other, as in a Jablochkoff candle, but without any insulating material. It may not be apparent to all why the arc always locates itself at the extremity of the pair of carbons, and this, as Mr. Wilde observes, whether the lamp be erect or inverted. The explanation is that the current in each carbon repels the electric arc exactly as the current in the mercury-troughs repels the connecting cross-wire in a well-known experiment of Ampère. In Wilde's candle we have two fixed conductors, the carbons, and a movable conductor, the arc. Since the current in the arc and the current in either carbon are one from the other towards the point of junction of that carbon with the arc, the arc must be repelled by the carbon. One cannot but be surprised that Ampère's experiment did not suggest the discovery made by Mr. Wilde, that the insulating material might with advantage be omitted in Jablochkoff's candle. J. HOPKINSON

4, Westminster Chambers, December 12

The Reproduction of the Eel

THE time of spawning and the differences between the sexes of the common eel has been hitherto unknown. Last year for the first time in this country eels (*Anguilla bostoniensis*) containing eggs were found, December 31, by Mr. Edwards, at New Bedford, Mass., as stated by Mr. Putnam in the *Proceedings* of the Boston Society of Natural History. From observations on eels brought me at Providence, R.I. it appears that eels spawn here in salt or brackish water from October to the end of November, as I have found several eels containing eggs which seemed nearly ripe, the ovary being full and large. To-day on examining the testis of a male eel 17 inches long, the mother-cells and sperm-cells were found, the latter numerous and lively, from $\frac{1}{3000}$ to $\frac{1}{20000}$ inch in diameter. The eggs were white, just large enough to be distinguished by the naked eye, measuring a little less than $\frac{1}{2}$ mm. in diameter. The females are larger than the males, with the belly white, while the males are easily distinguishable by the dark bellies and a narrow silvery or golden median stripe. A. S. PACKARD, Jun.

Laboratory of Brown University, U.S., November 27

AERATED BREAD

SOME remarks upon aerated bread which were made by Dr. B. W. Richardson at the recent general meeting of the company working Dr. Dauglish's patents require examination. Now it is noteworthy how imperfect our knowledge of the chemistry of the mill-products from the cereal grains still remains. Without such knowledge we are not in a position to dogmatise as to the exact nutritive values of different kinds of bread. As I pointed out in a previous article on "Real Brown Bread,"[1] the statement that whole wheaten meal, bran, pollard, &c., contain more nitrogen, and therefore more flesh-formers than fine flour, rests upon no certain basis of analytical fact. And if it were proved that all the nitrogen of the most nitrogenous of mill-products does really exist in what are called albuminoids or flesh-formers, we cannot ignore the presence of much indigestible fibrous material in bran and pollard, material which is not only non-nutritive itself, but which locks up in an inaccessible form much of the real nutrient substances associated with it. Thus a sample of wheaten bran, or rather, fine pollard, may refuse to give up to the boiling dilute acid and alkali used in fibre-determinations more than six-sevenths of its nitrogenous matter; and it can hardly be expected that the secretions of the alimentary canal will prove successful in withdrawing a larger proportion. Indeed, the analysis of the residues of such foods after having been submitted to the digestive process, has confirmed this expectation in the case of the human subject. Moreover, while a not inconsiderable part of the albuminoid matters of the outer coverings of the grain thus escapes digestion from its mechanical condition, there is good reason to believe that a further portion remains unabsorbed, by reason of the rather hurried passage of the branny particles through the digestive tract. And the same causes which operate to prevent a part of such flesh-formers as exist in the bran from being utilised, affect also and in a similar way the useful mineral substances which abound in the coarser mill-products, as well as the oil or fat which they contain.

Now let us see what are the distinctive advantages claimed for Dr. Dauglish's bread-making process by Dr. Richardson. It yields a bread which is said to be "perfectly clean, perfectly wholesome, and completely nutritious." As to the perfect cleanliness of this mechanical process for making bread there can be no question; it is immeasurably superior to the barbarous and old, but, as Dr. Richardson remarked, *not* "time-honoured system of kneading dough by the hands and feet of the workman." And we may agree, with almost equal confidence, in the statement that aerated bread is perfectly wholesome. The stream of pure water charged with carbonic acid gas vesiculates the dough, which has required neither alum, nor blue vitriol, nor lime-water, to check the irregular fermentation, and neutralise the sourness of mouldy or otherwise damaged or inferior flour. But, on the other hand, the adoption of the aerating process does not of itself necessarily exclude *all* adulterations of the bread: materials to whiten the loaf and to cause the retention of a larger percentage of water may still be used. As to the small loss of nutrient materials involved in the ordinary fermentation of dough, it hardly merits consideration. Perhaps Dr. Richardson alluded to it because it gave him an opportunity of having a fling at his old enemy, alcohol, of which it has been found that a newly-baked loaf, made by means of yeast, contains about 0·25 per cent. So that a man who eats twenty quartern loaves has therein consumed an amount of alcohol which is commonly contained in one bottle of port! But if there be no really serious loss of starch by conversion, first into sugar, and then into carbonic acid gas and alcohol, there can be no doubt that a number of altered products are present in a fermented loaf, and that these are less abundant and less variable in nature in aerated bread. But the presence in fermented bread of larger quantities of sugar, gum, and soluble starch than are found in aerated bread is not usually a disadvantage so far as the digestibility of the loaf is concerned. It is rather to the production of lactic acid and of nitrogenous ferments by the use of yeast or leaven that we should attribute the uncertain value of ordinary bread. The quality of the yeast, too, must not be left out of consideration, as some of our home and of our imported supplies are by no means of a satisfactory character.

[1] See NATURE, vol. xviii, p 229.

I may now discuss the third meritorious feature which Dr. Dauglish regarded as a conspicuous advantage possessed by his process—a view which is now endorsed by Dr. Richardson. We are told that "in brown bread, which contains the envelopes or coverings of the whole grain, the flesh-formers amount to 10 per cent., and that, while it is possible to live upon brown bread, without any other food, the health suffers, and death finally ensues, on white bread alone." "The flesh-formers in white bread amount," we are told, "to 7 or 8 per cent." Now by the use of the aerating process, a light and palatable loaf may undoubtedly be readily made, either from the entire meal of crushed wheat, or from such meal from which nothing but the long or coarse bran has been excluded. Such a result cannot be secured with any degree of certainty when yeast is used. This is quite true. But it is not by any means an ascertained fact that brown bread or whole meal bread contains a larger proportion of flesh-formers than white bread from the same wheat. If we make no deduction on account of insoluble and inaccessible flesh-formers in the coverings of the grain, we shall have to make a very considerable deduction on account of nitrogenous matters which are not really albuminoid or flesh-forming, and which are extensively present in the coats which form the main substance of pollard and sharps. We can hardly attribute flesh-forming properties to the "diastase," "cerealin," and other obscure nitrogenous ferments which we know to abound in the coarser mill products. Now these are the very bodies which the aerating process enables us to include in our bread without making it heavy, the very bodies whose presence contributes most largely to raise the percentage of assumed flesh-formers, from the 7 or 8 per cent. in white bread to the 10 per cent. in real brown bread. Now, although we have no absolute method of distinguishing between the true flesh-forming nitrogenous bodies in food-stuffs and those whose value is at the best problematical, yet the "carbolic acid process" which I devised in 1873, and which has latterly been attracting a good deal of attention, furnishes some instructive results when applied to the various mill products of the cereals. For instance, I found (in 1875) that pearl barley contains at least 92 per cent. of its nitrogen in the form of genuine coagulable albuminoids, but that "fine dust," consisting of the richest parts of the barley grains, though it contains three times as much nitrogen as pearl barley, does not contain twice as much unmistakable flesh-forming substance. A similar observation was made on comparing the coverings of the wheat grain with the flour—93 parts out of every 100 of nitrogen in ordinary wheaten flour being certainly in the form of flesh-producers, while the proportion in the entire bran sinks to an average of 72, and sometimes touches a much lower figure. The same result has been obtained with a large number of food materials, in some succulent vegetables the albuminoid nitrogen not exceeding one-fifth of the whole.

It is evident, from the preceding considerations, that whatever be the nutritive or medicinal merits of whole-meal bread, it cannot be definitely stated to be a much richer food so far as flesh-formers are concerned, than white bread made from the flour of the same sample of wheat grain. If we deduct from the supposed 10 per cent. of flesh-formers in whole meal bread $\frac{1}{5}$ or 2 per cent. because of the existence of non-albuminoid nitrogen in the branny particles, and because of the indigestible condition of a small part of the true albuminoids, we leave but 8 per cent. a proportion which does not greatly exceed the 7 or $7\frac{1}{2}$ per cent. usually attributed to white wheaten bread.

On some future occasion I may have a few words to say, should the Editor of NATURE accord his permission, as to the bread question treated of in the "Dietaries of Prisons' Report," lately presented to Parliament.

A. H. CHURCH

ON THE COMBUSTION OF DIFFERENT KINDS OF FUEL

OUR attention has been called to this subject on perusing a paper read before the American Institute of Mining Engineers. In this communication it is attempted to be proved, that the manner in which charcoal unites with oxygen in iron furnaces, explains its alleged superiority over mineral coal.

The question appears to us to involve considerations outside the mere technical limits of iron smelting. We therefore submit the following remarks, which have been prepared at our request, by a gentleman whose important researches on the economic application of fuel in the smelting of iron are known to all the world.—ED. NATURE.

Prof. John A. Church, of Columbus, states, in a recent publication, "that it is a well-known fact that under similar conditions a ton of pig-iron can be made from any ore with less fuel when charcoal is used than when coke or anthracite is employed for heating." He then discards, as untenable, all former explanations of this doctrine proceeding either from scientific or practical men, and maintains that "the highest carbon duty is given by the fuel which withdraws the most oxygen from the blast in a given time."

The professor afterwards proceeds to explain that charcoal, being highly porous, presents a more extended surface to the action of the oxygen than the more compact forms of carbon as it exists in coke or anthracite. He then considers the effect of what is designated in the paper as diluted oxygen on its way to the upper regions of the blast furnace.

In this latter statement with regard to free oxygen, it seems to be overlooked that whatever difficulty this gas may have in attacking dense carbon, there can be no reason why carbonic oxide, generated from coke or anthracite, should not be as easily burnt to the state of carbonic acid as the same carbonic oxide is burnt when formed by the combustion of charcoal.

If it could be shown that all the carbon consumed in the hearth of the blast furnace were in the condition of carbonic acid, then, no doubt, free oxygen might be expected there, but in the presence of so vast a volume of inflammable carbonic oxide in that region such a condition of things is scarcely possible.

It is unnecessary, however, to occupy space with merely speculative matter when actual analyses inform us that in a furnace using coke, not only is there no free oxygen at a very short distance from the tuyeres, but that there is no carbonic acid. In point of fact the whole of the gaseous carbon exists there in the form of carbonic oxide.

Admitting, then, the highest carbon duty to be dependent on the law laid down in the paper, it is clear that at a distance of two or three feet from the point where the blast enters, the conditions in this respect of a furnace burning charcoal or coke are the same.

Prof. Church quotes, in the course of his observations, the results obtained from two furnaces, one using charcoal the other anthracite, and from these he infers that there is an "inherent difference" between the two kinds of fuel of 900 lbs. per ton of iron in favour of charcoal. This figure is a mighty one when it is considered that the charcoal actually consumed for each ton of pig is under 1,800 lbs.

Somewhat inconsistent with this conclusion is the instance quoted of a Lake Champlain furnace, where the consumption of anthracite has been reduced nearly 25 per cent. below that quantity which formed the basis upon which the "inherent difference" was estimated.

The paper further recommends as a reasonable consequence of the superiority of charcoal over coke, that iron manufacturers should, as far as possible, imitate the physical structure of the former in the produce of their

coke ovens. Our "perversity" in preferring hard to soft coke is condemned, and we are invited to put reason in the room of traditions which have not "one recorded fact to sustain them."

My attention, as an iron-smelter, was drawn to the very small consumption of charcoal in a certain instance quoted by Prof. Tunner, of Leoben, and being unable to reconcile his experience with my own observations, I obtained from this respected authority further confirmation of the exactness of his figures. Along with this he kindly repeated his analyses, including one not given in his original paper, viz., that of the gases taken on leaving the furnace, in other words, after they had *completed* their work.

Other investigations prevented my examining the question at the time with that attention it deserved, and I concluded, upon other grounds than those advanced by Prof. Church, that there was some actual difference in the mode in which charcoal effected the reduction of at least *some* ores.

In this I now think that I was mistaken, and I will endeavour to prove that the author of the paper commits a grave error in comparing two blast-furnaces working upon different kinds and quantities of ores and flux used in the production of pig-iron. I will also attempt to demonstrate that the use of hard coke, held to with such "singular perversity," can be maintained by "reason" as well as by "tradition."

Carbonic oxide, as is well understood, is the chief reducing agent in the smelting of iron, and for our present purpose we may regard it as the only substance which deprives the metal, as it exists in ores, of its oxygen. The carbonic acid which is formed by this deoxidising action possesses a tendency, at certain temperatures, diametrically opposed to that of the lower oxide of carbon—it reoxidises the iron.

This conflicting property of the carbonic acid is kept in check, in iron smelting, by the presence of carbon in the form of its lower oxide, being about double that which is present in its highest form of oxidation.

When I came to examine the ratio in which the carbon in these two conditions existed in the escaping gases, as described privately by Prof. Tunner, I observed that they agreed very nearly with that which I imagine constitutes, in furnaces using coke, a state of equilibrium between the gases and the ore in the process of reduction.

Now no one will dispute that when two pounds of carbon are burnt to the state of carbonic oxide and one pound to the form of carbonic acid the quantity of heat is the same, from whatever source the carbon itself may be derived.

If, after having determined a perfect resemblance between the extent to which the fuel has been oxidized, in the furnace, when using charcoal and using coke, we find that there is no difference in the amount of heat wasted, it is time to consider whether the pig-iron itself, obtained by means of these two kinds of fuel, may not require very different quantities of heat for its production.

It may be here stated that the Cleveland furnaces in England, with which I compared those of Carinthia, were consuming 21 to 22 cwts. of coke against 14 or 15 cwts. of charcoal used in the latter per ton of iron. The weight of the slag produced in the former was very much the larger of the two. The additional heat absorbed in fusing this substance was further augmented by that required for the decomposition of silica and phosphoric acid, the bases of which are found more largely in Cleveland than in Carinthian pig. The diminished quantity of fuel burnt in the charcoal furnaces reduces very greatly the weight of gases, and this consequently lessens the amount of heat carried away through their instrumentality.

The following figures exhibit the number of centigrade heat units required, according to my calculations, per unit of each kind of pig-iron referred to.

	Cleveland iron.	Carinthian iron.
Evaporation of water in fuel	15	15
Reduction of oxide of iron and dissociation of carbonic oxide	1730	1730
Expulsion of carbonic acid from carbonate of lime	250	25
Decomposition of this carbonic acid	265	25
,, water in blast	130	75
,, silica and phosphoric acid	200	50
Fusion of iron	330	330
,, slag	830	330
Loss by radiation	270	200
Carried off in gases	440	210
	4460	2990

Now when the proportions between the heat requirements and fuel-consumption come to be examined, we find that the two sets of numbers agree almost exactly; for—

$$4460 : 2990 :: 21 : 14{\cdot}08$$

Since writing on this question, nearly seven years ago, I have had frequent opportunities of examining the performance of charcoal-furnaces in Sweden, Italy, and Spain, as well as in Virginia, Kentucky, Ohio, Missouri, and Michigan, in the United States. I soon became aware that different kinds of ores demanded very different quantities of charcoal for their treatment. In some cases the weight was nearly as low as that consumed in Carinthia, in others it was quite as much as if coke had been the fuel employed. I failed on every occasion to detect any circumstance beyond the change in the ore itself, or in the nature of the iron it afforded, to which I could ascribe the difference in question. All doubt, however, on the supposed difference of value between the vegetable and mineral fuel was dispelled on receiving from the owners of works in the North of Spain and in Virginia, statements from their books exhibiting the smelting of the same ores with the two kinds of fuel. The difference in these was often in favour of the coke, but such difference as existed could easily be accounted for by fluctuations in the amount of foreign matter in the fuel, or in small changes in the quality of the ores.

It only remains for me briefly to mention the grounds upon which, for chemical reasons, hard coke may be superior to that softer variety recommended by Prof. Church.

Carbonic acid, as is pointed out in the paper by this author, possesses the power of dissolving carbon, and every pound so dissolved involves a double loss. There is a reduction of temperature where the action takes place, and there is a pound less of fuel remaining to be burnt at the tuyeres. I ascertained, however, by direct experiment, that all forms of carbon are not equally easily affected by carbonic acid, that in hard coke being capable of resisting the solvent action much better than the same carbon as it occurs in soft coke. The importance, therefore, of employing our fuel in that condition where it is least susceptible of being oxidized in the region of the furnace where its combustion is useless, need not be further insisted on.

I am ready to admit with Prof. Church, that the traditional opinions of practical men have often required correction from scientific investigation, but upon the present occasion, so far as I have been able to judge, the ironmaker and the chemist agree in the relative value of hard and soft coke. I. LOWTHIAN BELL

PAPUAN HERPETOLOGY[1]

In this memoir, which forms a portion of the thirteenth volume of the *Annals* of the Civic Museum of Genoa, we have by far the best account of the herpetology of

[1] "Catalogo dei Rettili e dei Batraci raccolti da O. Beccari, L. M. D'Albertis e A. A. Bruijn, nella sotto-regione Austro-malese." Por W. Peters e G. Doria. *Ann. del Mus. Civico*, Genova, vol. xiii., 1878.

the great Papuan subregion, that has hitherto appeared. At the same time the authors have not attempted to make a perfect faunistic work of it. Only such species as are represented in the collections made by the Italian travellers, Beccari and D'Albertis, on their various joint and several expeditions, and in the collections sent to Genoa, by Herr Bruijn of Ternate are enumerated in the list. On the other hand the mass of these collections is so great—consisting of 3,000 examples from 44 different localities, and the series thus brought together is so much more nearly perfect than that which any other authors have had before them—that the result has been to give us an excellent idea of the general character of the reptiles and batrachians of this division of the earth's surface. Let us, therefore, look through the pages of Messrs. Peters and Doria's excellent memoir, and see what general views we can obtain from it as to the peculiarities of this little-known branch of the Papuan fauna.

In the first place, land-tortoises are few in the Papuan sub-region. Our list contains but two species, one from Amboina and Celebes, the other from the southern extremity of New Guinea, and very doubtfully distinguishable from an Australian species.

Crocodilians are also scarce. The series contained examples but of one species—*Crocodilus porosus*—which extends from India into Northern Australia.

Of lizards, besides the monitors, of which genus not less than 16 species are contained in the list, the great mass are Skinks, Geekos and Agamids. As in Australia the Skinks are especially numerous, upwards of 30 species being represented in the series, of which 8 are now described for the first time.

The ophidians are also well represented in the Papuan fauna, the series examined by our authors affording examples of 54 species. Amongst the Boas *Liasis* and *Chondropython* are characteristic Papuan forms, whilst the Australian *Morelia* also extends into New Guinea. But no greater sign of the essential unity of the Australian and Papuan faunas can be shown than the presence in New Guinea of the three Elapine genera, *Diemenia*, *Pseudechis* and *Acanthophis*, all characteristic forms of the Australian continent. At the same time the presence of an *Ophiophagus* in New Guinea, proves that an intruding element from the north has reached thus far.

About 20 batrachians close the list, amongst which *Limnodytes* and *Hyla* seem to be the most prevalent genera. The Australian *Pelodryas cærulea* is abundant all through the sub-region. A new and very singular form obtained by Beccari in the interior of New Guinea, near the river Wa-Sampson is characterized as *Sphenophryne cornuta*, and a second, obtained, by the same explorer on Mount Corfak as *Xenobatrachus ophiodon*. The latter shows some points of affinity to the Australian *Myobatrachus*.

Seven excellent lithographic plates illustrate some of the rarities described in this memoir, and two maps are added to show the exact localities where the collections were found. The maps we see with great pleasure, as in our opinion no zoological work of a faunistic character can in these days be considered complete without such an appendage.

THE METEOROLOGICAL SOCIETY LECTURES

A COURSE of six lectures on meteorology was recently delivered under the auspices of the Meteorological Society at the Institution of Civil Engineers. The purpose of these lectures, we believe, is to spread a knowledge of the known facts and principles of meteorology, and in this useful mission we cannot but wish the Society every success. We here give abstracts of two of these lectures, by Mr. J. K. Laughton and the Rev. W. Clement Ley. The subject of Mr. Laughton's lecture was—

Air Temperature, its Distribution and Range

After calling attention to the importance of climatic knowledge the lecturer dwelt on the fact that though all heat as affecting climate emanates directly or indirectly from the sun, temperatures have but little relation to latitude except when the distances are very great. He illustrated this by reference to isothermal and isabnormal maps, and went on to speak in some detail of the several causes of the disagreement between isotherms and parallels of latitude. Locally there is a very great difference between the temperatures of adjacent localities on account of the sunny aspect or sheltered situation of some as compared to others, as is found in an extreme degree in such places as the Undercliff of the Isle of Wight; but geographically, a cause of very considerable importance is the nature of the soil. The air over sandy or sterile ground is heated by direct contact and by radiation, to a degree far in excess of what happens to air resting on grass-grown or verdant plains; heat proceeding from an obscure source is unable to escape through the air, just as obscure heat-rays may be caught and accumulated in a closed conservatory or in a glass-covered box, so that the air may be raised to a very high temperature: several instances are on record of a temperature of 130° F. being observed under such circumstances. On the other hand, when the solar heat falls on ground, whether grassy or snow-covered, that will not easily part with it, the air may remain cool, or even cold; as is found in our every-day experience in summer of the pleasantness of a field path as compared with a high road; and as is shown more markedly by the great power of the direct rays of the sun in the Arctic, or at elevated stations in the Alps or Himalayas, whilst the snow is lying all around, and the temperature of the air is far below freezing-point.

But greater far than the effects of differences of soil are the effects of ocean currents, which warm the air to an almost incredible degree. Mr. Croll has calculated that the surface-water of the North Atlantic, if deprived of the Gulf Stream, would be reduced to a temperature very far below freezing-point; that the heat which the Gulf Stream disperses into the superincumbent air would, if converted into power, be equal to the united force of some 400 millions of ships such as our largest ironclads. This heat, thrown into the air, is wafted by the south-westerly winds over the north-west of Europe, and very largely over our own country. It is this that makes the extreme difference between the climate on this side the Atlantic and that on the other, that gives us green fields and open harbours during the winter, whilst in Labrador or Newfoundland they are buried in snow or choked with ice.

The carrying power of water is so great as compared with that of air that the climatic effect of winds heated by contact with hot earth is relatively small. The scirocco of the Mediterranean, a wind heated over the great African desert, has often been referred to as the "snow eater" of Switzerland. This has been proved to be a mistake. The snow-eating wind of Switzerland is a wind from the Atlantic, warmed by the Gulf Stream, and rendered dry and hot by the condensation of its vapour as it passes over the mountains. Similar winds have been observed in many different parts of the world—in New Zealand, in Norway, in Greenland, and in North America, where their peculiar dryness, carrying off all moisture, renders the grass so inflammable that the smallest accidental spark lights up a fire which may spread over a country, and is thus the true cause of those immense prairies which are a distinctive feature of North American geography. But such hot winds are quite distinct from such winds as blow from the Sahara, or the Stony Desert of Australia, or from many other sterile tracts of country; winds which are merely the escape of air heated to an extreme degree by contact with the burning soil. These hot winds are for the most part merely disagreeable; but cold winds are very often dangerous; in the North-Western States of America a cold wind, ushering in a violent snow-storm, caused the death of more than 300 people in January, 1873; and in many other localities, a cold wind bringing in a sudden fall of temperature through 40 or 50 degrees, is always a cause of grave anxiety. Our English "Blackthorn Winter" in April or May is only one, and a subdued instance of the ill-effects of such cold spells.

The presence of moisture in the air, by checking radiation from the ground by night, or during the winter, softens the rigour of the seasons, makes the summers less hot, the winters less cold. It is this that constitutes the difference between "insular" and "continental" climates; it is the want of the vapour-screen which causes "excessive" climates such as we read of

in the far east, where, as near Khiva, a summer of more than tropical heat is succeeded by a winter of Arctic rigour. In a very extreme degree the climate of Astrakhan contrasts with that of Fuegia, and yet the mean temperature of the two is about the same; but in the one the seasons are excessive, in the other the difference is but small. The difference in the products of the two countries is thus very great: on the one hand, plants requiring great heat, but able to withstand the cold, on the other plants of a more tender nature which can flourish with a very moderate amount of warmth; in the one grapes and corn, in the other fuchsias and veronicas.

In studying climate it is therefore necessary to observe not only the greatest heat and the greatest cold, but also the mean temperature. These can only be observed by means of thermometers, for personal feelings may be the effects of many other causes—of wind or evaporation, or state of health, or peculiarity of constitution, and are absolutely no index to the state of the air temperature.

The lecturer then proceeded to speak of the different kinds of thermometers, several of which were exhibited, and of the several stands for sheltering them. The Meteorological Society has decided positively in favour of the Stevenson stand, and directs its observers to record the temperature at 9 A.M. and 9 P.M., as well as the highest and lowest, as registered by the maximum and minimum thermometers. He then described some novel and ingenious contrivances for automatic registering, such as the "turn over" of Messrs. Negretti and Zambra, and the "chronothermometer" of Mr. Stanley, and concluded by pointing out that these instruments were but a means to an end, and that the study of climate was the study of nature in one of her most beautiful and most varied aspects.

Mr. Ley's lecture was on

Clouds and Weather Signs

The lecturer dwelt, in the first place, on the unsatisfactory condition of this portion of meteorology, as contrasted with those branches of the science in which instruments are employed. The great impediment to our progress arises from the fact that cloud-observation is, in large measure, an incommunicable art, requiring a special training of the eye. Specimens of the objects of study cannot be exhibited, neither is it possible, in illustration of the subject, to refer to types of cloud depicted in the well-known paintings of the best artists, because the latter, aiming at the production of atmospheric effects, employ the materials most easy to handle, which are commonly the least typical cloud-forms. The old classification and nomenclature of clouds is highly unsatisfactory, having been framed at a time when the relation of wind and weather to the distribution of barometric pressures was unknown; and with this relation the forms and movements of the clouds are intimately connected.

As regards configuration, clouds seem naturally divisible into two groups, those which arrange themselves in layers, whose vertical diameter is small as compared with its horizontal, and those which assume spherical or hemispherical shapes; and this division is related to certain physical conditions of the atmosphere and of the earth's surface beneath the cloud. It is, however, essential that we should possess some name or system of names to distinguish those clouds which are conveyed by the upper currents, and the term cirrus, with its compounds, must be more closely restricted to this class of clouds than has yet been done. From the use of weather-maps a new science of the winds has originated, on which all attempts at weather forecasting must be based. The movements of the upper clouds afford most valuable information concerning the distribution and movement of the areas of high and low barometric pressure. Rules by which this information may be interpreted, based in great measure on a former investigation by the lecturer,[1] are somewhat complex, and cannot well be given in a brief *résumé* like the present.[2] It may be sufficient to explain that in the front of an advancing barometric depression there usually extends a bank of the halo-producing cirro-stratus, the exterior edge of which is, roughly speaking, a parabola, the focus of which lies in the line about to be traversed by the centre of the depression. On the right-hand of the centre this bank or sheet is abruptly broken and is succeeded in the rear by local shower-clouds. On the left-hand the sky commonly continues overcast, but the cloud-plane gradually descends until little is to be seen but low stratus. A backing of the upper current takes place until after the centre of the depression has passed. In whatever direction a depression is advancing the same characteristic phenomena are observed. Thus changes in the clouds indicate to us probable alterations of wind and weather.

[1] "Relation between the Upper and Under Currents of the Atmosphere around Areas of Barometric Depression." *Quart. Journ.* of Met. Soc. vol. iii. p. 437.

[2] The lectures will shortly be published.

While the nimbus, which exists in the front of a depression, first makes its approach evident by changes in the higher cloud-strata, the process of nubification is the converse of this in those local showers which commonly occur on the right-hand and in the rear of a centre of depression, and therefore when the barometer is rising or just about to rise. These latter are developed in an upward direction through the formation of cumulus. The precipitation which occurs in them—always preceded by a change of appearance in the domes of cloud, which assume a soft and cirriform aspect—is attributed to the neutralisation of electricities as the summit of the cloud passes into a higher region; but there are important differences of appearance between those cumuli which are likely, and those which are unlikely, to undergo this transformation,

A physical explanation is given of the ordinary weather signs derived from the colours of the sky, from "visibility," and from unusual refraction. Attention is invited to some peculiar types of cloud, especially to a very elevated turreted stratus, often erroneously termed cirro-cumulus, occurring with high temperature on the south-western borders of anti-cyclones, a forerunner of thunderstorms. The formation of the low-level stratus and of the fog which usually prevails in our winter anti-cyclones, seems to be due to a downward movement of the air at a time when the earth's surface is colder than the atmosphere at a slight elevation above it.

In the course of this lecture sketches of some of the more definite varieties of cloud, with arrows indicating the direction of the currents, and a diagram showing the movements of the upper currents, and the prevailing cloud-types around areas of depression, were exhibited by the aid of the oxy-hydrogen lime-light.

THE MOTION OF A LUMINOUS SOURCE AS A TEST OF THE UNDULATORY THEORY OF LIGHT

1. ALTHOUGH the undulatory theory of light may now be considered as completely established, still any confirming test of a physical theory is in itself interesting as a fresh illustration of a natural truth. Considering how at one time crucial tests of this theory were sought after, it would appear perhaps rather an anomaly that attention should apparently not have been given to the effect attendant on the motion of a luminous source as a test between the two rival (undulatory and corpuscular) theories, and that more especially as the test would appear to be in principle a simple and decisive one. I should have considerable diffidence in directing attention to this point, but no record apparently exists of experiments proposed or attempted with this view. It might be said that the possible existence of practical difficulties in the way of carrying out the test may account for this; but then practical difficulties are seldom allowed to stand in the way, if a theoretic *principle* be correct; and, unless a thing were seriously proposed and discussed, no attempt would be made to surmount the difficulties that might exist in the way of carrying it out. Sir John Herschel, as far as he touches on this point, would appear to have had the idea that such a test between the two theories could not, *in principle*, be applicable; for he says ("Outlines of Astronomy," p. 214), speaking of the effect attendant on the motion of a luminous body, "The effect in question, which is *independent* of any theoretical views regarding the nature of light, ... &c." It is true he mentions afterwards, in a foot-note, a difference in the effect in the case of the two theories as regards the *velocity* of light, in the case of a luminous source moving directly towards or from the observer; but the following fundamental difference in the case of the two theories appears not to have been taken into account.

2. For the sake of precision we will take a special example. Suppose a luminous source with such an adjustment as to emit a parallel beam of light,[1] and let the luminous source be supposed put in motion in a direction at right angles to the path of the beam. Then on the basis of the corpuscular theory (according to which light consists of projected particles) since the particles or corpuscles, according to the laws of motion, partake necessarily of the motion of the body emitting them; so, therefore, if the corpuscular theory were true, the path of the emitted beam of light would be exactly the same in direction as if the luminous body were at rest. So, therefore, if we imagine a screen of the same breadth as the luminous beam to be placed at a distance, so that this screen is *exactly* illuminated when the luminous source is at rest, then (according to the corpuscular theory) this screen would also be *exactly* illuminated when both it and the luminous source were put in motion with equal velocities (in parallel paths) in the same direction.

3. This, however, will not be the case if the undulatory theory be true, for it is a known fact that waves emitted in a medium do not partake of the motion of the body emitting them. For when once a wave has left the body, the wave is dependent solely on the medium for its propagation and is not influenced by the motion of the body one way or the other. It follows, therefore, that in the case of a moving luminous source emitting waves transversely to its path, the waves forming the parallel beam will be left behind, or will not partake of the motion of the luminous source. The waves will form a slanting track of light which will no longer strike *exactly* the opposed distant screen, but will fall somewhat to the rear of it. The luminous beam which, when the screen and source were at rest, was exactly eclipsed or intercepted by the screen, will (when the screen and source are in motion) commence to escape behind the edge of the screen, or the eclipse will no longer be total.

4. Here, therefore, we should have in principle a simple and decisive test between the two theories, provided insuperable practical difficulties do not stand in the way of carrying it out (for which object probably various methods would suggest themselves).

In order to contrast further the different effects that would be produced in the case of the two theories (corpuscular and undulatory), we may consider various possible cases of relative motion, also the effect when the beam is received directly in a telescope, or in the eye. We have already considered the case where the beam is observed objectively (by the use of a screen), which we may call *Case I.*

5. *Case II.*—We may now consider the case when a telescope[2] is used. We will take the above example of a luminous source in motion, emitting a parallel beam of light at right angles to its path, and we will imagine that this beam is received in a distant *stationary* telescope, placed normal to the path of the moving luminous source, so that the beam flashes down the axis of the telescope at the instant of the passage of the luminous source. Then we have to compare the effects produced in the case of the undulatory and corpuscular theories. On the undulatory theory, waves emitted by a luminous source do not partake of the motion of the source, so that at the instant when the wave of the beam (singling out a particular wave) flashes down the axis of the telescope at the moment of passage of the luminous source, the source will have already moved on a distance from the point where the wave left it, this distance representing that traversed by it during the time the light took to pass from the source to the telescope; and the source is therefore seen out of its true position by precisely that amount. This is perfectly evident, and the correction for this error in the estimate of position (of the value above indicated) constitutes the well-known "equation of light."

6. We have now to consider what takes place on the corpuscular theory. Here the projected corpuscles will partake of the motion of the source. Singling out, therefore, one of the corpuscles that flashes down the axis of the telescope at the instant of the passage of the luminous source; this corpuscle will possess the transverse velocity of the source that emitted it, and therefore the corpuscle will not pass straight down the axis of the stationary telescope, but in its passage will deviate laterally from that axis. The telescope would accordingly have to be inclined in order that the corpuscle might pass along the axis. This deviation of the corpuscle from the axis of the telescope will cause the luminous source to be viewed out of its true position, and it is easily seen that this visual error in the estimate of the position of the source on the corpuscular theory is precisely the same in amount as the previous error (due to a different cause) on the undulatory theory. Indeed, the error on the corpuscular theory is simply a case of "aberration" due to the relative motion of the telescope and light), and the correction for it, according to known principles, is the same as the other correction on the undulatory theory, termed "equation of light.'" It is a remarkable fact, therefore, that though the path of the light in its transit is very different in the case of the two theories, the visual error in the estimate of the position of the object is the *same*, so that this error cannot itself serve as a test between the two theories. There is, however, one marked distinction between the two theories; for while on the undulatory theory a position of the telescope *normal* to the path of the moving luminous source, causes the flash of the beam to pass down the axis of the telescope; on the corpuscular theory, on the other hand, the telescope has to be *inclined* in order that the flash of the beam may pass down its axis. Here, therefore, we have a definite physical effect serving as a point of distinction between the two theories.

7. *Case III.*—We will now take the case when *both* the luminous source and the distant observer are in motion, moving with equal velocities in parallel paths alongside each other in the same direction. Here on the corpuscular theory, in which case the corpuscles partake of the motion of the source, since the whole system therefore moves with equal velocity, the whole system will therefore be *relatively* at rest; so that the light will pass across and enter the telescope just as it would have done if everything were at rest, or there will be no peculiarity in the passage of the light whatever on the corpuscular theory. It is widely different on the undulatory theory, for here the beam of light passing between the source and the telescope will be left behind in the medium, and therefore in the first place, the moving telescope, in order to catch the parallel beam, will have to be placed back a certain distance in the rear; for since the light takes a slanting track between the source and telescope (the degree of slant depending on their common velocity), the telescope to intercept the light, can no longer be placed exactly *opposite* the luminous source. The distance the telescope will require to be placed back evidently must be equal to that traversed by it during the time the light takes to pass from the source to the telescope. Secondly, the light on the undulatory theory will suffer aberration in passing along the tube of the telescope, owing to the latter being in motion relatively to the light; no such aberration taking place on the corpuscular theory, since the corpuscles are moving at the same velocity as the telescope. Thirdly, on the undulatory theory, there will be a correction necessary, due to the motion of the luminous source ("equation of light"); such correction not being required on the corpuscular theory, since on that theory the light emitted partakes of the motion of

[1] We consider a *parallel* beam of light for simplicity, though it is not necessary to the principle.

[2] Of course the effect produced by aberration is the same with the eye alone as with a telescope, but we prefer to consider the latter, as its larger scale enables the effect to be visualised better.

the source. But it is a curious fact that the two errors occurring on the undulatory theory ("aberration" and "equation of light") happen precisely to counteract each other; so that therefore the luminous source is seen in its *true* position, *i.e.*, in the *same* position as by the corpuscular theory. The fact of the two errors compensating each other therefore prevents this occurrence of error from serving as a visual test between the two theories. There is, however, a distinct objective difference between the two theories in this case, as regards the *position* of the telescope (previously referred to); but before recurring to this, we may consider more closely the mode in which the above compensation of the two errors is brought about.

8. It is well known that the effect of the error termed "aberration" is to make the luminous source appear *forwards* of the position it occupied when the wave left it, and this by an absolute amount equal to that traversed by the telescope during the time the light took to pass from the source to the telescope. But from the fact that the luminous source is in motion, the latter *is* actually at the time of observation situated *forwards* of the position it occupied when the wave left it, and by precisely the above amount (since the source is moving at the same velocity as the telescope). Hence the two errors will precisely compensate each other, and the luminous source will be seen through the telescope in its true position, *i.e.*, in the *same* position as by the corpuscular theory. But it is important to note that on the undulatory theory, the telescope, in order to receive the parallel beam emitted by the luminous source, must be placed *not* opposite the source, but somewhat backwards, to make up for the slanting track of the beam attendant on the motion of the luminous source. On the corpuscular theory, on the other hand (where the track of the beam of light is *normal* to the line of motion of the source), the telescope must be placed *opposite* the source in order to catch the beam. The *position* of the telescope in the two cases therefore constitutes a distinct physical difference between the two theories, which might serve as a test.[1]

9. The above considerations may suffice to show in a simple manner that important differences exist in the effects attendant on the motion of a luminous source on the corpuscular and undulatory theories of light, and that these differences would be *in principle* capable (supposing practical difficulties surmounted) of constituting a simple and decisive test between the two theories.

S. TOLVER PRESTON

[1] The fact pointed out by Sir John Herschel in regard to the difference in the *velocity* of light on the undulatory and corpuscular theories, in the case of a luminous body moving directly towards or from the observer, would appear also to be worthy of remark. It is evident, for example, that in the case of a luminous body moving towards the observer, the velocity of light would *add* itself to that of the body on the corpuscular theory; but the velocity would be unaffected on the undulatory theory.

GEOGRAPHICAL NOTES

THE Paris Geographical Society held last Friday its annual December meeting, at its hotel, under the presidency of Admiral La Roncière le Nourry. The report of the progress of geography was read by M. Maunoir, the secretary. M. de Ujfalvy delivered an address on the region of Central Asia which he visited, and which may be termed the extreme frontiers of the Russian empire, and which are just now attracting so much notice. This traveller will soon come to London to give the same address before the Royal Geographical Society. His exploration was made at the expense of the French Government. On the following Saturday a banquet took place at the Continental Hotel. The usual toasts were given. After dinner M. de Lesseps, who is very likely to be nominated president of the Society for 1879, at the April meeting, gave some account of his visit to Tunis with Capt. Roudaire, in order to investigate the conditions of the creation of the new Saharan Sea. M. de Lesseps described the whole scheme as being easily practicable for a sum of not more than 60,000,000 of francs. He said that the French extension telegraph system was extending all over Tunis and Tripoli, and that Arabs were accustomed to follow the telegraphic line as their camels travelled at a quicker rate when following its track. He intimated that the Egyptian telegraphic system was extending to the equator, and that he advised M. Cochery, the Director of French Postal Telegraphy, to extend it all over Sahara up to Senegal.

PETERMANN'S *Mittheilungen* (for so it will continue to be named) for December contains a long and careful account of Chartography at the Paris Exhibition; Dr. Carl Martin contributes a paper, based on Chilian sources, on the Chonos Archipelago, which is accompanied by a map. An excellent paper on the present condition of Afghanistan (with map), is contributed by Herr F. von Stein. This number, besides the usual table of contents to the volume, contains a complete alphabetically arranged index to Dr. Behm's useful monthly summaries.

WE have received a specimen of the first number of one of those stupendous geographical works of which the French are so fond, and the best example of which is Elisée Reclus' "Géographie Universelle." Indeed, the new work announced seems to have pretty much the same object as that of Reclus, though the method is different. The new work is to be issued by the Librarie des Connaissances Utiles, and the author is M. Charles Hertz. Its title is "La Géographie Contemporaine d'après les Voyageurs, les Émigrants, les Colons, les Commerçants." It will consist of ten series of from three to five volumes each. We trust the publishers will find subscribers patient enough to wait for the end. There will be from 600 to 800 maps and hundreds of illustrations, and the work will be issued in weekly parts. We calculate it will take fifteen years to complete. Judging from the specimen, a good deal of narrative and adventure will be introduced into the work, and that it will be at least as entertaining as instructive. The first series will deal with polar and maritime expeditions, the second with Africa, the third with Asia, the fourth with Australia, and the fifth with means of communication, geographical societies, &c. The other five series will be devoted to a description of the nations of European origin and their enterprises over the globe. It is a grand scheme.

DR. THOLOZAN, physician to the Shah of Persia, is organising a scientific exploration of the province of Khuzistan, the southern province of Persia. The expedition will start from Bassorah on February 1 next.

NOTES

THE Corporation of the City of London having determined to identify themselves with the movement by the Livery Companies of London for the advancement of technical education, on Thursday last elected the following to serve on the Board of Governors of "The City and Guilds of London Institute for the Advancement of Technical Education":—The Lord Mayor *ex officio*; the Recorder, *ex officio*; Aldermen: Sir Thomas Dakin, Knt.; Sir Robert W. Carden, Knt.; Mr. William Lawrence; Sir Francis W. Truscott, Knt.; Sir Wm. A. Rose, Knt., F.R.S.L., F.R.G.S.; Simeon C. Hadley; Common Council: Mr. Joseph Beck, F.R.A.S.; Mr. W. Bassingham; Mr. J. L. Shuter, F.R.A.S.; Sir C. Reed, Knt., LL.D., F.S.A., Dep.; Mr. George Shaw; Mr. J. Edmeston; H. Lowman Taylor, J.P., Dep.; S. E. Atkins, Dep.; Sir Jno. Bennett, Knt.; Mr. Henry Greene; Mr. John Faulkner; Mr. Thomas Waller.

Last week Dr. Gladstone read an important paper at the Society of Arts on science teaching in elementary schools. He assumed, first, that it is not good that poor children should go forth into the world in gross ignorance of the material objects among which they must always live and work; secondly, that it is far from desirable to try to make scientific men and women of boys and girls of twelve and thirteen years of age. "This earth," Dr. Gladstone said, "is our dwelling-place, from the cradle to the grave; our bodies are the complicated machines, so wonderfully made, by which every action of ours is performed; the sun, clouds, and atmosphere influence us every day; the animal, vegetable, and mineral kingdoms are ready to yield us their supplies; and the great mechanical and chemical forces, with heat, light, and electricity, are ready to be our servants if we do not allow them to become our masters. Every man, also, in his handicraft or trade, as well as every woman in her domestic duties, has to deal with some facts and objects of nature specially connected with them." Dr. Gladstone then proceeded to point out the present state of the question, showing that very much yet remains to be done ere science takes the place it ought to occupy in our elementary schools, though the energetic London School Board is doing much to give science-teaching an established place in the schools under their charge. He referred to the universality of science instruction in Germany, and expressed a hope that a "knowledge of common things" would soon take its place alongside of the older subjects in all our elementary schools.

We are glad to learn that the health of Prof. Hoffmann, the well-known chemist, is now completely restored, and that he is again among his pupils.

M. Flammarion writes to us in reference to a note from a correspondent last week, that the subscription he is organising is mainly for the purpose of founding at Paris a free observatory created by private means, on the model of those which exist in England. It is desired to establish in the observatory the most powerful instrument which the funds will enable to be constructed. This instrument is intended above all for the physical investigation of the planets, and particularly of the moon, "the vital state of which," M. Flammarion writes, "may thus be definitely settled. It is not proved," he says, "that the moon is a dead planet, but the progress of optics appears to me to be now such as to justify a serious investigation for traces of life upon it; in fine, to settle what opinion ought to be held on the question of the habitability of the moon."

The September number of the *Mineralogical Magazine and Journal of the Mineralogical Society of Great Britain and Ireland* contains three original papers. Mr. Sorby gives a few test-experiments of his new method of determining the refractive indices of mineral plates, which will hardly be new to readers of Nature. Prof. Heddle continues his papers on the geognosy of Scotland. The present one is almost entirely occupied with the geology of the Island of Fetlar, and gives several analyses of the minerals found in its different rocks. Prof. How's contribution to the mineralogy of Nova Scotia is mainly taken up with mordenite and some altered nodules found at Cape Split; the rest of the paper is not much more than a list of localities. Ten pages are taken up by very poor abstracts from the *Zeitschrift für Krystallographie* and other periodicals, which are far inferior to those published in some of the weekly journals. Most of these are by a gentleman who may be a mineralogist, but is scarcely a crystallographer. He is grandly impartial as to notation, apparently following the authors in using either the Millerian or Naumannian. When using the former he often forgets to state the system in which the mineral crystallises. When the latter is used in an English mineralogical journal, the least we can expect is to have the Millerian equivalents given side by side with the Naumannian symbols. The conversion is not difficult, and tables for the purpose are given in all the text-books. Near the end is an abstract by the editor of a flattering notice of his own book, the good taste of which is obvious. We find it difficult to think that a magazine so indifferently conducted can prove either a commercial or a scientific success.

Captain Howgate has sent us some of the results of the preliminary United States Arctic Expedition in the *Florence*, in the shape of reproductions of photographs and of drawings by the Eskimo. The latter are rude, but vigorous and amusing, and show the well-known talent of the natives for drawing.

Experiments on the electric light, with Jablochkoff candles, have been tried at Havre, and, in consequence of the frequency of maritime collisions, are attracting much notice from seamen. The *Breeze*, one of the British mail steamships, was stranded a few days ago, when trying to enter the Calais port, owing to the prevalence of fogs. This accident would not have taken place if an electric light had been placed at the end of the jetty, as had been proposed. It is said the matter has been reported to the French Minister of Public Works. The electric light experiments in the Avenue de l'Opera and in front of the Palais Legislatif are to be continued up to January 15, but at the expense of the company, the Municipal Council having refused to pay more than the sum which would have been spent by them if the streets had been lighted with ordinary gas.

The question of "reserves" for the aborigines having been recently raised in the Queensland Legislature, it has been recommended that the system of the Durundur reserve should be extended, as there are many other places where it might be advantageously tried.

We have received from Messrs. De la Rue several specimens of their diaries, which are marked by all their usual elegance and usefulness. Much valuable information is prefixed to several of the diaries, though we still regret the absence of some of the astronomical information which used to give them a distinctive value. Messrs. Letts have also sent us a number of their various forms of diaries, the marked feature of which is their utility. They are of all sizes and prices, and no one need have any difficulty in providing himself with this useful help to order and memory. Messrs. Letts have also published a handy weather table by Mr. Saxby, containing a great deal of useful and well-arranged information.

The *Madras Mail* states that great progress is being made in the cultivation of chinchona in the Wynaad, and that nearly a million plants have been taken there this year from the Neddiwuttum estates, and this is in addition to what is obtained from the extensive chinchona nurseries on all the coffee estates. All the poorer parts of these are being planted with chinchona which is found to thrive well where coffee will not grow.

A letter from Peking states that the Viceroy Li Hungchang has entered into a contract with Mr. Arnold Hague, of New York, an able geologist and mining expert, for the purpose of prospecting the gold, silver, and other mines in the north of China. Mr. Hague is expected to start shortly from Tientsin for the mining regions. The just published Consular Report from Canton also states that General Fang, a well-known military officer, has been instructed to arrange for an immediate supply of European machinery to be used in local mines. It appears to be thought that there is great likelihood of the early working of coal and other mines in the provinces of Chihli, Kiangsi, Kiangsu, and Szechuen.

For some time past there has been so little water in portions of the Grand Canal of China, that it has only been navigable by

small boats, and for this reason, among others, it has been found necessary to send the grain tribute up to Peking by sea. The last mail from China, however, brings news of an unusual rise in its waters. A correspondent of the *North China Herald* left Tientsin on October 10 and observed no especial increase in the water until he reached the town of Hsingchi, about seventy miles to the south. There he met with a south-west wind, and noticed the water up to the top of the banks. Further on the people were seen raising narrow ridges to prevent the water from overflowing into the fields. Beyond the city of Tsingchow, and as far as the Narpi district, large tracts of land were under water. The general level of the country east of the canal is about six or eight feet below the artificial embankment, and in not a few places this is very weak from the constant wash, especially at the bends, so that the danger is serious. The writer had not been able to learn the cause of this unusual and rapid rise in the Grand Canal, but the Chinese attribute it to heavy rains in the mountainous region to the south-west, though it has never been known to happen before so late in the year.

THE Board of Trade having received, through the Foreign Office, reports from Her Majesty's Consul at Taganrog as to the recent destruction of corn crops in the neighbourhood of that place by a beetle described as the *Anisoplia austriaca*, have communicated with the Entomological Society of London, and have been favoured by that Society with a report upon the subject. The Report is signed by Messrs. McLachlan and Waterhouse, who state that the insect *Anisoplia austriaca* has nothing whatever to do with the "Colorado beetle." It belongs to a group of beetles (Rutelidæ) allied to our common cockchafer, but is of very much smaller size. There can be no doubt, they state, that the eggs are deposited in the earth at the roots of corn and grasses, that they soon hatch, and that the larva feeds upon the roots. How long a period elapses before the pupal state is assumed they know not, but they think that two years may be the outside limit, and that in the autumn of the second year of its existence, the larva either forms a cocoon in which it remains quiescent until the following spring, when it assumes the pupal state, or, as is more probable, it assumes that state in the autumn, and the perfect insect may be developed soon afterwards, but remain in the cocoon until the following summer. All accounts, however, they have been able to refer to concerning this and congeneric species, agree (as does the information furnished by Mr. Carruthers) in attributing the chief damage to the perfect insect, which feeds upon the green corn in the ear. After referring to the abundance of the beetle in some parts of Germany, they commend, as a preventive of its ravishes, rotation of crop and encouragement of insectivorous birds, and state their opinion that there is no reason to apprehend the recurrence year after year of such multitudes of the beetles as have occasionally appeared. "In the present state of entomological science," they conclude, "it is impossible to accurately account for visitations like this, which occur with many insects, injurious or otherwise. It may be that the pupal condition is prolonged indefinitely, or until circumstances favour its determination; by this reasoning (which is warranted by what we know to be the case in some other insects) the pupæ might be accumulated from year to year, and the perfect insects from these accumulations burst forth simultaneously."

AT the second monthly meeting of the Statistical Society, held on the 17th inst., Dr. Mouat, Foreign Secretary of the Society, who was deputed to represent it at the meetings in Paris of the Demographic Congress, and of the Permanent Commission of the International Statistical Congress, and, in Stockholm, of the International Penitentiary Congress, read reports of the proceedings of those bodies, so far as they were of interest to the Statistical Society. The chief work of the Permanent Commission was its own reorganisation as the executive of the Statistical Congress, and the adoption of a scientific scheme of classification of statistics for international purposes. Statistical annuals for 1877 have been published in France, Italy, Prussia, Austria, Hungary, and Belgium, all differently arranged. With a view to the adoption of uniformity of system on strictly scientific principles, M. Deloche, the chief of the General Statistical Department in Paris, suggested such a system, and it was adopted. Taking territory and population as its basis, the subsequent grouping of the many and varied facts by which human activity is manifested was in the order of the faculties to which they naturally attach themselves, viz., the moral, the intellectual, and the physical. Reference was made by Dr. Mouat to the large amount of valuable statistical information collected and published annually in the form of the statistical abstracts of the British Empire; and to the mass of miscellaneous statistics furnished from time to time to Parliament. Dr. Mouat was of opinion that the time had not yet arrived for the scientific classification of statistics, and he suggested for consideration a more simple form under four heads, viz., (1) Territory and Population; (2) Revenue and Commerce; (3) Laws and Government; and (4) Miscellaneous Statistics, containing all that cannot be grouped under any of those heads. The Demographic Congress was entirely devoted to the consideration of questions of population in the aggregate, or the natural history of society, as distinguished from physiology—births, deaths, marriages, migrations, &c.

THE *Times* a few weeks ago gave prominence to some facts showing the highly electrical condition of the atmosphere in Canada during the fine dry winter there. A letter from Mr. A. H. McNab, of Teignmouth, in the *Western Times* of December 17, will show that similar conditions exist in England during similar weather. He says:—"About 7.45 P.M. on Friday I was crossing Shaldon Bridge from the Teignmouth side, and immediately after passing the 'drawbridge' I was much surprised to find both sides of the bridge illuminated at certain regular distances with pale blueish lights resembling flickering lamps. My first impression was that, owing to the dense fog that prevailed, some sort of lamps had been placed on the bridge as a warning for the boats and barges passing up and down the river, but on coming to the first iron upright in the bridge railing I at once came to the conclusion that the light was electrical, for each point of iron had a flame of about two inches in height, composed of electric sparks issuing from it, accompanied by a hissing sound resembling that caused by the escape of gas from unlighted burners, and all the iron points along the bridge were similarly illuminated, producing a most charming effect. The point of the umbrella which I carried was also emitting a light, and when moved about produced sparks and sound. When I was midway across the bridge a passenger suddenly appeared in the fog and called out to me, 'Sir, your umbrella has a light on the end of it.' His surprise, however, was not lessened when I informed him that his own umbrella had the same. So far as I observed the phenomenon was entirely confined to the bridge, and on my return some time after it had entirely disappeared. Perhaps the large amount of iron in the bridge may have something to do with it."

THE mission of Sergeant Jennings, of the United States Signal Corps, has not been quite fruitless in Paris. A sort of weather indicator has been placed, by order of the Prefect of the Seine, at the doors of the Luxembourg Palace; others will, it is said, be very shortly installed in several places of Paris.

AS No. 1 of "Dimmock's Special Bibliography" (Cambridge, Mass.) we have received a descriptive list, arranged according to date, of the Entomological Writings of Prof. John L. Le Conte, compiled by Mr. S. Henshaw. There are 150 entries, and the list seems compiled with great care and must prove useful to entomologists.

ABSORPTION OF WATER BY THE LEAVES OF PLANTS

THE experiments of Boussingault, referred to in NATURE, vol. xviii. p. 672, find a fitting sequel in those of the Rev. G. Henslow, detailed in a paper read before the Linnean Society on November 7. Although gardeners universally maintain that growing plants have the power of absorbing water through their leaves, both in the liquid and the gaseous form, in addition to the power of suction through the roots, yet the contrary theory has been in favour during recent years among vegetable physiologists. The first recorded experiments of any value on the subject were about the year 1727, by Hales,[1] as described in his "Statical Essays;" the conclusion to which he came being that "it is very probable that rain and dew are imbibed by vegetables, especially in dry seasons." This result was confirmed by Bonnet in 1753. A century later, however, in 1857, Duchartre, experimenting on the absorptive power of plants, came, after considerable wavering, to the conclusion that rain and dew are not absorbed by the leaves of plants. This opinion has been, with but little exception, held by all physiologists during the last twenty years, notably by De Candolle and Sachs; the explanation offered of the fact that withered plants revive when placed in moist air or when the leaves are moistened, being that transpiration is thus stopped, or is more than counterbalanced by the root-absorption. In his "Text-book of Botany" (English edition, p. 613), Sachs says:—"When land-plants wither on a hot day, and revive again in the evening, this is the result of diminished transpiration with the decrease of temperature and increase of the moisture in the air in the evening, the activity of the roots continuing; not of any absorption of aqueous vapour or dew through the leaves. Rain again revives withered plants, not by penetrating the leaves, but by moistening them, and thus hindering further transpiration, and conveying water to the roots, which they then conduct to the leaves." McNab has, however, proved that leaves do transpire, even in a moist atmosphere, provided they are exposed to the action of light.

The results of Mr. Henslow's experiments, extending over several years, are altogether in accordance with those of Boussingault, and may be considered to set the question of the absorbent power of the leaves of plants completely at rest. The following are the chief conclusions arrived at:—1. The absorption of water by internodes. The experiment consisted of wrapping up one or more internodes of herbaceous plants in saturated blotting-paper and in noticing the effects. As a rule the leaves on the shoots rapidly perished, showing that transpiration was too great for the supply. The stems, however, kept fresh for different periods up to six weeks. 2. Absorption by leaves, to see how far they could balance transpiration in others on the same shoot. The general result is that as long as the leaves remain green and fresh in or on water, they act as absorbents, but that the leaves in air keep fresh or wither according as the supply equals or falls short of the demand. 3. To test how far leaves on a shoot can nourish lower ones on the same shoot. It appears that it is quite immaterial to plants whether they be supplied from water by the absorbing leaves being above or below those transpiring. Water flows in either direction equally well. 4. Leaves floating on water. It was found that one part of a leaf can nourish another part for various periods, though the edges out of water died first. 5. Absorption of dew. A long series of cut leaves and shoots were gathered at 4 P.M., then exposed to sun and wind for three hours, then carefully weighed and exposed all night to dew. At 7.30 A.M., after having been dried, they were weighed again, and all had gained weight and quite recovered their freshness, proving that slightly wetted detached portions do absorb dew. 6. Imitation dew. Like results followed from using the "spray," by which dew could be exactly imitated. 7. Plants growing in pots and of which the earth was not watered, were kept alive by the ends of one or more shoots being placed in water; *e.g.*, *Mimulus moschatus* not only grew vigorously and developed axillary buds into shoots, but even blossomed.

By these interesting experiments the physiological botanist is again placed in harmony with the gardener who syringes his plants, not merely for the purpose of washing off dust and insects, but in order to facilitate the actual absorption of water by the surface; and with the field botanist, who sprinkles the plants in his vasculum with water to keep them fresh till he reaches home. The fact which now seems established beyond all doubt by the observations of Darwin and others, that certain plants have the power of absorbing through their leaves and digesting the remains of animal substances, also implies, as a necessary corollary, the absorbent power of leaves for certain liquid or gaseous substances. In connection with this subject sufficient attention has perhaps not been attracted to the observations of Prof. Calderon, as detailed in a paper printed at Madrid, in English (1877), entitled, "Considerations on Vegetable Nutrition."[1] Calderon's statements—which, however, require at present to be confirmed by other observers—are to the effect that plants have the power of absorbing the nitrogenous organic matter which is constantly floating in the air, and that, if air be deprived of all organic matter, it is unable to sustain vegetable life, all the physiological functions of plants being then suspended.

A. W. B.

[1] In his "Geschichte der Botanik," pp. 515-521, Sachs gives an admirable epitome of the great service rendered to the progress of botanical science by the researches of this eminent botanist and physicist.

[1] See NATURE, vol. xv. p. 108.

UNDERTONES

THESE formed the subject of a lecture delivered by Herr Auerbach before the meeting of Naturalists at Cassel this year.

The term "undertones," he pointed out, is an extension of the nomenclature which denotes certain accompanying tones of a given note "overtones." Undertones may be observed in the following way:—If a struck tuning-fork be set on a sound-board, a tone is heard sounding strongly, which before was little perceptible. The stem of the fork makes longitudinal vibrations, which, by action on the sounding-board (a very thin one), generate transverse vibrations, and these spread over the large surface of the plate. Should the tone of the board only differ in intensity from that of the fork, the vibrations executed by the stem of the tuning-fork must be small; it is otherwise, however, when the vibrations exceed a certain amount.

Herr Auerbach demonstrated his observation with a tuning-fork giving the A of a violin, and so 435 vibrations per second. When he placed the vibrating fork firmly on a sonud-board, the tone was heard distinctly at a distance. When, however, he brought the tuning-fork, struck very vigorously, into very light contact with the plate, there was heard the lower octave of the fork's tone. With other materials, which were not then at his command, he could produce also the lower fifth of the lower octave, and the lower fourth of this tone, *i.e.*, the double octave of the fork's tone. The vibration numbers of these resonance tones are $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c., of that of the tuning-fork's tone, *i.e.*, the resonance-tones form the series of the *harmonic undertones*.

With regard to the mode of occurrence of these tones, Herr Auerbach said this: "To prove to you, first of all, that the strength of the vibrations is the fundamental condition of the phenomenon, I will once more make the experiment, and continue it longer. You heard first, again, the lower octave; but then the tone sprang over into the higher, so that it became identical with the proper tone of the tuning-fork. Consider this result along with the fact that the vibrations of the fork rapidly diminish; remember, too, that it is only when the fork is vigorously struck and lightly placed on the plate that the undertones occur, and you will see that the cause of the phenomenon lies in the amplitude of the vibrations."

Herr Auerbach further supposes that the resonance-surface of the plate, being imperfectly elastic, follows the movements of the stem of the fork immediately downwards indeed, but not upwards; an interval then occurs which only disappears on the next passage downwards of the stem. If the retardation be a small one, the plate, at the moment of meeting the stem a second time, has nearly completed a vibration. If, however, the retardation be great, the undertones arise (as the lecturer showed graphically) from the combination of the vibrations of the stem and the plate. The stem-end, *i.e.*, in this latter case, is no longer an unconditionally free end, but its freedom is a periodic function of the time, and this period is twice as great as that of the tuning-fork vibrations. "That the undertones arise in consequence of internal friction, was easy to see *à priori*: what the experiments have shown and explained is the interesting fact, that precisely the *harmonic undertones* are produced; that is a consequence of the fact that the resonance is the action of a periodic force, and so, in a certain sense, a discontinuous phenomenon, otherwise the undertones must form a continuous series, which is not the case."

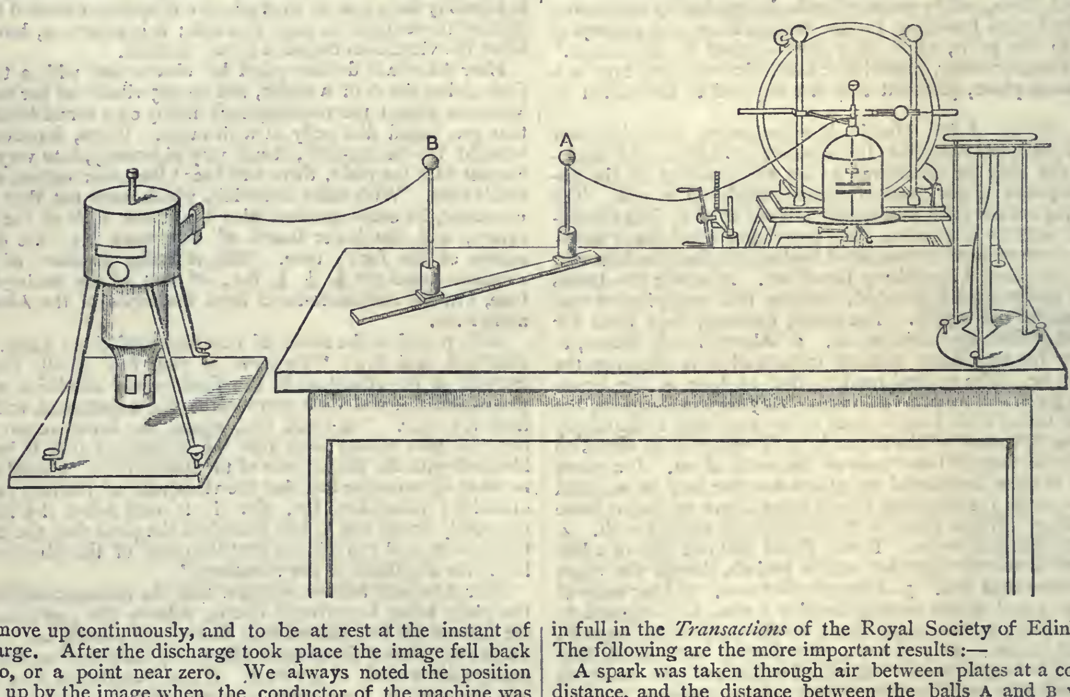
"It is obvious," he continues, "that the description given of the phenomenon is incomplete, for ductility, elasticity, variation of the resonance-plate, &c., co-operate to produce a more complicated phenomenon. I have tried a great many materials for undertones, and found that they fall, in this respect, into three groups. In the middle stand those materials which furnish undertones, that is, the great majority of all substances in general. On the one side are those substances which, as soon as the vibrations are pretty strong, give no resonance-tones, but merely an indeterminate noise; to this group belong rolled plate metal and most kinds of glass. On the other side are those substances which, however strong the vibrations, always give the tone of the tuning-fork. I have found only one example of this, viz., the wood of mountain fir, in thin polished plates. It was natural to try the belly of a violin, which is mostly made of fir-wood, for undertones, and in this way form an idea as to the elasticity of the wood, on which the excellence of the instrument greatly depends. From the German violins I have examined, I have always obtained undertones; from the few authentic Italian violins accessible to me I obtained, on the other hand, always the original tone. But I acknowledge that more abundant material is necessary for a decision of this question."

THE DISRUPTIVE DISCHARGE OF ELECTRICITY[1]

BY means of the following method we have been able to investigate the laws of the disruptive discharge of electricity of high potential—a subject of investigation which is the complement of that in which Drs. Warren de la Rue and Müller have been simultaneously engaged. In making these experiments I have had the able co-operation in succession of Messrs. Salvesen, Connor, Stewart, Simpson, and Playfair.

The method essentially consists in connecting the prime conductor of the Holtz machine, not with the electrometer directly, but with an insulated spherical ball placed at some distance from an equal spherical ball, the latter being connected with the electrometer. The woodcut represents, *in situ*, the apparatus which was used in the case of the gases. The receiver of the air-pump, which has a rod capable of moving air-tight, was attached to one of the conductors of the Holtz machine in such a manner that the conductor and the rod formed one conducting system. Projecting from the plate of the air-pump was a short metal rod, which formed one conductor with the metallic parts of the air-pump, and, by means of a wire, with the uninsulated conductor of the Holtz machine. Electrodes of various forms were made to screw on to the ends of the rods. Of the two insulated brass balls one, A, was fixed; the other, B, could be moved along the connecting board. The wire joining A to the collar of the receiver is insulated with gutta-percha. The electrometer in connection with B is one of Sir W. Thomson's divided ring reflecting electrometers.

When the potential of A is raised by driving the machine, the potential of B is also raised, and this goes on until a discharge takes place between the electrodes inside the receiver. Hence the maximum deflection of the spot of light from zero is an indication of the difference of potential of the two surfaces between which the spark passed immediately before the discharge. By breaking the contact between the conductors of the Holtz machine before beginning to turn the wheel, and, by turning slowly and uniformly, we were able to make the image of the



wire move up continuously, and to be at rest at the instant of discharge. After the discharge took place the image fell back to zero, or a point near zero. We always noted the position taken up by the image when the conductor of the machine was completely discharged.

The force resisting the deflection of the mirror is the action of two external magnets upon several small magnets fixed to the back of the mirror.[2]

One great merit of our method is the rapidity with which observations can be made. Three readings were in general taken for each entry. The mean of these is very probably free from any error due to accidental variations in the passage of the spark. An extensive series of observations have been printed in full in the *Transactions* of the Royal Society of Edinburgh. The following are the more important results:—

A spark was taken through air between plates at a constant distance, and the distance between the balls A and B varied. Let V denote the induced potential, and r the distance between the centres of A and B; then the experimental curve obtained satisfies the equation—

$$V = 6081r^{-1} - 42{\cdot}26$$

for values of r greater than 24 centimetres; but for less values of r the function requires to be corrected by being multiplied by—

$$\cdot524 + \cdot02r.$$

Sparks were taken through air at the atmospheric pressure between parallel metal disks of 4 inches diameter for distances up to 1·2 centimetres. The function for V, the difference of potential in terms of s, the length of the spark is—

$$V = 66{\cdot}94\sqrt{\{s^2 + \cdot205s\}},$$

[1] Abstract by the author of thesis for D.Sc. and other papers printed in the recently issued part of the *Transactions* of the Royal Society of Edinburgh. By Alexander Macfarlane, M.A., D.Sc.

[2] Our results were reduced to absolute measure by means of the absolute electrometer represented on the table.

the equation of a hyperbola whose semi-transverse axis is ·1025 centimetres, and semi-conjugate axis 6·8623 C.G.S. units. From the above equation we infer that—

$$R = 66·94 \sqrt{\left\{1 + ·205 \frac{1}{s}\right\}}$$

where R denotes the electrostatic force; from which it is evident that as s becomes smaller R becomes greater. A similar curve was obtained when hydrogen was substituted for air.

When the disks were heated before taking the sparks, the curve obtained satisfies the equation—

$$V = 87·04s - 19·56s^2,$$

a parabola, from which we deduce—

$$R = 87·04 - 19·56s.$$

It was found that when the capacity of the charged conductor was changed, the difference of potential required to produce a spark remained constant.

When the discharge was continued so as to keep the spot of light at a fixed deflection, the reading was always less than for the corresponding single discharge, but the curves were similar.

Readings were taken of the difference of potential required to produce a ·5 centimetre spark through air at different pressures from the atmospheric to 20 mm. They give—

$$V = ·0458 \sqrt{\left\{p^2 + 203p\right\}}$$

where p denotes the pressure in millimetres of mercury.

The electric strengths of several gases were determined by comparing the differences of potential required to pass a ·5 centimetre spark through the gas at the atmospheric pressure.

Dielectric.	Electric Strength.		
	Macfarlane.	De la Rue and Müller.	Faraday.
Air...	1	1	1
Carbonic Acid ...	·95	1·06	·91
Oxygen	·93	1	·71
Hydrogen... ...	·63	·54	·53
Coal Gas	·93	—	·71

"Electric strength" is the term used by Prof. Clerk-Maxwell to denote the physical constant in question. I have added, for the sake of comparison, values deduced from the results of De la Rue and Müller and of Faraday, but the ratios given do not strictly give the relative electric strength, but the ratio of the lengths of spark when the difference of potential is kept constant.

The difference of potential required to produce a spark between two spherical balls is approximately proportional to the square root of the length of the spark. This we have verified up to 15 cm.

On proceeding to investigate the discharge through insulating liquids, we first took up oil of turpentine. The liquid was placed in a glass jar of 7 inches diameter and 5 inches height. A screw passing through the bottom of the jar served to fix the lower electrode, and also to afford conducting connection with the earth. We observed four modes of discharge: by means of threads of solid particles, by motion of the liquid, by a disruptive discharge, and by motion of gas bubbles. When a chain was formed the index of the electrometer behaved as if a current were passing. The discharge, when sufficiently great, broke the thread and turned into a spark. The liquid was more easily set in motion when its surface was not much higher than the upper plate. The bubbles of gas appeared to be formed by the passing of the spark. They were always attracted to the negative electrode. When the electrification was neutralised they of course adhered to the under surface of the upper disk; when the disk was electrified negatively they still adhered; when positively they were repelled so as as to remain suspended in the liquid or to adhere to the lower electrode, according to the greater or less distance between the electrodes. At a diminished pressure the bubbles produced at the upper surface were observed to effect the discharge by carrying the electricity with them to the negative electrode. The fact that it is possible to cause a shower of electrified bubbles to descend and produce a flash and sound on impinging on the lower surface appears to throw some light upon the nature of lightning balls.

Similar phenomena were observed in paraffin oil, excepting that the gas bubbles produced were generally attracted to the positive surface.

We observed the differences of potential required to pass a spark through paraffin oil and oil of turpentine between plates for distances up to ·5 cm. It was impossible to observe for greater distances, as our insulated wire allowed the charge to escape. For paraffin oil,

$$V = 750\,s - 15;$$

therefore, $R = 750.$

The above has not been reduced to absolute measure. Thus R is constant in the case of the liquids, but variable in the case of the gases.

Electric Strength of Liquid Dielectrics

Air	1
Paraffin Oil (kind used for burning)	4
Oil of Turpentine	3·7

Sparks were taken between two platinum wires placed at right angles to one another. When one of the wires was heated by a voltaic current the electrometer deflection was diminished by about one-fourth of its amount.

We have also investigated the effect upon the electric spark of heating the air round the disks, the pressure being kept constant. The deflections of the electrometer for a constant spark for temperatures from 20° C. to 280° C. indicate a curve which slopes down gradually as the temperature is increased, while the deflections during cooling give a curve which is somewhat lower at the lower temperatures.

These experiments were made in Prof. Tait's laboratory, to whom we are indebted, not only for the use of apparatus, but also for ever ready advice.

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 10.—The loss of electricity by an insulated charged body in rarefied gas in an envelope that has conductive connection with the earth is here stated by Herr Narr to be due to two processes distinct in time and intensity, the first, one of outflow, rapid and intense, the other, one of dispersion, slow and weak. The intensity of the former increases with decreasing density of each of the gases used (CO_2 air and H), and also on substituting one gas for the other in the order just given, the density remaining constant. These differences between the gases decrease with the density, and in vacuum fal within the limits of errors of observation. In discussing these results, Herr Narr is led to regard the condensed layer of gas on the conducting system as an insulator, not as a conductor.—Dr. Holz finds that the specific magnetism of magnetic ironstone is the greatest of all magnetic substances hitherto examined. Its maximum permanent magnetism is nearly as great, and partly greater than that of steel as hard as glass. Its permanent magnetism is sooner removed in demagnetisation with the same external forces than that of steel, &c.—Dr. Strouhal enunciates the laws of a mode of sound-production not much studied hitherto, that, viz., of rapid swinging of a rod, a blade, or the like, in air, or the passage of air-currents over strong wires or sharp edges, &c. —Herr Braun contributes a long and interesting paper on the development of electricity as equivalent of chemical processes.—Herr Koch demonstrates the applicability of the method of determining coefficients of elasticity from the bending of short bars supported at the two ends, the sinking in the middle being measured by means of Newton's interference-bands, and he suggests a more thorough investigation of the elasticity of crystals, by the improved means he describes.—Some remarks on the atomic weight of antimony, with reference to Cooke's recent research, are communicated by Herr Schneider.

American Journal of Science and Arts, November.—In the opening paper Prof. Dana considers the value of some distinctive characters generally accepted in defining certain kinds of rocks, as, "older and younger," foliated or not, and porphyritic structure; showing them to be often trivial and inapplicable. —With regard to the relative agency of glaciers and sub-glacial streams in the erosion of valleys, Prof. Miles considers that the streams are of primary importance in working in advance of the ice in deepening and enlarging these valleys, and that the glaciers abrade, modify, and reduce the prominent portions left by the streams, and give them the well-known glaciated sur-

faces.—Prof. Holden describes certain cloud-shaped forms (obscuring the smaller forms of Janssen) observed on the sun's disc on September 16, and cites a like observation made by Prof. Langley, in 1873, who thinks the effect chiefly due to our own atmosphere, while disposed to admit the possibility of some obscuration in the sun itself.—A pseudomorph after anorthite, from Franklin, New Jersey, is described by Prof. Roepper; and Prof. Verrill gives an account of recent additions to marine fauna of the east coast of North America.—There is also a notice of Edison's sonorous voltameter.—Prof. Marsh's important contribution on the principal characters of American Jurassic dinosaurs has been previously referred to in these columns.

Morphologisches Jahrbuch, vol. iv., Part 3.—Studies on the innervation of the hair-bulbs of domestic animals, by R. Bormel, 70 pages, 3 plates.—On *Gloidium quadrifidum*, a new genus of Protista, by N. Sorokin.—The development of the knee-joint in man, with remarks on the joints in general, and the knee-joints of vertebrates, by A. Bernays.—The skeleton of the Alcyonaria, by G. von Koch, including a minute description of the skeleton in each genus, a general account of it, and a new systematic arrangement, 33 pages, with 2 plates.—C. Hasse continues his studies on fossil vertebræ; this part is devoted to their histology, and is illustrated by 4 plates.

Zeitschrift für wissenschaftliche Zoologie, vol. xxxi. Part 2.—Contribution on the Julidæ, by E. Voges, dealing very considerably with the tracheal system and its development. There are descriptions of many new species of Julus, Spirostreptus, and Spirobolus; 68 pages, 3 plates.—On the development of the blastoderm and the germinal layers in insects, by N. Bobretzky, with figures chiefly of *Porthesia chrysorrhœa*.—On the genus Brisinga, by H. Ludwig.—On Aspidura, a mesozoic genus of ophiurid, by Hans Pohlig.—On the structure and development of sponges, Part 5, by F. E. Schulze; another most valuable contribution, the author having now completely followed the development of *Sycandra raphanus*, 34 pages, with 2 beautiful plates.

Parts 3 and 4 in one.—On the cerebral sulci in Ungulata, by Julius Krueg; the paper deals very largely with the fœtal development of the convolutions, 50 pages, 4 plates.—Contributions to the anatomy of Ophiurans, by Hubert Ludwig, treating especially on the skeleton of arm and mouth, and the sexual organs, 50 pages, 4 plates.—On the generative organs of *Asterina gibbosa*, by Hubert Ludwig, 1 plate.—An account of the anatomy of Magelona, an interesting form, by Dr. W. C. McIntosh, of St. Andrews; translated from English for the journal, 72 pages, 10 plates.—On some cases of parasitism among Infusoria, by J. van Rees.—Brief notes on the development of Anodon, by C. Schierholz.

SOCIETIES AND ACADEMIES

London

Royal Society, December 12.—"The Magic Mirror of Japan," Part 1, by Professors W. E. Ayrton and John Perry, the Imperial College of Engineering, Japan. Communicated by William Spottiswoode, M.A., Treas. R.S., &c.

The President stated that Prof. Ayrton had agreed to give, in the Friday evening discourse on January 24, at the Royal Institution, a full account of Japanese mirrors, so that on the present occasion he understood the authors of the paper merely proposed to enter very shortly into the subject.

Prof. Ayrton commenced by remarking that mirrors in Japan held a very high position, and constituted the most prominent feature in the Japanese temples, taking the place of the cross in Roman Catholic countries, and that the principal mirror in the Imperial Palace ranked higher than even the Emperor himself. He referred to the important place the mirror held in the very limited furniture of a Japanese household; to the respect attached to it by the women, and to the fact that while the sword was considered as "the soul of the samurai" (or two-sworded class) the mirror was looked on as "the soul of the woman." He next showed experimentally the so-called magic property possessed by certain rare bronze mirrors, sold by the Chinese at about twenty times the cost of the ordinary mirrors of that country, and which consisted in these mirrors being able to reflect from their smooth polished faces the raised patterns of birds, flowers, dragons, or Chinese letters with which their backs were adorned. He stated that he had found this property to be possessed by a very small percentage of the Japanese mirrors which he had experimented on, but that its existence was quite unknown to the people of that country. The phenomenon had been known in China for centuries, and that, therefore, while he showed it experimentally to the Fellows, he did so in case there might be some there who had never seen it, in consequence of these magic mirrors being rare; but he desired it to be remembered that it was not the phenomenon itself but the explanation of it which he had the honour of bringing before them as new.

After citing all the possible ways in which this curious reflecting power could be accounted for, and referring to a number of printed notices that had at various times appeared of the magic mirror, the majority of which were accompanied with a theoretical explanation, he remarked that as the authors had apparently not made direct experiments with the mirror itself to elucidate the cause of the phenomenon, but rather to have satisfied themselves with endeavouring to find out how it could be reproduced in Europe, it was not to be wondered at that many of the suggested possible explanations were very far from the truth. Up to the present time, he believed, the idea of inequality of density of the surface of the metal mirror produced naturally in cooling, or in the supposed process of stamping, seemed to have found most favour in the West, while the belief that this variation in density arose from trickery on the part of the maker was the view entertained in China. Sir David Brewster and Sir Charles Wheatstone, on the other hand, who also thought that trickery was the explanation, believed the artifice to consist in the maker skilfully scratching on the face of the mirror, before polishing, lines exactly corresponding with the pattern on the back.

Prof. Ayrton next described what was the explanation of the phenomenon his experiments, made during the winter of 1877–78, had led him to, viz., that there existed extremely slight irregularities in the curvature of the polished surface (quite invisible to direct vision), of such a nature that the thicker parts, corresponding, of course, with the raised patterns on the back, were flatter than the remaining convex surface, so that there was less dispersion of light from the thick portion than from the thinner. He then described one of a series of diagrams illustrating various experimental arrangements of convergent and divergent beams of light which the authors had availed themselves of, and the use of which constituted, he said, the essence of the system of investigation employed by Prof. Perry and himself, and he explained that if his theory of the phenomenon was correct, then placing the screen, on which the reflection of the light from the Japanese mirror was cast, in a certain position, the phenomenon ought to *disappear*, and again putting the screen in another position, the phenomenon ought to be *inverted*; that is, instead of a bright image on a dark ground, which hitherto had alone been what has been observed by previous investigators, a dark image of the pattern on a bright ground ought to appear. This disappearance and absolute inversion of the phenomenon he said he had found to actually take place, but that he was compelled from want of time to leave the experimental exhibition of it for the Royal Institution. Various other facts, such as the necessity of holding the screen rather near, but not very near, the mirror when ordinary sunlight without lenses was employed, was, like the inversion phenomenon just referred to, shown to be explainable only on the inequality of curvature theory, and not on the inequality of density theory.

The next question that arose was how was this inequality of curvature produced? This was explained to be due to the method employed by the Japanese for making the face of the mirrors convex, which method had hitherto been quite unknown to foreigners, but which Prof. Ayrton had, after much trouble, found to consist in scratching the face while cold with a *megebo*, or "distorting rod." During the operation the mirrors became visibly concave, but, receiving a "buckle," sprung back again so as to become convex when the pressure of the rod was removed. The thicker parts of these magic mirrors yielded less under the pressure, were made therefore less concave when under the rod, and sprung back less, or became less convex, when the pressure of the rod had been removed. He then showed how this explained the fact discovered by Prof. Atkinson, of the Imperial University, Japan, in 1877, that a small scratch made on the back of a mirror with a blunt nail, although producing apparently no effect on the other side, became nevertheless visible as a bright line on the screen when a light was reflected from the mirror.

Prof. Ayrton concluded by remarking that while the Japanese knew nothing of the so-called magic phenomenon that formed the subject of the paper that evening, he had ascertained that

they had used another property for their priestcraft, but the account of this he would reserve for the lecture at the Royal Institution.

"On the Torsional Strain which remains in a Glass Fibre after Release from Twisting Stress," by J. Hopkinson, D.Sc., F.R.S. It has long been known that if a wire of metal or fibre of glass be for a time twisted, and be then released, it will not at once return to its initial position, but will exhibit a gradually decreasing torsion in the direction of the impressed twist. The best method of approximating to an expression of the facts has been given by Boltzmann ("Akad. der Wissensch. zu Wien," 1874). He rests his theory upon the assumption that a stress acting for a short time will leave after it has ceased a strain which decreases in amount as time elapses, and that the principle of superposition is applicable to these strains, that is to say, that we may add the after-effects of stresses, whether simultaneous or successive. Boltzmann also finds that, if $\phi(t)\tau$ be the strain at time t resulting from a twist lasting a very short time τ, at time $t = 0$, $\phi(t) = \frac{A}{t}$, where A is constant for moderate values of t, but decreases when t is very large or very small. A year ago I made a few experiments on a glass fibre, which showed a deviation from Boltzmann's law.

The glass fibre examined was about 20 inches in length. The glass from which it was drawn was composed of silica, soda, and lime—in fact, was glass No. 1 of my paper on "Residual Change of the Leyden Jar" (*Phil. Trans.*, 1877). In all cases the twist given was one complete revolution. The deflection at any time was determined by the position on a scale of the image of a wire before a lamp, formed by reflection from a light concave mirror, as in Sir W. Thomson's galvanometers and quadrant electrometer. The extremities of the fibre were held in clamps of cork; in the first attempts the upper clamp was not disturbed during the experiment, and the upper extremity of the fibre was assumed to be fixed; the mirror also was attached to the lower clamp. This arrangement was unsatisfactory, as one could not be certain that a part of the observed after-effect was not due to the fibre twisting within the clamps and then sticking. The difficulty was easily avoided by employing two mirrors, each cemented at a single point to the glass fibre itself, one just below the upper clamp, the other just above the lower clamp. The upper mirror merely served, by means of a subsidiary lamp and scale, to bring back the part of the fibre to which it was attached to its initial position. The motion of the lower clamp was damped by attaching to it a vane dipping into a vessel of oil. The temperature of the room when the experiments were tried ranged from 13° C. to 13·8° C., and for the present purpose may be regarded as constant. The lower or reading scale had forty divisions to the inch, and was distant from the glass fibre and mirror 38⅜ inches. Sufficient time elapsed between the experiments to allow all sign of change due to after-effect of torsion to disappear. In all cases the first line of the table gives the time in minutes from release from torsion, the second the deflection of the image from its initial position in scale divisions.

Experiment VI.—Twisted for 121 minutes.

t	½	1	2	3	4	5	7
Scale divisions	191	170	148	136	126½	119½	108½
t	10	15	30	65	90	120	589
Scale divisions	97	84½	63½	41½	34	28	3½

The time was taken by ear from a clock beating seconds very distinctly.

The first point to be ascertained from these results is whether or not the principle of superposition, assumed by Boltzmann, holds for torsions of the magnitude here used.

If the fibre be twisted for time T through angle X, then the torsion at time t after release will be $X\left\{\psi(T+t) - \psi(t)\right\}$ where

$$\psi(t) = \int \phi(t)\, dt.$$

If now $T = t_1 + t_2 + t_3 + \ldots$ we may express the effect of one long twist in terms of several shorter twists by simply noticing that

$$X\left\{\psi(t) - \psi(t+T)\right\} = X\left[\left\{\psi(t) - \psi(t+t_1)\right\} + \left\{\psi(t+t_1) - \psi(t+t_1+t_2)\right\} + \left\{\psi(t+t_1+t_2) - \psi(t+t_1+t_2+t_3)\right\} +, \&c.\right]$$

Apply this to the preceding results, calculating each experiment from results for shorter time. Let x_t be the value of $\psi(T+t) - \psi(t)$, that is, the torsion at time t, when free, divided by the impressed twist measured in same unit; we obtain the following tables of comparison:—

Results for T = 121 compared with those from T = 20.

t	½	1	2	3	4	5	7
x_t observed ...	c·00979	871	758	697	648	612	556
x_t calculated ...	—	1070	950	880	830	780	730
t	10	15	30	65	90	120	589
x_t observed ...	497	433	325	212	174	144	18
x_t calculated ...	670	600	500	380	350		

The three last tables agree in indicating a large deviation from the principle of superposition, the actual effect being *less* than the sum of the separate effects of the periods of stress into which the actual period may be broken up. Kohlrausch finds the same to be the case for india-rubber, either greater torsions or longer durations give less after-effects than would be expected from smaller torsions and shorter periods.

The deviation from formula $\phi(t) = \frac{A}{t}$ appears to indicate the form $\phi(t) = \frac{A}{t^{\alpha}}$, α being less than, but near to, unity. If $\alpha = 0{\cdot}95$ we have a fairly satisfactory formula.

$$x_t = A'\left(\overline{T+t}^{\frac{1}{20}} - t^{\frac{1}{20}}\right), \text{ where } A' = \frac{A}{t_2} \text{ when } T = 121.$$

In the following table the observed and calculated values of x_t when T = 121 are compared, A′ being taken as 0·032.

t	½	1	2	3	4	5	7
x_t observed ...	0·00979	871	758	697	648	612	556
x_t calculated ...	0·00976	870	755	691	643	600	550
t	10	15	30	65	90	120	589
x_t observed ...	497	433	325	212	174	144	18
x_t calculated ...	493	429	320	218	176	147	42

Taking the formula $\phi(t) = \frac{A}{t}$ these experiments give values of A ranging from 0·0017 to 0·0022. Boltzmann for a fibre, probably of a quite different composition, gives numbers from which it follows that A = 0·0036.

In my paper on "Residual Change of the Leyden Jar," that subject is discussed in the same manner as Boltzmann discusses the after-effect of torsion on a fibre, and it is worth remarking that the results of my experiments can be roughly expressed by a formula in which $\phi(t) = \frac{A}{t^{\alpha}}$. For glass No. 5 (soft crown) $\alpha = 0{\cdot}65$, whilst for No. 7 (light flint) it is greater; but in the electrical experiment no sign of a definite deviation from the law of superposition was detected.

Geological Society, November 20.—R. Etheridge, F.R.S., vice-president, in the chair.—Rev. James Crompton and John Dennis Paul were elected Fellows.—The following communications were read:—On the upper greensand coral fauna of Haldon, Devonshire, by Prof. P. Martin Duncan, F.R.S. The author in this paper stated that since the publication of his supplement to the British fossil corals, published by the Palæontographical Society, several new corals have been obtained at Haldon by Mr. Vicary, of Exeter. Twelve additional species were noticed, of which ten were new. This brings the total number of species in the Haldon Greensand up to twenty-one. The new species are thus distributed:—*Aporosa:* Oculinidæ (1), Astræidæ (3), Fungidæ (5); *Perforata:* Turbinariæ (2); *Tabulata* (1). The paper concluded with remarks on the genera and species represented, from which it appeared that the coral fauna of Haldon is the northern expression of that of the French and Central European deposits, which are the equivalents of the British upper greensand. The Haldon deposit was formed in shallow water, and the corals grew upon the rolled *débris* of the age.—Notes on *Pleurodon affinis*, sp. ined., Agassiz, and description of three spines of cestracionts from the lower coal-measures, by J. W. Davis, F.G.S.—On the distribution of boulders by other agencies than that of icebergs, by C. E. Austin, C.E., F.G.S.

December 4.—Henry Clifton Sorby, F.R.S., president, in the chair.—Rev. William H. Allen, George Grey Butler, John Dixon, Rev. William Downes, B.A., Joseph Drew, M.D., Arthur Tom Metcalfe, E. P. Monckton, M.A., Albert J. Mott, Philip Lutley Sclater, Ph.D., F.R.S., William Hobbs Shrubsole, and Alexander Thuey, were elected

Fellows of the Society.—The following communications were read:—On some mica-traps from the Kendal and Sedbergh districts, by Prof. T. G. Bonney, F.R.S., and F. T. S. Houghton, B.A. The rocks described by the authors are mapped by the Geological Survey on quarter sheets 98 N.E., 98 S.E., and 97 N.W., and in parts briefly mentioned in the accompanying memoirs, under the generic name mica-trap. Seventeen examples are described macroscopically and microscopically, and of eight chemical analyses are given. It appears better to call one a porphyrite and two diorites (micaceous varieties). The remainder are all characterised by abundance of mica (biotite). Augite also appears to have been generally a constituent; but it has almost invariably been replaced by secondary products, calcite, dolomite, viridite, &c. Three are crystalline in structure; one of these is named minette, the others kersantite. The remaining eleven show a microcrystalline or cryptocrystalline base. It is proposed to call eight of them minette-felsite, the rest kersantite-porphyrite. These rocks commonly occur in rather narrow dykes; they are intrusive in Silurian strata, and, in the authors' opinion, are undoubtedly true igneous rocks.—Pleistocene notes on the Cornish coast near Padstow, by W. A. E. Ussher, F.G.S. In this paper the author described certain deposits seen in a small bay near St. Enodock's chapel, and known as Daymer Bay, and in section at Greenway cliffs.—The pleistocene history of Cornwall, by W. A. E. Ussher, F.G.S. In the first part of this paper the author, from his own observations and the writings of other geologists, gave detailed descriptions of the various superficial deposits of Cornwall, as exposed in numerous coast-sections. In the second part he discussed the relative ages of these deposits, and proposed a classification.

Physical Society, December 14.—Prof. W. G. Adams, president, in the chair.—Mr. W. Gleed and Mr. J. G. McGregor were elected members of the Society.—Prof. Guthrie read a note by Mr. C. Boys on a condenser of variable capacity. This condenser was designed for use in connection with the Holtz electrical machine to show the effect of condensation on the length of the spark. It consists of a test-tube coated externally with tinfoil to form the inner armature and a glass tube inclosing the test-tube, and having its outer surface covered with tinfoil for the outer armature. The inner tube can be slid out or in along the length of the external tube, and the capacity thereby varied. Prof. Guthrie showed that a spark from the Holtz machine could by its means be gradually reduced. Prof. Macdonnel stated that he had for some years used a similar apparatus, the inner coating, however, being strong sulphuric acid.—Dr. O. J. Lodge exhibited a differential thermometer in which saturated water vapour takes the place of air or other gas. This application is based on the fact that the pressure of a saturated vapour in contact with its liquid depends only on the temperature. An ordinary cryophorous answers the purpose when held so that the water occupies part of one bulb and a part of the stem next it; the greater the length of the water column in the latter, that is, the more horizontal the cryophorous is held, the greater the sensitiveness of the instrument. If, now, there be a difference of temperature between the two bulbs there will be a difference of pressure in the vapour in their interiors, and the level of the water will change until the pressure is equilibriated. When both bulbs are at one temperature the water in tube and bulb is on a level. Unlike air thermometers, the sensitiveness does not depend on the size of the bulbs or tube, and there is no increase of volume of the vapour. Another form consists of a U-tube, with bulbs at the end of each arm, each bulb having some liquid and the bend of the tube containing a short column of it, or, for greater sensitiveness, a series of films across the tube like diaphragms. This thermometer is found to be correct for temperatures below that of the ordinary temperature of the water and vapour, but inexact for high temperatures. With these latter the vapour tension is not the same throughout the tube, and distillation is set up. The instrument is a much more sensitive thermoscope than the air thermometer, and there is almost no limit to its sensitiveness to low temperatures. The radiation from the hand, held six inches from it, sensibly affects it, as also does the radiation from a piece of ice. For class purposes it is likely to be useful, from its simplicity and range of delicacy.—Mr. W. Clarke, Cooper's Hill College, from a series of experiments which he is making on the surface-tension of liquid gases, by means of their capillarity, gave the surface-tension of sulphurous anhydride as 2·3 milligrammes per square millimetre at −15° C.

Royal Microscopical Society, November 13.—Mr. H. J. Slack, president, in the chair.—A paper was read by Dr. Royston Pigott on some further inquiry into the limits of microscopic vision and the delusive application of Fraunhofer's optical law of vision, in the course of which he described numerous experiments to show that this well-known formula depends upon the laws of diffraction from rays reflected by bright discs or objects, but that it failed when applied to dark lines which were capable of being rendered visible far beyond the limits therein laid down. The subject was illustrated by numerous diagrams and by objects and apparatus exhibited in the room.—The President detailed the result of some recent experiments to determine the distance at which a human hair could be seen under various conditions by ordinary unassisted vision.—Mr. F. H. Wenham read a paper on the measurement of the angle of aperture of objectives, in which he described the method of measuring the true angle of aperture as distinguished from that of the angle of field with which it was commonly confused.—Mr. Henry Davis read a paper on the pygidium of insects, showing the organ commonly known by this name had its representatives in the Neuroptera, Gryllidæ, and other groups of insects, as well as in the flea and lacewing fly. He gave reasons for regarding it as a special organ of sensation conveying to the insect an intimation of the presence of dangerous enemies. Some discussion upon the subject took place between Mr. C. Stewart, Mr. Beck, Mr. Slack, and the author of the paper.—Some further communications, arising out of correspondence with Mr. Bedwell, were laid before the meeting by the Secretaries.—Three new Fellows were elected.

Institution of Civil Engineers, December 3.—Mr. Abernethy, vice-president, in the chair.—A paper was read on the heating and ventilating apparatus of the Glasgow University, by Mr. Wilson W. Phipson, M. Inst. C.E.

VIENNA

Imperial Academy of Sciences, October 17.—The following among other papers were read:—Remarks on Stephan's fundamental formulæ of electrodynamics, by Dr. Margules.—A hypothesis on the physical state of the sun, by Prof. Puschl.—Light as a reagent, by Herr Bohatta.—On the *Tonsilla* and *Bursa pharyngea*, by Dr. Ganghofner.

October 24.—On a simple apparatus for obtaining a constant gas pressure, by Dr. Handl.—A contribution to the doctrine of conic sections in descriptive geometry, by Prof. Miksic.—On meteorology of Alpine heights, by Dr. Hann.—On the formation of a space-curve of the fourth order, with a double point on a conic section.

November 7.—On the Venus transit of December 6, 1882, by Dr. Friesach.—Contribution to knowledge of internal friction in iron, by Herr Klemencic.—On the pitch of a tuning-fork in an incompressible liquid, by Prof. Kolacek.—Determination of orbit of the sixth comet of 1874.

CONTENTS

THURSDAY, JANUARY 2, 1879

ROBERT DICK OF THURSO

Robert Dick, Baker of Thurso, Geologist and Botanist. By Samuel Smiles, LL.D. (London: John Murray, 1878.)

BLEAK and bare, flat and featureless, the county of Caithness lies apart at the far end of Scotland, separated on one side from the rest of the country by rugged mountains and girdled on the other sides by boisterous seas—an unlovely region of brown moor and black morass, partially redeemed to agriculture along the sea-board, but so swept by storm and salt-spray that trees will not grow, save in a few sheltered spots where they have been carefully screened. The solitary mountain group of Morven and the Scarabins, visible from every quarter, lies at the far southern limit of the county, where it seems rather to be part of the uplands of Sutherland, to which indeed in structure it belongs. One redeeming feature, however, can be claimed for Caithness. It is one which compensates, or even more than compensates for the general monotony. The coast-line is almost everywhere formed by a range of mural precipices, rising here and there to heights of 200 and even 300 feet above the waves. Huge massive quadrangular sea-stacks tower out of the water in advance of the main cliff. The sea, moreover, runs inland in innumerable deep dark clefts or "gyoes," and is ever booming in the far recesses of caves that have been worn out of the solid rock by the chafing tides.

The monotony of scenery corresponds with, and indeed depends upon, the sameness of rock underneath. From one end of the county to the other the same interminable dark-grey flagstones in gently undulating beds underlie the scanty soil and peaty morasses. It is these rocks too which, truncated by the sea, run out boldly into headland after headland, or retire into sheltered bays and there extend in reefs upon the shore.

Over the wide Caithness plains the roads run in straight, unvaried lines for miles together. A curious consequence is alleged to be traceable in the physiognomy of the inhabitants. Two acquaintances, advancing along a road from opposite quarters, begin to recognise each other some time before they can actually meet. The smile of recognition is thus prolonged and fixed, so that the people are said to wear a characteristic Caithness grin.

A more unpromising field for the development of natural history tastes it might seem at first somewhat difficult to find within the compass of these islands. No lover of flowers is attracted to settle where short chilly summers and long damp stormy winters make up the year. And where flowers are few insect life will be scanty. Nor is the assemblage of birds likely to be varied where there is neither bush nor tree on which to perch or nest. The waters of these northern seas offer undoubtedly the greatest prospect of reward to the naturalist. They teem with life. Their plants and animals are cast up on the beach by every storm. Every pool on these rocky shores may be made a subject of patient and delightful study.

It was into the midst of these scenes that in the summer of the year 1830 fortune cast Robert Dick, then a young man of about twenty years. His life had not been altogether a happy one. The son of an officer in the Excise, he had received the ordinary education of a rural district in Scotland, and had shown such aptness at school that there was a proposal to send him to college with a view to his entering one of the learned professions. His father, however, married a second time. Robert's position at home eventually became so uncomfortable that at the age of thirteen he was glad to escape from the paternal roof and become apprentice to a baker in his native village of Tullibody, at the foot of the Ochil Hills. During his school days, and still when employed in distributing bread through the district, he developed an intense love for nature, which remained the master passion of his life. Flowers were his special study in these early years. He knew them in their abode in every bosky dell of his native hills, though as yet he had been able to learn little regarding their scientific names and classification.

With this yearning after wild plants and the scenes amid which they grow Dick came to Thurso (whither his father had already removed) and established himself there as a baker on his own account. His business, however, though he gave very diligent heed to it, did not afford occupation for more than a small part of his time. He was accordingly left with plenty of leisure for making himself acquainted with the natural history of his new home. The sea-shore naturally first attracted him. He wandered for miles along the coast, and collected such shells as he could procure from the beach. But he seems never to have thrown himself with zeal into the study of the marine fauna. His eye, indeed, was ever open, and, with the instinct of a true naturalist, he could recognise the value of facts in departments of knowledge with which he had little practical acquaintance. He ransacked the moors and mosses for beetles, bees, butterflies, and moths, gathering every species and variety he could find, and forming in this way a tolerably complete collection of the insect fauna of Caithness. Eventually he gave himself up, heart and soul, to the flora of the county, traversed on foot every parish and moor from end to end, and formed a herbarium containing not only each species of plant, with its locality and habitat carefully affixed, but also many singular and interesting varieties. He watched the vegetation from season to season, was familiar with the haunts of every species, knew when and where each began to show the earliest buddings, traced out the geography of the flora, and marked on what kinds of soil or rock particular varieties were to be found.

It was not until some years after his settlement in Thurso that he began to look into the rocks of the sea-shore. He stumbled upon scales of fishes imbedded within them, which greatly excited his wonder. Further examination brought to light abundant bones and plates, such as he could not find described in any accessible book. He began to collect these fossils, noting particularly the circumstances under which they occurred in the rock, and endeavouring to work out, as well as he could, their peculiarities of structure.

The way in which Dick found time for long excursions, without in anywise neglecting his business, brings out

the wonderful energy and enthusiasm of the man. He would bake his daily supply of bread in the early hours of the morning, have it ready for sale by his faithful housekeeper, and start off himself long before even the earliest riser in Thurso was out of bed. Often would he leave home about midnight, taking advantage of moonlight, and cross the county to reach some special ground for observation by daybreak. Yet he would always get home again in time for the preparation of next day's baking. In this way he would walk sometimes sixty miles or more in a single journey.

Of course such a man could not escape criticism in a small town where everybody knew everybody. And Dick's personal appearance not less than his singular occupations made him a "character" in Thurso. At dusk a tall figure with chimney-pot hat, swallow-tailed coat, jean trousers and travel-stained boots, usually with some bundle of stones, ferns, grass, or what-not, might be seen marching with a swinging pace towards the bakehouse in Wilson's Lane, and the neighbours would watch him as he passed, shrugging their shoulders, and wondering where the poor eccentric baker had been wandering this time. There was no congenial society for him in the place. Though naturally of a sunny hopeful temperament the bitterness of his early life had in some measure soured him; or at least had made him shrink within himself, avoiding the society of others, and finding his companionship among the flowers, mosses and rocks out of doors, and with his books at home. He was a diligent reader, not merely of such books on his favourite pursuits as he could afford to purchase, but of general literature, and in particular of poetry. He had considerable aptitude in quotation and availed himself freely of the gift in his letters. He taught himself drawing, also; turning the acquisition to account not only in the delineation of the objects of natural history which he encountered, but in such excursive subjects as Egyptian antiquities and classical figures, with charcoal outlines of which he would at times adorn the walls of his bakehouse. His artistic taste led him too to procure always the best edition of a book and to put it into the best style of binding.

Dick made Hugh Miller's acquaintance when that eminent writer was at the height of his reputation. There was much in the history and characters of the two men to draw them together. The one had told the whole world his story and had enlisted the sympathy of every reader in the pursuits that had made him famous. Dick on the other hand shrank from notoriety. He told his friend all he knew, showed him all his collections made him welcome to the use of everything, and took him over the ground whence he had quarried many a rare fossil. Such generous help could not but meet with fitting public acknowledgment from its recipient. "He has robbed himself to do me service," said Miller, who fully and frankly stated the nature and extent of his obligations; and then for the first time the geological world heard of the labours of the baker of Thurso. Dick, sitting by his own oven-mouth and reading the allusions to himself in Miller's paper, blushed to find himself thus in print, and begged that he might not be so often mentioned by name: "Leave it to be understood," he writes to Hugh Miller, "who found the old bones; and let them guess who can."

Nevertheless, like many self-taught men, Dick with this avoidance of publicity, united not a little vanity. He was proud of his humble position, and contrasted it with that of the "gentlemen-geologists," who could never do any good work for fear of soiling their clothes. He was proud of his prowess as a pedestrian, losing no opportunity of telling his friends how many miles he had walked and how many hours and minutes he had been on the tramp, while the "gentlemen-geologists" would have been in bed or lazily driving over the ground in gigs. He was proud of his achievements in science, of his power of seizing and sifting facts, of the collections which he had made, of the opinions he had formed. He had indeed much to justify this egotism, though few except the very limited number admitted into his intimacy, knew how much. Hence to ordinary acquaintances, or casual visitors he seemed morose, sarcastic, almost contemptuous. But above these little idiosyncrasies stood out bright and clear, his purity of character, his generous unselfishness, his enthusiasm for nature and his stern conscientious devotion to the daily drudgery of his business. His life was on the one hand a struggle with poverty, and on the other an exuberant communion with the works of God. After fifty-six years of such a life he died, leaving as the result of all his toil just enough of money to defray his debts.

It was well that such a story should be generally known. Dr. Smiles deserves our best thanks for having rescued it from oblivion and thrown it into the form of a volume, made up largely of Dick's own letters. It was fortunate that so many letters could be recovered, for otherwise, as Dick never published anything, and his friends were few, it would have been hardly possible to gather details enough for his biography.

The author has doubtless tried to do his best with the materials at his command, and nobody but he can know the difficulty of his task. But, in spite of the interest of his subject, he cannot be congratulated on having fully sustained in this new venture his well-earned reputation as a biographer. He appears to have taken up the life at intervals sufficiently removed to allow him to forget what he had already written, so that he repeats the same idea, sometimes almost in the same words. We are told three times, for instance, that there is no land between Thurso and Labrador, and twice within the space of three pages that Dick was a favourite with the children of his employer. Dick's habit of Sunday walking is referred to in Chapter XII., and after the lapse of more than a hundred pages it comes up again as if it had never been spoken of before. His baking operations, and how he carried them all on himself, are not likely to be forgotten by any reader of the volume.

More serious fault must be found with the inaccuracies of the book. The author states (p. 98) that "distinguished geologists had asserted that no fossil remains were to be found in the Scotch Highlands," and in support of this assertion he quotes a passage from Conybeare and Phillips's "Geology of England and Wales." The statement is meant to mark the importance of Dick's supposed discovery of fossil fishes in the Old Red Sandstone of Caithness. But a more unfortunate confusion could hardly have been made. In the first place, Caithness is not part of the Scotch Highlands. Geologically and

ethnographically it is a portion of the northern lowlands peopled by Scandinavian colonists. Again, while it is true that the rocks of the Scotch Highlands are with rare exceptions unfossiliferous, no geologist for half a century or more has said that those of Caithness are so also. Dr. Smiles, in repeating, amplifies his assertion (p. 245) and blunders still more; for this time he makes Dean Conybeare the author of the astounding statement that "the rocks of *Scotland* are unfossiliferous!" and drags in Sir Roderick Murchison, "who took the statement on trust," and "many writers" as propagating the delusion. A third time he refers to the subject, when (p. 238) he says "Robert Dick discovered numerous remains of fossil fishes in Caithness where distinguished geologists had stated that no fossil fishes were to be found." How he could print these sentences in the same volume with the letter from Murchison given on p. 275, it is hard to understand. In that letter Murchison speaks of himself as an old geologist who had written upon the Caithness fishes thirty-two years before. In truth, that geologist and his companion Sedgwick had found abundant fossil fishes in Caithness and had published an account with drawings of some of them, while Dick was still an apprentice carrying bread among the villages of the Ochils. No fact in Scottish geology was more familiar than that the flagstones of Caithness abounded in fossil fishes. That Dick should have been filled with surprise when he found them, only shows that he had not had opportunities of learning what had already been done in the district.

Again, Dr. Smiles refers to a remark of Sir Charles Lyell's that "very few organic remains had been found in the boulder-clay and especially in the till, throughout Scotland." It would seem as if he were quoting from a letter of Dick to Hugh Miller; for a passage from this letter follows, showing that the writer had found fossils in the boulder-clay almost in every place where he had looked for them. And the reader is left to draw the inference that Dick in testing Lyell's statement by an appeal to nature, had found it to be incorrect. But it remains absolutely true to this hour. The boulder-clay, as a whole, is singularly barren of organic remains. In one or two exceptional places, and Caithness is one of them, it is full of fragmentary marine shells. Dick's observations were quite accurate; but it was not necessary to enhance their importance by showing that they contradicted the published statements of so distinguished a writer as Sir Charles Lyell.

But the most serious defect of all is one with which it is somewhat difficult to deal. Every reader of the book will recognise that its preparation has been a labour of love. Dr. Smiles has wandered over all the scenes of Dick's rambles, has tried to realise as vividly as possible the circumstances and surroundings of the enthusiast's life, has recognised his devotion to the acquisition of knowledge, and has written with the most heartfelt sympathy with Dick's love of nature and the struggles and trials of his position. And yet one feels that into the spirit of the researches which formed the bright side of Dick's lonely life, which cheered him and furnished him with mental food and recreation from beginning to end, the writer of the Life hardly enters at all. It was more than a mere love of nature which carried Dick so buoyantly through his monotonous drudgery; more than the mere pleasure of finding flower, or insect, or fossil in its native habitat, and bringing it home to enrich his collection. We have glimpses of this in his graphic letters, and a reader who knows something of the contemporary history of scientific progress, can read between the lines of these letters and find in them an interest tenfold greater than they can possibly have without this information. The want of such assistance to an ordinary reader must make the letters somewhat monotonous, and give the impression that the book is unnecessarily long. When he reads, for example, Dick's account of his numerous and laborious traverses of Caithness in search of sections of boulder-clay, he will naturally ask the object of these toilsome journeys, what was to be gained from them, and what in actual fact was gained. It would have increased his appreciation of these labours to have learnt something of the problem to the solution of which Dick set himself, and he would have been the better able to realise the eager enthusiasm which led that votary of nature to cross the county on foot at night to get to his boulder-clay scars by daybreak. It would have heightened the reader's respect for the subject of the biography to have been shown how, if the results briefly sketched in the letters now published, had been given in detail to the world a quarter of a century ago, they might have placed the name of Robert Dick among the pioneers of glacial geology.

But of all this we learn nothing from the memoir. Dr. Smiles sums up Dick's character and points the moral to be drawn from the story of his life. But what was the outcome of these long years of indefatigable labour? Apart from the man himself, what did his work advantage the world? It is, indeed, a worthy thing to have lived a life that may serve as an ensample and encouragement to after generations. Dick did that nobly. But he did more. He felt that he had "done the State some service." Though he never published his knowledge he worked incessantly and freely communicated his stores of information to others. Much of that knowledge died with him. Yet from his letters, his scientific collections, the published references to the assistance freely given by him to fellow-workers in science, and the recollections of his contemporaries, it might have been possible to have given at least an outline of what he had achieved. Such a sketch would have been a fitting tribute to his memory, a recognition of the meaning and value of those long years of solitary toil.

In an interesting and genially written episode, Dr. Smiles sketches the career of another, but still living enthusiast in natural history—Charles W. Peach, who was one of Dick's most intimate friends, worked with him among the Old Red Sandstone fossils, corresponded, argued, battled with him over their respective opinions. But here again the writer's general sympathy with a heroic struggle for the acquisition of knowledge betrays no special interest in or acquaintance with the life-work of his hero. Unwittingly, therefore, he is led to do but scant justice to his subject. From the allusions, for example, in Dick's letters and elsewhere, to a discussion between that dogmatic observer and Mr. Peach regarding fossil wood, no reader could guess what a momentous point in the history of the Old Red Sandstone of Caith-

ness was really in dispute, and how much Peach's observations went to settle it. No one reading the volume, with its account of Dick's hammerings and Hugh Miller's visits and writings, could surmise that in the palæontology of the Old Red Sandstone of Caithness Peach has done far more than Dick, far more than Hugh Miller, more, indeed, than all other geologists put together.

The illustrations of Caithness scenery, plentifully interspersed throughout the book, are well engraved, and, on the whole, very faithful and characteristic. Nothing could be better than the Deil's Brig of Scrabster Bay. We see the very lichens quivering in the gusts that blow for ever through that hideous cleft, and we hear the screams of the northern sea-fowl as they wheel in restless circles from the neighbouring Clett. In transferring the author's sketches to the wood, however, the artists have taken a few liberties which would have roused poor Dick's indignation. Dirlet Castle, which stands on a rock some twenty or thirty feet above the stream, is raised at least 300 feet into the air; and dear old Morven, glorified into a second Matterhorn, is placed just opposite to Dick's contemptuous ridicule of what the books say about the hill—"None of the hills are as big as books make them"—"downright nonsense! Morven is accessible on every side."

ARCH. GEIKIE

TELEGRAPHY

Instructions for Testing Telegraph Lines and the Technical Arrangements of Offices. By Louis Schwendler. (London: Trübner and Co., 1878.)

THE criterion of the good working of a line of telegraph is its freedom from interruption. Interruptions to the communication are technically called "faults," and on our overground lines men are stationed at certain intervals for the express purpose of patrolling these lines and removing defects from them that sooner or later might culminate in faults. Of course accidents, such as those arising from snowstorms and violent winds, cannot be prevented, but most of the interruptions that are met with in practice can by proper supervision be eliminated before they can arrive at such a condition as to interfere with the communication. In telegraphy more than in anything else, "prevention is better than cure," and for many years past all our telegraph engineers who have devoted their attention to the proper maintenance of telegraphs have striven to devise as perfect a method as they can for detecting the presence of faults and for establishing an accurate system of testing.

It is, however, upon our submarine cables, not only in their manufacture but during the process of laying, and whilst subsequently working, that the greatest skill and ingenuity has been employed to devise a perfect system of testing.

The first rational mode of testing our cables was introduced by Dr. Siemens, but Mr. Varley had previously introduced into the service of a Telegraph Company a very elaborate system of testing by the aid of differential galvanometers and resistance coils. Rheostats or resistance coils had been invented by Wheatstone as far back as 1843, and Sir Charles Bright and his brother, Mr. Edward Bright, had introduced them into use on the Magnetic Telegraph Company's system. It was, however, in the telegraph companies' service that the system was to a certain extent perfected, and when all the systems of the different companies were concentrated into the hands of the General Post Office the system became universal for the whole country. We cannot think that Mr. Schwendler, when he asserts that no really practical system of testing had been adopted by any other telegraphic administration than that of India, could have been aware of the perfect system in use by our English administration, and it is a pity that he has not embodied in his book a description of the system in use in England. This perhaps is unnecessary, because it is fully detailed in the "Handbook of Practical Telegraphy," by Mr. Culley, and in the textbook of science on "Telegraphy," by Messrs. Preece and Sivewright. Moreover, there is an excellent little "Handbook of Testing" detailing not only the practice on land lines but on cables also, by Mr. H. R. Kempe, and with another capital little book by Capt. Hoskiaer, on "testing cables," as well as a work on "Electrical Measurements," by Mr. Latimer Clark, leaves very little to be desired on the literature of the subject. Mr. Schwendler really adds little or nothing to our knowledge of the subject, and his book is only valuable as an indication of what has been done in India.

Great strides have been made in the Telegraphic Department in India ever since the accession to power of the lamented Col. Robinson. There is, according to Mr. Schwendler, a large staff of officers available with a first-rate general education and with a strong desire for improvement, and many of them are well trained in conducting physical experiments. It is to be hoped that their education is sufficiently advanced to enable them to follow the rather intricate mathematical developments of Mr. Schwendler. If his book has a defect it is that it is overloaded with mathematical investigations. There is no necessity to appeal to laboured formulæ when simple observations alone are needed to interpret phenomena. The mathematician loves his formulæ as a hen her brood, but the practical man prefers to kick them aside when he can do so and when he can do without them. Now, at p. 16, Mr. Schwendler gives no less than six elaborate formulæ, one of which must be selected for each particular condition to enable the tester to discover the value of any foreign electromotive force that may be in the circuit, the result of what he calls a "natural" current. Now there is no necessity whatever for any formula. The elimination of earth currents in cable and land testing is of daily and constant occurrence, and it is only necessary to compare the deflection upon any galvanometer given by the earth current with a deflection produced by one cell through similar resistance to find its value. Readings by reversals when taken rapidly always give a mean that is approximately true, for an earth current rarely varies so rapidly as to introduce any sensible error. His formulæ for eliminating the electromotive force when measuring with a differential galvanometer simply appal one.

Mr. Schwendler wisely says, "however much testing may become routine by continual practice it *will* always and *should* always partake of something of the nature of a physical experiment which must be conducted with a perfectly clear understanding. Then only can the tester draw the right conclusions from his observed facts; then

only can testing become a real benefit to the administration."

Again he says, "We know quantitatively the electrical state of the lines at all hours of the day, and seasons of the year; we are able to localise faults of all kinds very accurately and repair them with despatch; we test all our telegraphic material, and by it have greatly improved its essential qualities; we are not groping in the dark any more—we *measure* and *know*."

It never must be forgotten that testing is in reality a physical experiment, and these physical experiments are being conducted every day throughout the whole of our English telegraph system. Our cable electricians under the guidance of Sir William Thomson have carried this system of physical experiment to a high standard of perfection, and our Indian friends would do well to profit by their teaching.

Mr. Schwendler's explanation of the theory of the bridge is not clear, nor does his use of Kirchhoff's corollaries to Ohm's law much help the student. Indeed it is very doubtful whether his proof that the sensibility of the bridge method is greatest when the branch and the resistance are equal is true. At any rate in our practice we find that the more delicate the galvanometer of the bridge the more sensitive and the more accurate is our test.

The most valuable portion of Mr. Schwendler's book is his abstract of Ohm's classical paper, a translation of which is to be found in Taylor's "Scientific Memoirs," and also in his account of Kirchhoff's corollaries to this law.

The practice generally of line testing and testing for faults contains nothing new, but his chapter on natural currents, showing the effect of polarisation of earth plates and the presence of earth currents, is interesting.

He says, also "*Defective insulation at a few points* in a line is a fruitful source of currents. At all such points polarisation is produced by the working currents, in a manner precisely similar to that of the earth plates, by the same cause already alluded to, and to a degree dependent on the resistance and the position of the faults. These currents will be strongest in rainy weather, when the line is in contact with trees, when the insulators are covered with dew—in fine, under those circumstances which diminish the resistance of faults and promote electrolytic action.

"The stronger the working currents used, and the fewer the defective points, the stronger will be the polarisation currents.

"If these currents become very strong their direction may be reversed by sending for a short time a strong current with zinc to line; and, in such a case, this invariably indicates a single fault in the line or cable." This is a defect which we do not experience in England.

We find that (p. 66) "on all the lines in India positive signalling currents (copper to line) are used in order to have the greatest possible insulation of each line under all circumstances. Now, when measuring the insulation of a line with a positive test current, it is evident that the value obtained must give the insulation much too high, *i.e.*, higher than the line actually has when signals pass through it; because the signalling currents can only have a comparatively small oxidising effect on the line, since only a very small part can escape to earth in the different points of the line, while a positive testing current, the further end of the line being insulated, must all escape to earth at the defective points of the line. Again, when measuring the insulation of a line with a negative testing current, we get a value which gives the insulation of the line much too low, because negative signalling currents are never used. In the absence of any known law, which would give us how much too high the insulation of the line is obtained with a positive testing current, and how much too low with a negative testing current, we can do nothing better than to take the arithmetic mean of the measured values as representing the insulation the line probably has when signals are passing through it. Of this mean it may, however, be said that it must be always somewhat too low, for the very reason that negative signalling currents are never used, and therefore the arithmetic mean again of the *first mean* and the *positive measured value* would represent a value most probably approximating to the one which the line actually has when signals pass, and which alone is of practical interest and consequence to be known."

The latter part of the book is devoted to fault testing, *i.e.*, to the localisation of the positions of faults.

The book itself is a very valuable addition to the literature of the subject, but we doubt whether it will be of any practical use to our English electricians.

OUR BOOK SHELF

Sketches of Wild Sport and Natural History of the Highlands. By Charles St. John. Illustrated Edition. (London: Murray, 1878.)

MANY of our readers must be familiar with the inimitable "Sketches" of St. John, which has long ago achieved the position of a classic for both the sportsman and the naturalist. We do not know of any descriptions of sport to equal those that abound in these pages, in truthfulness, vigour, and genial humour. To the naturalist who loves to know the habits of an animal in its native haunts, the book must be a treasure; and now that Harrison Weir, Whymper, Corbould, Collins, and Elwes have adorned it with their art, the book should become a greater favourite than ever. No artist equals Whymper in his faithfulness to life in drawing animals. Every picture in the book—and there are about eighty of them—is a masterpiece in its way, and an impressive lesson in natural history. We need only say that the engraver is Mr. J. W. Whymper to convince our readers that the artists' charming work has been faithfully and skilfully rendered. No one can read a chapter of the book without being both refreshed and instructed.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Paradoxical Philosophy

IT is strange to see a writer on philosophy like Mr. S. H. Hodgson, as well as physicists so exceptionally able as Prof. Clifford, and now Prof. Clerk-Maxwell, falling into the same *errors of observation* as more ordinary mortals. Neither the authors of the "Unseen Universe," nor any of the members of the Parodoxical Society, have, so far as I am aware, expressed the

notion that the invisible order of things which continuity requires as antecedent to the visible order, is in any sense *material.* They only assume that it must be conditioned. Indeed, the authors of the "Unseen Universe" have expressed this conviction *in the preface to the second edition of their work, in italics, and in language that is not only exceedingly clear, but also extremely strong.*

But it seems to be taken for granted on all sides that a man of science can only imagine a mechanical unseen. This is really very hard.

The analogy (however inadequate) furnished by Thomson's vortex atoms, and the invisible fluid which they postulate, is too good an *illustration* of a novel and difficult conception to be disregarded; but it will have to be laid aside at once, if it can be shown to be necessarily productive of such extraordinary misconceptions even in intellects of the highest order.

HERMANN STOFFKRAFT

Schloss Ehrenberg, Baden, December 25, 1878

Force and Energy

SINCE a year or two back, when Herbert Spencer started, in the columns of NATURE, a discussion as to the real meaning of the word "force," most careful-thinking students of mechanics have probably come to the conclusion that either the use of the word "force" must be discontinued as a physical scientific term, or that it must be defined in a different manner from that adopted almost universally by those "doctors" whose writings seemed to weigh so heavily on the brain of "poor Publius." They all agree in saying that in its physical application the word "force" means that which produces *i.e.*, the CAUSE of change of momentum. It is needless to give quotations. They are all, except one, curiously explicit. Germans, French, and English agree. "So sehen wir diese Aenderung als Wirkung irgend einer in demselben thätigen Ursache an; diese URSACHE nennen wir Kraft." (Ritter's "Mechanik," p. 36). "On donne, en general, le nom de FORCE à la CAUSE quelconque qui met un corps en mouvement, ou seulement qui tend à le mouvoir." (Poisson: "Traité de Méchanique," Introduction, p. 2.) Although in Tait's "Recent Advances" we find on p. 11 "that we have not yet quite cast off that tendency to so-called metaphysics which has so often blasted," &c., &c.; yet on p. 16 of the same book there is reproduced the fine old crusty Newtonian maxim to which Thomson and Tait and Tait and Steele cling with such fond reverence: "force is any CAUSE which," &c. Clerk Maxwell gives no formal definition of force in his "Electricity and Magnetism." On p. 5 he simply gives its dimensions. On p. 83 of his invaluable "Theory of Heat" he defines, "force is WHATEVER changes or tends to change," &c. This is a very ingenious mode of escaping the difficulty by simply giving no definition at all. We are told what the result of force is, but not what force itself is. We are told that force is "whatever," which is not very clear. Jeames would hardly think that justice was done him if we asserted that the complete definition of him was "whatever opens a door," and made no mention of the fact of his humanity or of his grand plush breeches. It is, in fact, a confusion between a statement of the mode of measuring quantitatively the force, and the definition of the force itself. A physical definition should certainly show clearly what the proper way of measuring the quantity is; but this latter is not the definition itself. Moreover, there may be different almost equally good modes of measurement, all leading to the same numerical result. Clerk Maxwell's definition is clear as to *a* mode of measuring force, but furnishes absolutely no information as to the nature of the thing intended to be defined. It, therefore, differs from the others in that they are real metaphysical definitions, presumably comprehensible to those who understand metaphysics, while his is no definition at all. Prof. John Perry, in his book on "Steam," adopts the same device as Prof. Clerk Maxwell, substituting the word "anything" for "whatever." Rankine forms a remarkable exception. He says that "force is an action between two bodies either causing or tending to cause change in their relative rest or motion." Here the word "cause" is used in such a sound, practical, common-sense way that no one could take exception to such use of it, even in a physical definition, and probably "action," as here used, might be explained clearly enough for all useful purposes as "a changing relation" or "a change of relation." Rankine, however, does not take the trouble to do this last.

Now clearly a cause is a metaphysical entity, if it is an entity at all, and from the very nature of the difference between metaphysics and physics, a metaphysical entity cannot possibly be made of any use in physical investigations. If, then, the word force is to be usefully employed in physics, it must be defined as something else than a "cause." When we talk of forces, the physical facts the observation of which we think of, are accelerations of momentum; and in his Glasgow lecture Prof. Tait seems half inclined to use "force" and "acceleration of momentum" as synonymous terms. But an acceleration of momentum is a function of one body only; and every one knows that what is mentioned in Rankine's definition is true, namely, that force is a function of two bodies, and can have neither objective nor any other kind of existence except as a relation between two bodies. Seeing that it is so, I beg to lay before your readers for their favourable consideration the meaning of the word force which I have used for several years past. I wish force to be defined as "time rate of transference of momentum." A transference of anything can only take place between one body and another, and in the transference the amount transferred from the first body to the second is necessarily equal to the amount transferred to the second body from the first. This might seem to be such a truism as to be a mere repetition of words; but we must remember that it is the law of motion which the "transcendently lucent" Newton discovered from his extensive physical experience; and, in order to discountenance scepticism, we might add, by way of parenthesis, that during the transference no spilling takes place.

"Poor Publius" might thus get a hint that there is such a physical fact as conservation of momentum which is independent of all formal definitions. If momentum is conserved, *i.e.*, if it has an enduring existence so that at one time there is no more nor less of it than at another, then during a direct transference of some of it from one part of the system in which it is lodged to another part, the amount lost by the one part must evidently be the same as that gained by the other part. Thus an acceleration or time-rate of gain of momentum to one part necessarily implies a simultaneous equal time-rate of loss of momentum from another part, and also a simultaneous equal rate of transference of momentum from that other to the first part. All these three rates have directions inasmuch as they are time-rates of directed quantities. The first is a rate of gain of momentum, which momentum has a certain direction. If that direction be reckoned positive the gain is one of positive momentum, and the acceleration is naturally reckoned as positive. The second is a rate of loss of momentum of the same direction, *i.e.*, a loss of positive momentum which is equivalent to a gain of negative momentum, and therefore this time-rate is naturally reckoned negative. The meaning of this is simply that the proper physical sign to ascribe to acceleration of momentum is the directional sign of the momentum gained. The two opposite signs of the above two rates have given rise to the idea of two equal and opposite forces acting between the bodies. If the forces were located IN the bodies and not BETWEEN them, the phraseology would be consistent with Tait's definition of force as simply "acceleration of momentum." But I do not hesitate to say that this idea of force is quite unnecessarily out of accord with the commonly received notion of force as a mutual action or relation between TWO bodies, because in this view force would distinctly have reference to only one body. If, however, we use force to mean the transference of momentum, there is, of course, only one force between the two bodies. The question is what sign is to be given to this force, and it is not quite easy to answer. Force is in this view a flux, a rate of flow of momentum. This flow takes place in a certain direction, and it is the flow of a directed quantity. Are we to take the direction of the flow or the direction of the momentum that flows, to determine the proper sign of the force? These two directions need not be the same. Thus in a bar subjected to tension the flow of momentum is in the direction opposite to that of the momentum itself. In a bar in compression the flow of momentum takes place in the same direction as that of the momentum. In a mass subjected to shearing stress the direction of the flow is perpendicular to that of the momentum. In the case of the attraction of gravitation between two bodies the direction of the flow of momentum is always the exact opposite of that of the momentum that flows from one to the other in whatever way the two may be moving. In the case of impact if we take the direction of the flow of momentum as that of the perpendicular to the surfaces that touch during impact drawn from the body that loses momentum towards the body that gains momentum, then this direction of

flow may make any angle within certain limits with that of the momentum exchanged. If we are to adhere strictly to the ordinary conventions with regard to the directions of forces, it is clear that we would need to consider the transference of momentum which we term force, only with reference to the direction of the momentum transferred, and without any reference to the direction of transference. It is, however, evident that this latter direction is of very great importance in physical investigation, and it is a matter worthy of serious consideration whether or not force should not be considered a two-directional quantity, one into whose definition two directions enter. Impact is a difficult subject, perhaps, just because of the large possible variation of the difference of these two directions. All other forces (rates of transference of momentum), except those involved in impact may be divided into three simple classes corresponding to compression, tension, and shear.

In considering stress, the phrases "transmission of momentum" and "rate of transmission of momentum" are as convenient, perhaps, as the corresponding phrases with "transference" substituted for "transmission."

The most obvious objection to this definition of force is that a force may be applied to a body, and yet it receives no momentum. The objector would probably say that though the force be applied, yet there may be no momentum transferred to the body. But this would be quite wrong, as can be most easily recognised if Prévost's theory of exchanges of heat by radiation and the similar theory for conduction of heat be recalled to mind. A body may quite easily have simultaneously equal amounts of opposite momentum transferred to it. These will balance, and its centre of gravity will suffer no acceleration of velocity. This remark will make it evident that the theory of force gives an easy and unhesitating answer to the much-debated question as to whether there are really such things as unbalanced forces. A transference of momentum between two bodies may just as readily be unbalanced as balanced. Let us consider this balancing of transferences of momentum more particularly. Let a body have momentum transferred to it by the pressure of another body upon a certain portion of its surface. This can be balanced in different ways. It may be balanced by a perpendicular pull applied to a portion of the surface parallel to that to which the pressure is applied, and facing the same way, *i.e.*, on the same side of the body. The directions of the momenta transferred at these two surfaces are the same, but the directions of transference or flow are opposite. Or the pressure may be balanced by an oppositely directed pressure upon a parallel surface facing the other way. In this case the directions of the momenta transferred at the two surfaces are again the same, while the directions of flow are also the same. In all cases when a body is kept in balance by transferences of momentum going on through its different surfaces, it is evident that for any amount of momentum of a given direction transferred into it at one surface an equal amount of momentum of the same direction must be transferred out of it at some other surface. The directions of transference or flow at these two surfaces may be relatively any whatever—they are quite independent. The balance of the body, with respect to the velocity of its centre of inertia, is quite uninfluenced by the directions of the momentum-flows through its different surfaces. But evidently the state of stress and strain throughout the interior of the body depends a great deal upon the relative directions of these flows as well as upon the relative positions of the surfaces.

But, as regards the direction of the momentum, it must be remembered that this depends upon what we arbitrarily choose to be our standard positive direction, whereas the equilibrium of the body acted on certainly does not depend in the least upon that arbitrarily chosen direction. Thus, as in the above example, let the body 2 be kept in equilibrium by the equal and opposite pressures of the bodies 1 and 3 on its opposite faces. The question is whether momentum is being transferred from 1 to 2 and from 2 to 3, the momentum transferred having also this direction; or whether both the flow and the momentum flowing have exactly the opposite direction, viz., from 3 through 2 to 1. If we have a standard positive direction to go by, and if 1 is not in equilibrium, but is being stopped in its motion by impact on 2, then the above question is easily answered at once. But if 1 is in equilibrium as well as 2, we must, in order to answer the question, look beyond 1 to find out the direction of the other transference of momentum, which, along with that between 1 and 2, keeps 1 in equilibrium. If this other transference takes place between 1 and another body which is again in equilibrium, it would be necessary to go back still another step in order to find out in which direction the flow is really taking place. If the whole system of which these bodies form parts is everywhere in equilibrium, *i.e.*, all the parts at rest relatively to each other, we would in this way travel from one body to another in a complete circuit in search of some point which would disclose the real direction of flow, but without ever coming to any such point. Because, following round the circuit, we would again come back to 3 and 2 and 1. The choice of a standard direction as the positive one does not help us in the least to come to even a formal conclusion. We remain, however, sure of two things—first, that there is really a continual flow of momentum taking place all round this circuit in the system; and, secondly, that the direction of this flow is at some places, which we can definitely specify, in the same direction as the momentum transferred, and at some other places, equally easily specified, in the opposite direction. Take as an example a piano. We may suppose the upper horizontal bar of the frame to which the strings are attached to be continually losing upward momentum, which is being continually received by the top parts of the tightened strings. This upward momentum the strings are continually transmitting downwards from particle to particle, and at the foot of the strings it is delivered to the bottom horizontal bar of the frame. This bottom bar transmits the upward momentum horizontally, each section being in shear, to the vertical sides of the frame. The transmission down the wire is in the direction opposite to that of the momentum transmitted; in the horizontal bottom bar the direction of transmission is perpendicular to that of the momentum, through the sides this momentum flows upwards, that is, in the same direction as the momentum itself, and finally, it is transmitted again horizontally through the upper bar to be redelivered to the strings. This explanation is completely satisfactory in accounting for the conditions of strain of the various parts of the piano. But to explain these conditions an equally satisfactory hypothesis would be that a stream of downward momentum is continually circulating through the piano in the same circuit as the above, but in the opposite direction round that circuit. Or again we might suppose two opposite circulations to be continually going on, one of upward momentum and the other of downward momentum. But whichever of the three hypotheses we may adopt, we always have the flow in the string which is in tension opposite to the momentum flowing through it, and the flow through the horizontal bar perpendicular to the momentum, and the flow through the sides of the frames in the same direction as the momentum transmitted.

Which of the three is to be chosen, or is it of any consequence that we should know which should be taken? The question is not one that can be made to have any degree of unreality in appearance by merely measuring the motions relatively to one thing or another. It is not whether the momentum transferred is upward or downward relatively to the centre of the earth, or relatively to the sun or to the stars. It is, what is the direction of this momentum relatively to the centre of inertia of the piano frame itself, whether this relative momentum is directed from one end of the piano towards the other or from that latter to the former, and the answer to this question is quite independent of what we arbitrarily choose to call the absolute velocity of the centre of inertia of the whole structure.

I will venture to say that the correct answer is that there are two opposite streams of equal amounts through the structure. What is meant by equal amounts is, of course, that the opposite rates of transference through any section are numerically equal. The simplest and clearest proof is this very simple one: If there were only one stream circulating in one direction, since from the above it is clear that the momentum flowing along in this stream is at every point of it of the same direction, and since the stream is a continuous steady one, every part of the structure through which this stream flows would have the velocity corresponding to this momentum, and in consequence the centre of inertia of the structure would have a certain velocity in the same direction. The inconsistency of this result with the datum from which we started, namely, that the momentum transmitted was to be measured relatively to the centre of inertia, need not be pointed out. To look at the question in another way, let us only consider what this momentum, this thing that is being transferred from particle to particle, really is, viz., mass multiplied by velocity, and we cannot fail to come in a moment to the conclusion that these streams are simply streams of molecular vibration.

And since each particle maintains constantly the same average position relatively to the centre of inertia of the whole, it is evident that its alternate opposite displacements and velocities relatively to that centre of inertia must be numerically equal. A certain particle has first a certain velocity in one direction, and immediately afterwards has a numerically equal velocity in the opposite direction. This change cannot take place except by its transferring to the next particle the same numerical amount of momentum of one direction as it receives from that particle of momentum of the opposite direction. In this way constant streams run in the two opposite directions, the momentum flowing along one having the opposite direction to that flowing along the other, and equal numerical amounts of these oppositely directed momenta flowing past any given sections of the two streams per unit of time.

As a sort of parenthesis let me give the following symbolical statement of the foregoing. Let V be the velocity of flow of either of these two opposite streams, and μ the mass per unit volume of the material, and v the average numerical velocity of the particles. Then since at any given instant half the particles must be supposed to be moving in one direction, and the other half in the opposite direction, the amount of momentum of one direction passing per unit of time through any section of unit area of the correspondingly directed stream, is $\frac{1}{2} V\mu v$. A numerically equal amount of oppositely directed momentum is flowing per unit of time through the same unit section in the opposite direction. Observe that the material through which these two streams are flowing is in "balance," "in equilibrio." Suppose the one stream to lead out of, and the other to lead into, an unbalanced mass, which mass suppose not to be losing or gaining momentum except by these two streams. By means of the one it loses, say $(+\frac{1}{2} V\mu v)$ per unit of time. By means of the other it gains $(-\frac{1}{2} V\mu v)$ per unit of time. The amount of positive momentum it transfers to the balanced material for unit of time is, therefore, $+V\mu v$, and this is the rate of transference of momentum from the unbalanced mass to the balanced material, and through this latter. If the ratio of comparison or extension, *i.e.*, the strain of this balanced material, be called e, then what we usually call its modulus of elasticity is E given by the equation $eE = V\mu v$. If we insert in this expression the proper value of $V = \sqrt{\frac{E}{\mu}}$, the velocity of transmission of longitudinal vibration, we obtain a value of $v = e\sqrt{\frac{E}{\mu}}$, similar to that deduced by De St. Venant for the first stage of an impact, during which a single *unbalanced* wave of momentum is running forward through the body impinged on. But the important point to notice is that the rate of transference of momentum per unit area is the product of a mass per unit volume (μ) and of two velocities (V and v). In unbalanced transmission these two may be in the same direction, in which case the mass being accelerated is in compression, or they may be in opposite directions, in which case the accelerated mass is in tension; or they may be at right-angles, in which case the accelerated mass is in shear. In balanced transmission if in the one stream the velocity of flow is in the same direction as that of the flowing momentum, then also in the opposite balancing stream these two velocities have the same directions and the material is in compression, the strain being double that which would occur if either of these opposing streams existed by itself unbalanced in the material. Similarly for a balanced state of tension and for one of shear.

Considering these reasonings, does it not seem right to make the direction sign of the force, or rate of transference of momentum, the same as that of the product of these two velocities. The sign of the product of two vectors does not depend on the absolute direction of either, or rather it does not depend on the relation of either to what we arbitrarily choose as our standard direction. It depends only on their mutual relation. Thus we get a definite sign for each force not arbitrary, but real. For a compression force the two vectors have different signs, and their product is a multiple of -1. For tension, the two being of the same sign, their product is a multiple of $+1$. For shear, the two being perpendicular, their product is a multiple of $\sqrt{-1}$ or of $-\sqrt{-1}$. If the direction of transference be oblique to that of the momentum transferred, their product is the sum of a scalar and of a vector. In this case we have compound stress, that is a shear compounded with either compression or tension; and, as every one knows, it is usually convenient to consider the scalar and the vector parts separately. The question of the *mode* of transmission of momentum corresponding to these main kinds of stress is one of molecular mechanics, into which there is no need of entering here.

ROBERT H. SMITH

(*To be continued.*)

Leibnitz's Mathematics

IN perusing some old files of NATURE I came upon the following sentence in a letter from Prof. Tait (vol. v. p. 81) in reference to the invention of the Differential and Integral Calculus:—"Leibnitz was, I fear, simply a *thief* as regards mathematics." Prof. Tait has more than once intimated or expressed a similar opinion.

In reply to this imputation Dr. Ingleby says (NATURE, vol. v. p. 122):—"I do not object to the Professor calling a spade a spade; but I assure him that this charge is made just twenty years too late. It is exactly that time since the *last vestige* of presumption against the fair fame of the great German was obliterated. If Prof. Tait does not understand me, or, understanding me, disputes the *unqualified truth* of my statement, I promise to be more explicit in a future letter. But I incline to think the question is not susceptible of *proof* until the Council of the Royal Society, who so grossly disgraced themselves in 1712, shall do the simple act of justice and reparation required of them, viz., publish the letters and papers relating to this controversy, which since that date have slumbered in the secret archives."

Prof. Tait, as far as I know, never responded to the challenge, and I presume there is but one inference to be drawn from his silence.

In a late reading of an account of this controversy from the German standpoint, my interest in the subject has been reawakened, and I feel a strong desire to see the whole question thoroughly ventilated. Such a consummation must surely be wished by every fair-minded man, and in the name of justice I would ask Dr. Ingleby to be more explicit and do what lies in his power to remove the imputation which has been attempted to be fastened upon Leibnitz.

This question will not down at the bidding of any one, and the documentary evidence alluded to by Dr. Ingleby must sooner or later see the light. Let us have the matter at once and for ever definitely and honourably settled. A. B. NELSON

Danville, Ky., U.S.A., November 27, 1878

[It is not to be absolutely presumed that, when a busy scientific man lets pass such challenges, he has given up his point. The question has now lost all but a species of antiquarian interest:—still it is worth clearing up. We might begin by asking Mr. Nelson and other defenders of Leibnitz to explain the very singular appropriation which Leibnitz made of "Gregory's Series" *after* having acknowledged whence he got it.—ED.]

Commercial Crises and Sun-Spots

A SUGGESTION is made by Mr. John Kemp, in NATURE, vol. xix. p. 97, to test the relation of sun-spots to the variation in weight of the cereal grains. Probably the difficulties of giving such a test scientific precision are insurmountable. No doubt these grains do vary in weight from year to year. Of some samples of oats, of crop 1877, contributed by me to the South Kensington Museum, the pound contained 13,642 grains, while the pound of crop 1878 contained 16,870. But there are many varieties of oats, barley, and wheat in general cultivation, each producing grains differing in weight from the others. In an inquiry which I made regarding the weight of the *sterling*, average grains of wheat of crop 1876 from the south of England were found, in an air-dry condition, to weigh as follows: Talavera, 1·01 gr. troy; Chidham white, ·76; Sherriff's bearded, ·86; Kessingland red, ·92; Nursery red, ·76; Trump white, ·81; Red rivet, 1·00; Lammas red, ·89; Hunter's white, ·75. And different ears of a given variety of wheat have grains of different weight. If six or eight culms come up on one stool, the largest ears have the heaviest grains. In general, the larger flower-cups in an ear, contain the heavier grains. Then, there is scarcely such a thing to be found as a crop of one pure variety. Any variety rapidly gets mixed with others. And, supposing that a plot were set aside for a pure variety, year after

year, for a few cycles of sun-spots, the mineral conditions would be constantly varying; so that any test by the balance to compare the fruit of one year with that of another, would involve too many unappraisable elements to have a real value.

Prof. Jevons observes that his investigation is "embarrassed by the fact that no inquirer has been able to discover a clear periodic variation in the price of corn." But the quality of corn must be a more immediate effect of solar action than the price. Now, although perhaps not much is to be arrived at from the method suggested by Mr. Kemp, there is another direction in which something might be found, and in which the necessary data already exist. I allude to the records of those Corn Exchanges which contain full details of the measure-weight of every parcel of grain which has been sold in them for several sun-spot periods. I reduced the sales in the Haddington Corn Exchange for the year from July 3, 1868, to June 25, 1869, and found the average bushel-weights as under:—

Wheat ...	27,764 quarters	63·15 lbs. per bushel.
Barley ...	33,022 ,, ...	56·85 ,, ,,
Oats	16,223 ,, ...	43·49 ,, ,,

The sales in the Edinburgh Corn Exchange from November 4, 1868, to October 27, 1869, gave the following weights:—

Wheat ...	27,322 quarters	62·84 lbs. per bushel.
Barley ...	35,752 ,, ...	56·18 ,, ,,
Oats	53,843 ,, ...	42·28 ,, ,,

Reductions on this larger scale probably eliminate most of the elements of uncertainty. The measure-weight of the cereal grains depends on various factors, one of which is the comparative distension of the epicarp by the inclosed albumen. It is this element which may vary with variations of solar radiation. And if a cycle of measure-weight should be found corresponding with the sun-spot period, a clue might be gained to some unsuspected commercial relationship.

North Kinmundy, Aberdeen A. STEPHEN WILSON

Time and Longitude

I HAVE been much amused at the questions on the above (NATURE, vol. xviii. p. 40), by Mr. Latimer Clark, and the answer (p. 66) by my old friend Capt. J. P. Maclear; the numbers of NATURE for May having only just reached my "out-of-the-world" residence. I suspect Mr. L. C. has had in his mind what I have often had, and with which I have frequently puzzled some "unco guid" Sabbatarians! If it is such a deadly sin to work on Sunday, one or the other of A and B coming, one from the east, the other from the west, of 180° meridian, must, if he continues his daily avocations, be in a bad way! Some of our people in Fiji are in this unenviable position, as the line of 180° passes through Loma-Loma!

I went from Fiji to Tonga in H.M.S. *Nymph*, and arrived at our destination on *Sunday*, according to our reckoning from Fiji, but *Monday*, according to the proper computation west from Greenwich. We, however, found the natives all keeping Sunday. On my asking the missionaries about it they told me that the missions to that group and the "navigators," having all come from the *eastward*, had determined to observe their *seventh* day, as usual, so as not to subject the natives to any future puzzle, and agreed to put the dividing line further off, between them and Hawaii, somewhere in the broad ocean, where there were no metaphysical natives or "intelligent Zulus" to cross-question them! E. L. LAYARD

British Consulate, Noumea, New Caledonia

Hereditary Transmission

I HAVE perused with interest Mr. Edmund Watt's account of the six-fingered family in Dominica, as it recalls to my memory a family showing precisely the same peculiarities in Ceylon, at Point Pedro, the most northerly point of the island, where, twenty-six years ago, I was magistrate.

A family quarrel came before me, and I found, to my great astonishment, that plaintiff and defendant, and all the witnesses, had six fingers on each hand and six toes on each foot! The additional finger or toe was, in each instance, a "little finger" (or toe) inserted in the side of the hand or foot, quite loosely, adhering to the skin, and not part of the skeleton. It might easily have been excised with a pair of ordinary scissors. The parties were all closely related—brothers and sisters, uncles and aunts, nephews, nieces, and cousins—they must have had a common progenitor. It would be easy, and most interesting, to ascertain if any of the family now exist, and, if so, if the supplementary finger has been transmitted to the present generation. A note to the "Resident Magistrate," Point Pedro, would, I hope, produce a reply. If any of the family of my old clerk, Mr. Dehoedt, survive, they would recollect the fact. I think the party came from Panditerripu. E. L. LAYARD

British Consulate, Noumea

"Survival of the Fittest"

IN NATURE, vol. xix. p. 155, Mr. S. F. Clarke's observations on the cannibal habits so rapidly developed by the larvæ of the New England salamanders are cited in illustration of the survival of the fittest. The fact that similar tendencies are invariably betrayed very early in life by the young of the common Mexican Axolotl (*Siredon mexicanum*), numbers of which are annually hatched out in the Brighton Aquarium, may perhaps be of interest. Many of the smaller and weaker ones are bodily devoured by their stronger brethren of the same brood, an inclination which is so marked that systematic over-feeding is resorted to in order to arrest the diminution in the number of specimens.

Brighton, December 27, 1878 A. CRANE

Shakespeare's Colour-Names

IN the very interesting articles and correspondence which you have published on the subject of colour-blindness, it is rather surprising that no one has referred to a passage which, if taken alone, would appear to show that Shakespeare did not know the difference between green and blue. In "Romeo and Juliet" (Act iii., Scene 5), the Nurse says to Juliet, speaking of Paris:—

> "Oh, he's a lovely gentleman;
> Romeo's a dish-clout to him; an eagle, madam,
> Hath not so green, so quick, so fair an eye
> As Paris hath."

What is here called a green eye is evidently what we call a blue one. But Iago ("Othello," Act iii., Scene 3) calls jealousy a "green-eyed monster," using the expression "green-eyed" as a modern might use it, and meaning something very unlike "blue-eyed." These instances appear only to show that in the language of Shakespeare's time the names of colours were used somewhat vaguely. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, co. Antrim, December 23

DISCUSSION OF THE WORKING HYPOTHESIS THAT THE SO-CALLED ELEMENTS ARE COMPOUND BODIES[1]

I.

IT is known to many Fellows of the Society that I have for the last four years been engaged upon the preparation of a map of the solar spectrum on a large scale, the work including a comparison of the Fraunhofer lines with those visible in the spectrum of the vapour of each of the metallic elements in the electric arc.

To give an idea of the thoroughness of the work, at all events in intention, I may state that the complete spectrum of the sun, on the scale of the working map, will be half a furlong long; that to map the metallic lines and purify the spectra in the manner which has already been described to the Society, more than 100,000 observations have been made and about two thousand photographs taken.

In some of these photographs we have vapours compared with the sun; in others vapours compared with each other; and others again have been taken to show which lines are long and which are short in the spectra.

I may state in way of reminder that the process of purification consisted in this: When, for instance, an impurity of manganese was searched for in iron, if the longest line of Mn was absent, the short lines must also be absent on the hypothesis that the elements are elementary; if the longest line were present, then the impurity was traced down to the shortest line present.

The Hypothesis that the Elements are Simple Bodies does not include all the Phenomena

The final reduction of the photographs of all the metallic elements in the region 39-40—a reduction I

[1] Paper read at the Royal Society, December 12, by J. Norman Lockyer, F.R.S.

began in the early part of the present year, and which has taken six months, summarised all the observations of metallic spectra compared with the Fraunhofer lines accumulated during the whole period of observation. Now this reduction has shown me that the hypothesis that identical lines in different spectra are due to im-

TABLE I.—FINAL REDUCTION—IRON.

Intensity in Sun.	Wave-length and length of line.	Coincidences with Short Lines.
1	39 0600 / 2	U/3, Zr/5, Yt/4
3	0622 / 4	Va/4
2	0920 / 3	Va/2, Ba/3
3	1010 / 4	Va/4, Pt/3
2	1648 / 2	Co/3
2	1755 / 3	Mn/3, Ce/4
2	1835 / 4	Os/2
1	2700 / 1	Va/2
1	2950 / 1	Mo/3
3	3023 / 4	Ce/4
5	3435 / 4	U/2
3	3475 / 2	Ba/2, Rh/2
3	3628 / 3	Ta/3
2	3975 / 2	Co/3
3	4026 / 3	Va/5
3	4422 / 4	Mo/3
3	4720 / 2	Yt/5, Th/3
2	5012 / 2	Di/2
2	5160 / 2	Ce/3, Ru/3
2	5210 / 3	W/3
2	5423 / 4	U/3, Mo/3, W/4
3	6215 / 3	Yt/5, Ce/3, Di/2
2	6571 / 2	Zr/2
3	6662 / 2	Th/1
1	7555 / 3	Os/2, Ta/4, Cr/2
3	7578 / 4	Di/2
2	7685 / 2	Va/4
2	8083 / 2	Ti/1
1	8320 / 1	Cr/3
3	9520 / 3	Ru/3
2	9750 / 2	Mo/3

TABLE II.—FINAL REDUCTION—TITANIUM.

Intensity in Sun.	Wave-length and length of line.		Coincidences with Short Lines.
	39		
1	0·00	3	Zr 4
4	0048	3	Th 4
5	1040	5	Mn 4, Ce 5, Di 3
2	1360	3	Va 4
5	1915	8	Ce 4
4	2050	3	U 3, La 3
3	2368	2	Va 3
3	3718	5	Th 4, Ce 4
2	4775	1	Fe 2
2	5722	1	Zr 1, Rh 3
4	6175	2	U 3
3	6335	2	Di 3, Ta 5
2	8083	1	Fe 2
3	8152	2	Mo 3
1	8922	1	Mn 4, Cr 4
2	9798	longest	Va longest

purities is not sufficient. I shall show in detail in a subsequent paper the hopeless confusion in which I have been landed. I limit myself on the present occasion to giving tables showing how the hypothesis deals with the spectra of iron and titanium.

We find short line coincidences between many metals the impurities of which have been eliminated or in which the freedom from mutual impurity has been demonstrated by the absence of the longest lines.

Evidences of Celestial Dissociation

It is five years since I first pointed out that there are many facts and many trains of thought suggested by solar and stellar physics which point to another hypothesis—namely, *that the elements themselves, or at all events some of them, are compound bodies.*

In a letter written to M. Dumas, December 3, 1873, and printed in the *Comptes Rendus*, I thus summarised a memoir which has since appeared in the *Philosophical Transactions*.

"Il semble que plus une étoile est chaude plus son spectre est simple, et que les éléments métalliques se font voir dans l'ordre de leurs poids atomiques.[1]

"Ainsi nous avons :

"1. Des étoiles très-brillantes où nous ne voyons que l'hydrogène, *en quantité énorme*, et le magnésium ;

"2. Des étoiles plus froides, comme notre Soleil, où nous trouvons :

$$\mathrm{H} + \mathrm{Mg} + \mathrm{Na}$$
$$\mathrm{H} + \mathrm{Mg} + \mathrm{Na} + \mathrm{Ca}, \mathrm{Fe}, \ldots ;$$

dans ces étoiles, pas de métalloïdes ;

"3. Des étoiles plus froides encore, dans lesquelles *tous les éléments métalliques sont* ASSOCIÉS, où leurs lignes ne sont plus visibles, et où nous n'avons que les spectres des métalloïdes et des composés.

"4. Plus une étoile est âgée, *plus l'hydrogène libre disparaît;* sur la terre, nous ne trouvons plus d'hydrogène en liberté.

"Il me semble que ces faits sont les preuves de plusieurs idées émises par vous. J'ai pensé que nous pouvions imaginer une '*dissociation céleste*,' qui continue le travail de nos fourneaux, et que les métalloïdes sont des composés qui sont dissociés par la température solaire, pendant que les éléments métalliques monatomiques, dont les poids atomiques sont les moindres, son précisément ceux qui résistent, même à la température des étoiles les plus chaudes."

Before I proceed further, I should state that while observations of the sun have since shown that calcium should be introduced between hydrogen and magnesium for that luminary, Dr. Huggins' photographs have demonstrated the same fact for the stars, so that in the present state of our knowledge, independent of all hypotheses, the facts may be represented as follows, the symbol indicating the spectrum in which the lines are visible.

Hottest Stars	Lines of	H + Ca + Mg
Sun ...	Lines of	H + Ca + Mg + Na + Fe
Cooler Stars	Lines of	— — Mg + Na + Fe + Bi + Hg
Coolest ...	Fluted bands of	— — — — — — Metalloids

Following out these views, I some time since communi-

[1] This referred to the old numbers in which Mg = 12, Na = 23.

cated a paper to the Society on the spectrum of calcium, to which I shall refer more expressly in the sequel.

Differentiation of the Phenomena to be observed on the Two Hypotheses

When the reductions of the observations made on metallic spectra, on the hypothesis that the elements were really elementary, had landed me in the state of utter confusion to which I have already referred, I at once made up my mind to try the other hypothesis, and therefore at once sought for a critical differentiation of the phenomena on the two hypotheses.

Obviously the first thing to be done was to inquire whether one hypothesis would explain these short line coincidences which remained after the reduction of all the observations on the other. Calling for simplicity' sake the short lines common to many spectra *basic lines*, the new hypothesis, to be of any value, should present us with a state of things in which basic molecules representing bases of the so-called elements should give us their lines, varying in intensity from one condition to another, the *conditions* representing various compoundings.

Suppose A to contain B as an impurity and as an element, what will be the difference in the spectroscopic result?

A in both cases will have a spectrum of its own;

B as an impurity will add its lines according to the amount of impurity, as I have shown in previous papers.

B as an element will add its lines according to the amount of dissociation, as I have also shown.

The difference in the phenomena, therefore, will be that, with gradually increasing temperature, the spectrum of A *will fade*, if it be a compound body, as it will be increasingly dissociated, and it *will not* fade if it be a simple one.

Again, on the hypothesis that A is a compound body, that is, one compounded of at least two similar or dissimilar molecular groupings, then the longest lines at one temperature will not be the longest at another, the whole fabric of "impurity elimination," based upon the assumed single molecular grouping, falls to pieces, and the origin of the basic lines is at once evident.

This may be rendered clearer by some general considerations of another order.

General Considerations

Let us assume a series of furnaces A . . . D, of which A is the hottest.

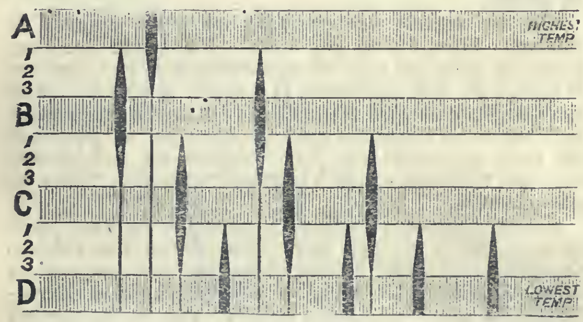


Fig. 1.[1]

Let us further assume that in A there exists a substance α by itself competent to form a compound body β by union with itself or with something else when the temperature is lowered.

Then we may imagine a furnace B in which this compound body exists alone. The spectrum of the compound β would be the only one visible in B, as the spectrum of the assumed elementary body α would be the only one visible in A.

A lower temperature furnace C will provide us with a more compound substance γ, and the same considerations will hold good.

Now if into the furnace A we throw some of this doubly compounded body γ we shall get at first an integration of the three spectra to which I have drawn attention; the lines of γ will first be thickest, then those of β, and finally α would exist alone, and the spectrum would be reduced to one of the utmost simplicity.

This is not the only conclusion to be drawn from these considerations. Although we have by hypothesis β, γ, and δ all higher, that is, more compound forms of α, and although the strong lines in the diagram may represent the true spectra of these substances in the furnaces B, C, and D, respectively, yet, in consequence of incomplete dissociation, the strong lines of β will be seen in furnace C, and the strong lines of γ will be seen in furnace D, *all as thin lines*. Thus, although in C we have no line which is not represented in D, the intensities of the lines in C and D are entirely changed.

In short, the line of α strong in A is *basic* in B, C, and D, the lines of β strong in B are *basic* in C and D, and so on.

I have prepared another diagram which represents the facts on the supposition that the furnace A, instead of having a temperature sufficient to dissociate β, γ, and δ into α is far below that stage, although higher than B.

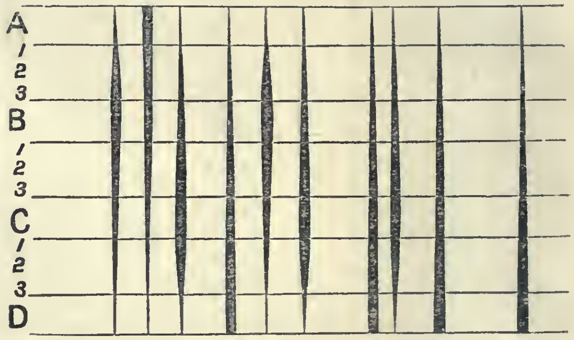


Fig. 2.

It will be seen from this diagram that then the only difference in the spectra of the bodies existing in the four furnaces would consist merely in the relative thicknesses of the lines. The spectrum of the substances as they exist in A would contain as many lines as would the spectrum of the substances as they exist in D; each line would in turn be *basic in the whole series of furnaces* instead of in one or two only.

Application of these General Considerations to Impurity Elimination

Now let us suppose that in the last diagram (Fig. 2) the four furnaces represent the spectra of say, iron, broken up into different finenesses by successive stages of heat. It is first of all abundantly clear that the relative thicknesses of the iron lines observed will vary according as the temperature resembles that of A, B, C, or D. The positions in the spectra will be the same, but the intensities will vary; this is the point. *The longest lines, represented in the diagram by the thickest ones, will vary as we pass from one temperature to another.* It is on this ground that I have before stated that the whole fabric of impurity elimination must fall to pieces on such an hypothesis. Let us suppose, for instance, that manganese is a compound of the form of iron represented in furnace B, with something else; and suppose again that the photograph of iron which I compare with manganese represents the spectrum of the vapour at the temperature of the furnace D. To eliminate the impurity of iron in manganese, as I have eliminated it, we begin the search by looking for the longest and strongest lines shown in the photograph of iron, in the photograph of manganese taken under the same conditions. I do not find these lines. I

[1] The figures between the hypothetical spectra point to the gradual change as the spectrum is observed near the temperature of each of the furnaces.

say, therefore, that there is no impurity of iron in manganese, but although the longest iron lines are not there, some of the fainter basic ones are. This I hold to be the explanation of the apparent confusion in which we are landed on the supposition that the elements are elementary.

Application of these Considerations to Known Compounds

Now to apply this reasoning to the dissociation of a known compound body into its elements—

A compound body, such as a salt of calcium, has as definite a spectrum as a simple one; but while the spectrum of the metal itself consists of lines, the number and thickness of some of which increase with increased quantity, the spectrum of the compound consists in the main of channelled spaces and bands, which increase in like manner.

In short, the molecules of a simple body and a compound one are affected in the same manner by quantity in so far as their spectra are concerned; *in other words, both spectra have their long and short lines*, the lines in the spectrum of the element being represented by bands or fluted lines in the spectrum of the compound; and in each case the greatest simplicity of the spectrum depends upon the smallest quantity, and the greatest complexity (a continuous spectrum) upon the greatest.

The heat required to act upon such a compound as a salt of calcium so as to render its spectrum visible, dissociates the compound according to its volatility; the number of true metallic lines which thus appear is a measure of the quantity of the metal resulting from the dissociation, and as the metal lines increase in number, the compound bands thin out.

I have shown in previous papers how we have been led to the conclusion that binary compounds have spectra of their own, and how this idea has been established by considerations having for a basis the observations of the long and short lines.

It is absolutely similar observations and similar reasoning which I have to bring forward in discussing the compound nature of the chemical elements themselves.

In a paper communicated to the Royal Society in 1874, referring, among other matters, to the reversal of some lines in the solar spectrum, I remarked[1]:—

"It is obvious that greater attention will have to be given to the precise *character* as well as to the position of each of the Fraunhofer lines, in the thickness of which I have already observed several anomalies. I may refer more particularly at present to the two H lines 3933 and 3968 belonging to calcium, which are much thicker in all photographs of the solar spectrum [I might have added that they were by far the thickest lines in the solar spectrum] than the largest calcium line of this region (4226·3), this latter being invariably thicker than the H lines in all photographs of the calcium spectrum, and remaining, moreover, visible in the spectrum of substances containing calcium in such small quantities as not to show any traces of the H lines.

"How far this and similar variations between photographic records and the solar spectrum are due to causes incident to the photographic record itself, or to variations in the intensities of the various molecular vibrations under solar and terrestrial conditions, are questions which up to the present time I have been unable to discuss."

An Objection Discussed

I was careful at the very commencement of this paper to point out that the conclusions I have advanced are based upon the analogies furnished by those bodies which, by common consent and beyond cavil and discussion are compound bodies. Indeed, had I not been careful to urge this point the remark might have been made that the various changes in the spectra to which I shall draw attention are not the results of successive dissociations, but are effects due to putting the same mass into different kinds of vibration or of producing the vibration in different ways. Thus the many high notes, both true and false, which can be produced out of a bell with or without its fundamental one, might have been put forward as analogous with those spectral lines which are produced at different degrees of temperature with or without the line, due to each substance when vibrating visibly with the lowest temperature. To this argument, however, if it were brought forward, the reply would be that it proves too much. If it demonstrates that the *h* hydrogen line in the sun is produced by the same molecular grouping of hydrogen as that which gives us two green lines only when the weakest possible spark is taken in hydrogen inclosed in a large glass globe, it also proves that calcium is identical with its salts. For we can get the spectrum of any of the salts alone without its common base, calcium, as we can get the green lines of hydrogen without the red one.

I submit, therefore, that the argument founded on the overnotes of a sounding body, such as a bell, cannot be urged by any one who believes in the existence of any compound bodies at all, because there is no spectroscopic break between acknowledged compounds and the supposed elementary bodies. The spectroscopic differences between calcium itself at different temperatures is, as I shall show, as great as when we pass from known compounds of calcium to calcium itself. There is a perfect continuity of phenomena from one end of the scale of temperature to the other.

Inquiry into the Probable Arrangement of the Basic Molecules

As the results obtained from the above considerations seemed to be so far satisfactory, inasmuch as they at once furnished an explanation of the *basic lines* actually observed, the inquiry seemed worthy of being carried to a further stage.

The next point I considered was to obtain a clear mental view of the manner in which, on the principle of evolution, various bases might now be formed, and then become basic themselves.

It did not seem unnatural that the bases should increase their complexity by a process of continual multiplication, the factor being 1, 2, or even 3, if conditions were available under which the temperature of their environment should decrease, as we imagined it to do from the furnace A down to furnace D. This would bring about a condition of molecular complexity in which the proportion of the molecular weight of a substance so produced in a combination with another substance would go on continually increasing.

Another method of increasing molecular complexity would be represented by the addition of molecules of different origins. Representing the first method by $A + A$, we could represent the second by $A + B$. A variation of the last process would consist in a still further complexity being brought about by the addition of another molecule of B, so that instead of $(A + B)_2$ merely, we should have $A + B_2$.

Of these three processes the first one seemed that which it was possible to attack under the best conditions, because the consideration of impurities was eliminated; the prior work has left no doubt upon the mind about such and such lines being due to calcium, others to iron, and so forth. That is to say, they are visible in the spectra of these substances as a rule. The inquiry took this form: Granting that these lines are special to such and such a substance, does each become basic in turn as the temperature is changed?

I therefore began the search by reviewing the evidence concerning calcium and seeing if hydrogen, iron, and lithium behaved in the same way.

(*To be continued.*)

[1] *Phil. Trans.*, vol. clxiv., part 2, p. 807.

ZÖPPRITZ ON OCEAN CURRENTS

I SEND you a translation by a friend of an important contribution to the theory of ocean currents by Prof. Zöppritz, of Giessen, which has recently appeared in the *Annalen der Hydrographie und Maritimen Meteorologie.* The mathematical part of the subject has been published in the *Annalen der Physik* for April last, a translation of which will be found in the *Philosophical Magazine* for September.

One of the main objections urged against the theory that ocean currents are due to the impulse of the winds is that the winds can, it is alleged, produce only a *surface drift*, whereas many of the currents extend to great depths. I have always maintained that this objection is totally erroneous; that if the surface of the ocean be impelled forward with a constant velocity by the wind or by any other cause whatever, the layer immediately below will be dragged along with a constant velocity somewhat less. The layer underneath this second layer will in turn be also dragged along with a velocity less than the one above it. The same will take place in regard to each successive layer, the velocity of each being somewhat less than the one immediately above it, and greater than the one below it. In this manner the surface velocity may be transmitted downwards to any depth. This conclusion has now been demonstrated by Prof. Zöppritz, in the following paper, to be perfectly correct. JAMES CROLL

Though for a long time the majority of seamen and geographers have firmly held the opinion that the great equatorial ocean currents derived their origin from the trade winds, yet, so far as I know, no attempt has yet been made to treat the physical problem of the propagation of surface-velocities downwards through a very thick stratum of water, with the means presented by the theory of the friction of fluids, as elaborated within the last thirty years. Such an attempt is all the more demanded as many authors have lately denied that surface-forces could set the sea in motion to any considerable depth. At the same time the most groundless assumptions have been set forth as to the depth of such drift-currents.

The essential principle of the theory of the internal friction of fluids is that when a plane stratum of water is moved forward, by any cause, in its own plane with a given velocity, the adjoining stratum cannot remain at rest, but, in consequence of its molecular cohesion experiences an impulse to move in the same direction. And if the velocity of the former stratum be continuous the latter assumes a velocity which tends to approximate constantly to the given velocity. This second stratum now exerts the same influence on a third adjoining stratum that it had to suffer from the first, and sets it in motion in the same direction. The third stratum draws with it in a similar manner a fourth, a fourth a fifth, and so on. The propagation of the velocity is only bounded by the limits of the fluid itself. If these limits consist of a solid plane parallel to the strata, then the propagation of the velocity will cease only at this point, *i.e.*, between the last liquid stratum and the first solid stratum.

The law according to which two neighbouring strata of velocities mutually influence one another has already been demonstrated by Newton, and the accelerating force exerted by the friction has been assumed as independent of the pressure and proportional to the difference of velocity. The later theory of the friction of fluids carries out this fundamental hypothesis as to the propagation of velocity between strata of the same medium which lie at an indefinitely small distance ξ from one another, and have accordingly only an indefinitely small difference of velocity Δ, inasmuch as it makes the acceleration produced by the friction, at the plane in which the strata meet, proportional to the quotient $\Delta : \xi$. The factor k, by which this quotient must be multiplied in order to give the acceleration, is called the *Coefficient of Internal Friction.*

The Newtonian hypothesis can be applied likewise to those parts of the bounding-surfaces of the fluid (where it is in contact with other bodies) which may possess independent motion. Here the acceleration produced by the limiting medium (which may be solid, fluid, or gaseous) is proportional to the difference of velocity which may in this case be finite. The factor of the proportion is called the *Coefficient of External Friction.* If the bounding body is a solid or even a fluid, then the fluid may wet it, that is, the stratum of fluid touching the limiting body may cling so fast to that body as to assume the same velocity. The coefficient of external friction is in this case infinitely great. This is the case between wood and water, glass and water; and, on the other hand, not so between glass and quicksilver.

The theory founded on this simple hypothesis has been subjected to the most varied experimental tests, and has, on the whole, been found to agree with the facts, so that the hypothesis may be regarded as proved.

In order to apply this theory to ocean currents, the simplifying presupposition has been made that the ocean is a mass of fluid contained between two horizontal planes at the distance h from one another, but in other respects unbounded. On the surface of this mass of fluid a wind of uniform strength and direction is acting at all times, while the under-surface wets a solid plane, the sea-bottom, and is therefore always at rest.

We must not, however, look on the action of the moving air on the surface stratum of the water as proceeding according to the Newtonian hypothesis; it will act in this way only so long as the surface remains level.

But the wind produces waves and acts on them according to quite different laws. One fact of experience is available here, viz., that the surface-stratum of the ocean under the influence of a uniform wind, moves in the direction of the wind with a constant velocity dependent on the strength of the wind. If, therefore, we place on the velocity of the water at the surface the condition that it has a value w_0 at all times given, everywhere uniform, and of uniform direction, then the problem of the determination of the internal velocity becomes soluble.

But the simplifying presuppositions here assumed are almost realised in the central equatorial regions of the great ocean; the solution of the problem becomes, therefore, of deep interest.

The following are the chief results of the solution:—

If for an infinitely long time the surface-stratum has been kept at an unchanging velocity, then the whole mass of water is in a steady state of motion, *i.e.*, a state which no longer varies according to the time. The velocity w is then dependent only on the depth x beneath the surface, and diminishes in proportion as the depth increases, till at the bottom it reaches *zero.* This relation is expressed by the formula

$$w = w_0 \frac{h - x}{h}.$$

Naturally it is presupposed that no other causes, *e.g.*, displacing currents, affect the motion of the deeper strata. If these deeper strata are kept by any foreign cause whatsoever in steady motion in a direction exactly opposite to the assumed motion, then at some point between the highest and the deepest strata there lies a plane where the velocity = 0. If this plane lies at the depth h_1, then in the mass which lies above it the velocity follows the formula

$$w = w_0 \frac{h_1 - x}{h_1},$$

and is therefore in the same condition as if the strata that lie beneath were a solid mass.

It is specially noteworthy that the velocity is independent of the coefficient of friction, *i.e.*, that in the

state of motion that prevails after an infinitely long time the distribution of the velocity is the same in a thin fluid like water and in a thick fluid like syrup. In the fixed state of motion the influence of friction is shown by the participation of all the strata in the motion which is imparted from without to the surface alone. Dependence on the coefficient of friction takes place only on the consideration of motions that vary with the time, and affords a measure for the depth of penetration of a surface-impulse within a given time.

The formula which gives the velocity at the depth x of a mass of water originally at rest when for the time t the surface has been kept at a constant velocity w_0, has naturally a less simple form than the formula which was found for steady motions. (The formula is the same as that which determines the propagation of heat in a solid wall whose one side is kept at a temperature $w_0°$, whilst the other remains at 0°.) From this formula results the simple law that any velocity whatever between o and w_0 prevails at different times at depths which are related to one another as the square roots of the times. I have used the formula to compute the time that a point at the depth of 100 metres requires to attain half the surface velocity, *i.e.*, $\frac{1}{2}w_0$. The coefficient of friction of the water was assumed according to O. E. Meyer's determination, at 0·0144, in which centimetres and seconds are the units of calculation. The result was that 239 years are required for the layer of water 100 metres deep to assume the half of the surface velocity. If it be asked what length of time is required for one-tenth of the surface velocity to penetrate to that depth, the answer is 41 years. Accordingly, the same velocities will be attained at a depth of 10 metres after 2·39 and 0·41 years respectively. In a more viscous fluid the resulting numbers would be smaller.

These numbers are well calculated to give an idea of the slow rate at which changes of motion are propagated downwards. For the numbers computed for the propagation of a given surface motion, hold likewise for the penetration of a change of the motion from the surface downwards, whose influence is simply added to the already existing motion. A steady current, therefore, whose velocity diminishes linearly according to the depth, will sustain only an extremely slight alteration (except in the strata nearest the surface) from passing changes of motion that affect the surface, *e.g.* from contrary winds or storms. There will prevail, rather, at every deep-lying point of this current, a mean velocity that changes only very slightly according to the time, and which is determined by the mean velocity at the surface. This latter velocity has the direction of the prevailing wind, according to whose strength it varies by a law that cannot be more accurately settled.

If the surface velocity varies periodically according to the time, as is the case with all winds that depend on seasons and the hours of the day, then, after this periodic state has lasted an infinitely long time, the velocity at all depths is a periodic function of the time of similar period, but such that the amount of variation decreases rapidly according to the depth and that the occurrence of the maxima and minima is delayed proportionally to the depth. At a depth of 10 metres the amount of the yearly oscillation is already diminished to less than $\frac{1}{13}$th; at a depth of 100 metres it is beyond observation; at this depth the velocity is that corresponding to the steady state when the mean annual velocity is given to the surface. When the depths decrease in arithmetical proportion, the amounts of the oscillation decrease in geometrical proportion such that at four depths x_1, x_2, x_3, x_4, which stand in the relation

$$x_4 - x_3 = x_2 - x_1;$$

the amounts D_1, D_2, D_3, D_4 stand in relation

$$D_4 : D_3 = D_2 : D_1.$$

A maximum and the following minimum of the annual oscillation always exist at the same time at a vertical distance of 11·9 metres.

To give a conception of the time that a constant surface-velocity which begins at the time $t = o$ requires, in order to bring the interior of an ocean 4,000 metres deep, which was previously at rest, to the state of steady motion, the following numbers will serve:—After 10,000 years there prevails at the half-depth, *i.e.*, at $x = 2,000$ metres, just the velocity $0·037w_0$. Since, according to the already-stated formula, in the steady state the velocity $0·5w_0$ must prevail at this point, it is easily seen how far the ocean is still removed after 10,000 years from the steady state. After 100,000 years the velocity at the depth stated is already $0·461w_0$, therefore very near the definitive value. After 200,000 years it differs only by two units in the third decimal place.

Among the results we have found, particular emphasis is to be laid on two, which seem more or less to contradict the views which have prevailed up to this time. In the first place, the steady motion arising in the interior of an unlimited stratum of water from an unvarying surface velocity makes itself felt with linearly decreasing velocity down to the bottom. Hitherto the view frequently expressed was, that the influence of surface currents, *e.g.*, the drift caused in the tropical ocean by the trade winds, reached only to very moderate depths. Secondly, it was found that all variations according to time, whether periodic or aperiodic, of the forces acting on the surface, propagate themselves downwards with extraordinary slowness, the periodic in very quickly decreasing amount. Taking both statements together, it follows that the movement of the chief part of a stratum of water exposed to periodically varying surface forces is determined by the mean velocity of the surface, and that the periodic variations are observable only in a comparatively thin surface stratum. From this it is obvious that hitherto the influence of the friction was undervalued in one direction, in so far, namely, as it was believed that its influence need not be considered as penetrating so deep, but in another direction it was overvalued, as too great an influence was wont to be ascribed to friction in respect of the propagation of varying current motions Its effect was also very much overvalued in another point, viz., in respect of the action of a bank on a stream flowing along it. If, I repeat, the whole surface is kept at a constant velocity, then also in the current bounded at the side the distribution of velocity in the steady state is independent of the co-efficient of friction. Beyond that, the influence of the banks on the distribution of velocity is exceedingly slight.

A further result is that two steady currents flowing parallel to one another, but in opposite directions, in a fluid-stratum of constant depth, may very well graze one another without mutual disturbance. Their surface of division is then a vertical plane parallel to their direction in which the velocity 0 prevails, and which, therefore, stands to each current in the relation of a solid bank.

We have already shown numerically how extraordinarily slow the velocity existing at the surface is propagated downwards when the interior was previously at rest. Hence it may be concluded, *vice versâ*, that when every point of the whole mass of fluid has at a given moment a given velocity varying according to the depth, and when from the same moment onwards the surface remains at rest, the effect of this initial state vanishes equally slowly, *i.e.*, the ocean passes into the state of rest with the same slowness with which in the first case the surface-velocity was propagated into the interior. In fact the formulæ show that the times for the increase and decrease of the same fraction of the given velocity are expressed by the same number.

If from some cause or other strong currents had been generated in the ocean, say 10,000 years ago, these

currents would certainly not have as yet disappeared, but would still be the chief agents in determining the movement of the ocean at great depths, supposing that the earth were completely covered by an ocean of the uniform depth of 4,000 metres.

The interruption by continents and islands of irregular form will contribute to weaken the effect of these former states of motion, not so much through the increased friction on the ocean-bed as through the reflex currents, which arise everywhere, crossing and impeding one another. But it must be observed after the above numerical proof of the extremely slow spread of local alterations of motion over the interior mass, that the difficulties of an exact computation must not be shirked, on account of the traditional expression: "Friction quickly uses up all these velocities."

It would be possible to determine by observations whether effects of former movements are still present in the ocean. There would be required for this purpose only comparative current-observations at the most varied depths, to be applied in the central parts of the great equatorial currents and of the region of calms. Yet, however, we dare not hope to be able to detect small remnants of interior motion with the same certainty with which the effect of the former high temperature of the earth, which disappears according to the same law, could be detected by subterranean observation of temperature, were one able to penetrate deep enough with the thermometer into the earth's crust.

The above computations give us also an idea how distant must be the time of the initial state. What a long time, for example, must we imagine the trade winds to have been blowing with their present extent and strength in order to be justified in assuming that the present state of motion of the equatorial currents is steady. For that about 100,000 years are needed, supposing we postulate a mean depth of 4,000 metres and do not take into account the deadening influence of continents and islands which must somewhat diminish that number. Every initial state, whatever it may have been, vanishes finally, and gives way to a steady state, only the time varies which is required to diminish the originally arising velocity to any required degree of smallness.

OUR ASTRONOMICAL COLUMN

The Melbourne Observatory.—The thirteenth official Report of the Board of Visitors of the Melbourne Observatory, with the annual statement of the Government Astronomer, is before us. Mr. Ellery reports that the new building to contain the magnetical and meteorological instruments registering continuously by photography is completed. The staff of the Observatory now consists of the director, with a chief assistant (Mr. White) and three junior assistants. The transit-circle is found to be inadequate for modern requirements, and the Board of Visitors lay stress upon the necessity of providing an instrument of greater pretensions, to enable Melbourne to co-operate effectively with European and American observers; the Sydney Observatory being already in possession of a very superior meridian-instrument, and one having been ordered, it is understood, for the observatory under the direction of Mr. Todd at Adelaide, it is hoped that a new transit-circle may soon be provided for Melbourne, and it is suggested that the necessary appropriation, about 1,200*l.*, might be made in two annual votes, as two years will be required for the completion of the instrument.

The great reflector, though reported to be working satisfactorily, the mirrors retaining an excellent polish, and no marked signs of deterioration being visible, is occasionally subject to trifling derangements of its mechanism. Unfortunately the publication of the work with this instrument, the drawings of nebulæ, has been delayed by the loss of the gentleman who copied the drawings on stone. The drawings, however, now only require printing, and their publication is not likely to be long retarded. Mr. Ellery refers to the miscellaneous observations made during the year to which his report relates (to June 30, 1877), including observations of D'Arrest's comet of short period, determination of positions of stars used by Mr. Gill during his expedition to Ascension, measures of southern double stars and of the polar and equatorial diameters of Mars, and of Saturn's ring. With regard to the use of the great reflector it is mentioned, "Out of 326 available nights 150 were unfitted for observation from unfavourable weather, bright moonlight interfered on 32, while 49 were occupied with visitors, which, together with about 20 nights during which the telescope was under repair, or which were unavailable from other causes, left only 75 nights upon which observations could be made." From the observations made during the year upon 77 of the smaller nebulæ in Sir John Herschel's "General Catalogue," it is gathered that while the actual aspect of many conforms precisely with Herschel's description, others are so considerably changed as to be only recognisable by their position. The only change detected in the great nebula about η Argûs, since the drawing in March, 1875, has been "a break or separation in one of the branches on the preceding side."

Observations of the satellites of Uranus were made on sixteen nights, and on the same number of nights the satellites of Mars (the announcement of the discovery of which had been telegraphed to Mr. Ellery by Sir George Airy) were unsuccessfully sought for; the failure to find these objects with certainty and ease Mr. Ellery considers "somewhat unaccountable," but the reader will hardly need to be reminded that there are other cases where the large reflectors have not proved so adequate for work as the large refractors: sooner or later, at Melbourne or elsewhere, we hope to see a large instrument of the latter class applied to the survey of the southern heavens: the real astronomical work in the northern hemisphere, the more precise micrometrical measures and more delicate observations falling to the task of the practical astronomer, have been, as yet, pre-eminently due to the use of the refractor.

Biela's and Halley's Comets.—There are near approximations between the orbits of these bodies not far from points which were first roughly indicated by Littröw, in a communication to the Vienna Academy in 1854, entitled "Bahnnähen zwischen den periodischen Gestirnen des Sonnensystemes." In heliocentric longitude 39° 25′ (equinox of 1836) the distance between the two orbits is 0·032 (the earth's mean distance from the sun = 1), and in 200° 51′, the distance is as small as 0·011. At the former point the true anomaly of Halley's comet is − 94° 9′, with the elements of 1836, and that of Biela's − 71° 17′; at 200° 51′ the true anomaly of Halley's is + 104° 59′, and of Biela's + 90° 2′; we see then that on the last return of Halley's comet to these parts of space, though its orbit approached so near to that of Biela's, there was no near approximation of the two bodies. It will be remembered that Biela's comet also passes very near to the orbit of Tempel's comet 1866 I., and consequently to the track of the November meteor-stream.

GEOGRAPHICAL NOTES

Among the geographical notes in the January number of the new periodical issued by the Royal Geographical Society we find some interesting information regarding the work to be done by Mr. Keith Johnston's East African Expedition. He is instructed to gather data for constructing as complete a map as possible of the route, and to make all practicable observations in meteorology,

geology, natural history, and ethnology, with the view of rendering as exact as possible the information obtained regarding the region, its inhabitants, and products. As special subjects of investigation he is to observe and note the routes best adapted for future more extensive communication, and to spare no efforts in examining the range of mountains seen by Mr. E. D. Young and by Capt. Elton and his party, at the north-east end of Lake Nyassa, ascertaining their extent and elevation, and the condition of the routes or passes over them. The practicability of constructing a line of telegraph from north to south through the region is also to be inquired into. If Mr. Johnston should succeed in reaching Lake Tanganyika he is directed to pay special attention to facts bearing upon the extraordinary rise in its level in very recent times, as stated by Mr. Stanley. Besides making accurate measurements, Mr. Johnston is recommended to institute inquiries as to whether the rise may not be periodical, or the result of a succession of years of excessive rainfall; but in the event of its proving continuous he is to investigate with care the causes and results of so remarkable a phenomenon. This note is followed by a summary of the survey arrangements of the Afghanistan Expedition, which promise to add much to our knowledge of the unknown tracts of country on our north-west frontier. The information contained in the remaining notes has already been placed before our readers in our own columns. The maps in the present number are those of the Fly River, New Guinea, from Signor D'Albertis' survey, of the Sulimani Mountains, on our Afghan frontier, illustrating an article by Mr. C. R. Markham, and of the routes of the Swedish and Dutch Arctic Expeditions.

THE International African Association at Brussels have recently received intelligence that MM. Wautier and Dutrieux, with 360 porters, had left Mpwapwa on October 15 to rejoin M. Cambier. On October 27 they were at Mvumi, in Ugogo, where a letter from M. Cambier reached them, announcing his arrival at Kasisi, which is two days' march from Urambo, the capital of King Mirambo, of Unyamwesi. They are now travelling in company with M. Broyon, Mirambo's son-in-law, who is said to be taking up a large convoy to Ujiji for the English missionaries, and under his able guidance and advice it may be hoped that they will escape similar misfortunes to those which they have experienced in the past.

INTELLIGENCE has been received at St. Petersburg that Prof. Nordenskjöld's steamer *Vega* is ice-bound on the Siberian coast.

IN the last number of the *Tour du Monde*, M. Alfred Marche, the former companion of M. Savorgnan de Brazza, in his explorations of Western Africa, concludes his admirably illustrated chapters, entitled "Voyage au Gabon et sur le Fleuve Ogooué."

PROF. KIEPERT, the eminent geographer, has recently expressed his opinion regarding the alleged return of the Amu Darya (or Oxus) into its ancient bed, and consequently becoming a tributary to the Caspian instead of the Aral Sea. The Professor remarks that all statements made hitherto, even as far back as those of the old Roman writers, are simple speculations, proving nothing else but merely the existence of a dry river-bed in the direction indicated. He thinks it a matter of course that, in the event of unusual accumulation of water in the Oxus, this bed may be filled with water for many miles' distance, and, during the few centuries for which we possess reliable data this event has happened so often, that the present recurrence need not in any way have given rise to so much talk and discussion.

MR. E. F. IM THURN, of the British Guiana Museum, paid a visit, in October and November last, to the Kaieteur Fall of the Potaro River, for the purpose of testing whether it was rightly described by its discoverer, Mr. Barrington Brown, as "one of the grandest falls in the world," as well as to prove the truth of his (Mr. im Thurn's) idea that such a place ought to be a rich treasure-ground for a collecting naturalist. He professes to be disappointed with the fall because it is neither so high as the Yosemité Fall nor so broad as Niagara. But he visited it when the water was at its lowest volume, and yet, when looking at it from above, he confesses that the fall is one of "splendid and awful beauty." Altogether we infer that Mr. Brown's description is essentially correct, especially when the river is at its fullest. The country on the road to and about the fall is described as of matchless beauty, and evidently it is a splendid field for a naturalist. The fall can be reached with comparative ease in a few days from Bartica Grove.

AT the November meeting of the Russian Geographical Society, Admiral Krusenstern described the results of his journey to Siberia in 1876 to investigate the possibility of connecting the basin of the Petchora with that of the Ob, and thus open a continuous water-way from Europe to Siberia. He reports favourably on the practicability of the scheme. The scientific results of the journey are topographical surveys, levellings of the principal parts of the route, a whole series of astronomical determinations, and a large addition to our knowledge of a region still little known.

THE last number of the *Zeitschrift* of the Berlin Geographical Society contains an elaborate paper by Herr G. Hartung on the formation of valleys and lakes. There is also a valuable paper by the late Saharan explorer, Erwin von Bary, on the character of the vegetation of Aïr; besides a large map of the African river Quanga, the result of the exploration of Herr Otto Schütt. The last two numbers of the *Verhandlungen* of the same Society contain some important papers. Prof. Karsten gives some data on the problem of ocean currents, and Dr. Tietze describes the results of his exploration of the volcanic Mount Demavend, to which we referred in a previous number. Dr. Hartmann has some interesting observations on the distribution of deep-sea animals. It will thus be seen that this Society regards geography as embracing a very wide field of research, and in this respect is a model that might with advantage be followed by other geographical societies.

A COMMITTEE has been formed at Berlin with the object of founding a "Central Union for Commercial Geography and the Furtherance of German Interests in Foreign Countries." The Society hopes to enter into friendly relations with all German and foreign geographical societies.

"BOSNIEN in Bild und Wort," is the title of an interesting work by Amand von Schweiger Lerchenfeld, just published by Hartleben, of Vienna. The geographical publications of this firm are of particular excellence, and the present work is a fair case in point. It contains some twenty charming drawings from the artistic pen of J. J. Kirchner, illustrating the most interesting parts of the province which has played so prominent a part in the past year's history. The text is carefully written, clear, and to the point. Altogether the work is an acceptable addition to geographical literature.

THE MARQUESS OF TWEEDDALE, P.Z.S.

IT is with extreme regret that we have to chronicle the death, after a three days' attack of bronchitis, on the morning of December 29, of Arthur Hay, ninth Marquess of Tweeddale, F.R.S., and President of the Zoological Society of London. Born in 1824, the second son of Field-Marshal the late Lord Tweeddale, K.T., a veteran of the Peninsula and other campaigns of "the Great War," Lord Arthur Hay at an early age entered the army, as befitted the godchild of the grand English

captain, and obtained a commission in the Grenadier Guards. But the ordinary guardsman's life in times of peace was inadequate to his aspirations, and reaching the rank of captain, he was soon after appointed aide-de-camp to Sir Henry (afterwards Lord) Hardinge, then Governor-General of India, and in that capacity accompanied his chief through the ever-memorable campaign of the Sutlej. After the English arms had triumphed in the conquest of the Punjab, Lord Arthur was attached to a mission, the details of which, we believe, have never been made public, to some of the tribes bordering upon our northern frontier, and in discharge of that duty reached places unvisited by any European traveller since the days of Moorcroft. Lord Arthur's services in India and the adjacent countries lasted over several years, in the course of which time his attention was attracted by their rich and little-known fauna, and he not only formed the acquaintance, but assiduously cultivated the friendship of two of the greatest Indian zoologists of the time—Jerdon and Blyth—of whom he became an apt pupil, fishes and birds being particularly the objects of his pursuit. Returning home at length he resumed his regimental duties, and on the outbreak of the Russian war, in 1854, he accompanied the expeditionary force first to Turkey and thence to the Crimea, taking part in the operations which ended in the fall of Sebastopol. Soon after the conclusion of peace he left the army, and his old zoological tastes, which had been growing slack, returned to him more strongly than ever. On the death of his eldest brother, Lord Gifford, he became heir to his father's honours and estates, and assumed the courtesy title of Lord Walden, by which, perhaps, he will be most generally recognised, for under that designation he published the greater part of his contributions to zoology, and under it he succeeded the late Sir George Clerk as President of the Zoological Society, performing the duties of that office with a singular amount of dignity and urbanity. For several years he continued to live in a cottage he had built for himself at Chislehurst, and there he began to form an ornithological library and collection on a scale almost unattempted hitherto in this country, though the collection was supposed to be limited to Indian, or at least Asiatic, specimens. On the death of his father, at a very advanced age, in 1876, Lord Walden inherited the Scottish peerage and estates, and thenceforth his home was mainly the old ancestral seat of Yester, near Haddington, where he entered, with the energy natural to his character, upon the life of an agriculturist; in this respect following the example of his father, who had long since turned his sword into a ploughshare, and had earned the reputation of being one of the most scientific farmers in that part of North Britain, which is the head-quarters of scientific farming.

The late Lord Tweeddale was a frequent and, when occasion required, a powerful writer. Most of his acknowledged communications are to be found in the *Journal* of the Asiatic Society of Bengal, the *Ibis*, and the *Proceedings* and *Transactions* of the Zoological Society, but it is believed that his anonymous contributions to the public press were still more numerous, though these were seldom on scientific topics. He married twice: first, the daughter of the late Count Kielmansegge, for many years the popular Minister of Hanover at this Court, who died in 1871, and secondly, a daughter of Mr. Mackenzie of Seaforth, who survives him.

One word must be said of Lord Tweeddale's generosity. No reasonable project for the advancement of zoology in any of its branches was ever started but he was ready to support it liberally. His loss will be deeply felt by a wide circle of his brother ornithologists, and the Zoological Society will find it very difficult to replace him in its presidency, a post which seems to require a peculiar position of scientific and social rank.

NOTES

WE are happy to state that at the end of the last legislative session the French Central Bureau of Meteorology obtained from the National Exchequer a sum of 120,000 francs, required for the organisation of the services which were decreed in the month of June. A semi-monthly paper will be issued by the Bureau summarising the results of observations during that period. The work of normal schools, which had been suspended during two or three years, will be resumed and published as in former times.

THE French Minister of Public Works has prepared a most important decree, which was signed on December 20 last. For the execution of the great works which have been voted by the French Parliament, an auxiliary corps of Ponts-et-Chaussées engineers has been created. The members of this newly created body will enjoy the same privileges as the government engineers who have been trained at the Polytechnic School. The consequence is that the privileges of that celebrated establishment are practically at an end, and the principle that office should be given to the fittest irrespective of their origin has a fair chance of becoming an axiom of the French administration.

THE first part of a posthumous work by Prof. Poggendorf on the History of Physics has been sent us by Messrs. Williams and Norgate. It will be completed in three parts and will contain much interesting matter collected by the late eminent physicist during his long career as lecturer at the Berlin University. We have also received the first part of the "Publications of the Astrophysical Observatory of Potsdam," containing observations of sun-spots from October, 1871, to December, 1873, by Dr. Spörer.

FROM *Science News* we learn that Mr. Alex. Agassiz left Cambridge (U.S.) on December 1 for a second dredging-trip in the West Indies on the Coast Survey steamer *Blake*. The specimens secured by him are divided among scientific men in Europe and America, who work them up, while many of them go into his own Cambridge collection. This year he will cruise between the Windward Islands and the coast of South America, having spent last winter in the Gulf of Mexico.

THE prominence given to science is a noteworthy feature in the annual summaries for the past year which appear in most of our newspapers.

WE have much pleasure in drawing our readers' attention to the following circular concerning a Society for the Collection of South African Folk Lore. The circular explains itself, and we trust that those of our readers who are interested in the subject will subscribe to the periodical which it is desired to start:—"The existence, among the aboriginal nations of South Africa, of a very extensive traditionary literature, is a well-known fact. Not a few stories forming part of this literature have been written down; and as in some of them terms occur which no longer appear to be used in colloquial language, and the meanings of which are, in many instances, not fully understood, there is no doubt that we meet in them with literary productions of great antiquity, handed down to the present generation in a somewhat similar manner to that in which the Homeric poems reached the age of Pisistratus. But European civilisation is gaining ground among the natives, and within a few years the opportunities for collecting South African folk-lore will be, if not altogether lost, at least far less frequent than they are now. This would be a great loss to 'the science of man,' particularly as there is much which is exceptionally primitive in the languages and ideas of the South African aboriginal races. There are not a few missionaries and other Europeans in South Africa who have ample opportunities for collecting South African folk-lore. Some of

these, however, are not aware of the importance of such collections, and those who are would be greatly encouraged in the task of making them, if a channel for their speedy publication existed. In the hope of contributing towards the collection of South African traditionary literature, a Folk-Lore Society is in course of formation at Cape Town, which already includes members in distant parts of South Africa. The publication of a small periodical every second month is also proposed by the Society. The annual subscription to this periodical will be four shillings, exclusive of postage. Folk-lore intended for publication in it should be accurately written down in the language and words of the narrator, and a translation into English, or some other well-known European language, added. Further information regarding facts illustrative of native life or native literature will also, whenever practicable, be published. Intending subscribers to the projected periodical are requested kindly to send in their names and addresses, stating the number of copies required by them, to the secretary of the South African Folk-Lore Society, care of Miss L. C. Lloyd, Cape Town."

ACCORDING to a report made by Prof. Palmieri an interesting application of the microphone to volcanic phenomena has just been made by Prof. Michele Stefano de Rossi, who during a series of experiments extending over several months and made at his seismic observatory at Rocca di Papa in the Albanese Mountains, has found that the present eruption activity of Mount Vesuvius could be perceived through the microphone even at that enormous distance. Prof. de Rossi, in order to continue his experiments, has recently stayed with Prof. Palmieri at the Vesuvius Observatory, and they have together visited the crater of the Solfatara near Pozzuoli, where the subterraneous work of the volcanic forces became so very evident to the sense of hearing, that a considerable amount of fear was caused amongst those present at the experiments. Prof. de Rossi will publish an account of his researches in his serial *Il Vulcanismo*.

EARTHQUAKES are reported from Seefeld (Tyrol) on December 14 at night, where the shock came in the direction from north to south, and from Luxemburg on December 15 at 11 A.M. where six or seven distinct oscillations were noticed.

NEWS from the American Republic of San Salvador states that the volcanoes of Santa Ana and of Izalco are in eruption. The eruption of the former had been anticipated for some time past (see NATURE, vol. xix. p. 86), and seems to be of particular violence.

"AN English Manufacturer" makes a strong appeal in yesterday's *Times* on behalf of the introduction into this country of the decimal system in weights, measures, and coinage. It is long since we showed the absurdity of our present systems, and the necessity for the introduction of something more scientific. But the "Manufacturer" shows that by our want of any international system, such as prevails among other nations, the trade of this country seriously suffers. We hope this aspect of the question will be urged upon the Government by all interested, and that a much more radical reform will be instituted than what has been attempted in the recent most unsatisfactory Weights and Measures Act.

THE director of the Vienna Geological Institution, Counsellor Franz von Hauer, has been nominated "Officier de l'Instruction publique" by the French Minister of Public Instruction.

M. W. DE FONVIELLE sends us the following details concerning the recent electrical observations at Montsouris Observatory:—Electrical observations are registered regularly at Mont souris seven times a day, according to a proper scale of variations, and with a Thomson electrometer. These observations are made by M. Descroix, under the general direction of M. Marie Davy, the director of the observatory, and the results are recorded daily in the Paris *Temps*. The series from the beginning of the month of December offers some notable peculiarities. The frost in Paris set in on December 7, and from that date to the 22nd there were fifteen days of continued cold. Only once, on the 18th, a thaw was for a few hours imminent, but the snow was not melted in the observatory grounds. During the whole of that period not less than 105 careful readings were taken and registered, but not a single one of these readings exhibited the least negative tendency. The variations were very few, and the sign + was always recorded. This high positive state of tension was observed in spite of a number of variations in the pressure of the air, which was almost always under 760 mm., and sometimes so low that the forecast published by the Bureau Central announced "approaching rainfall." The maximum was on the 19th, during a heavy cold fog; it was so large that the instrument was thrust out of balance, and the record of the number is wanting. The tension then exceeded 200 Daniell cells. From that time the scale of comparison was altered, so that the range of the instrument was enlarged. In consequence of this observation it was suggested that the real thaw, or change of weather would not set in without the previous appearance of negative tension. Instructions were given by M. Marie Davy to test this suggestion by a careful examination of the electrical circumstances attending this lengthened period of unprecedented cold and the future thaw which would put an end to it. The thaw set in in France on the night of December 25-26, at an hour varying according to the circumstances of the several localities. The electrical readings at Montsouris were found positive on the 25th during the whole of the day, but the mean value was greatly diminished, and the readings very unequal. On December 26 at six in the morning, negative readings were taken and registered for the first time since December 7. It must be added that under the circumstances the Thomson electrometer kept at Montsouris is not considered by M. Descroix as exhibiting the exact numerical value of the tension of the air, but merely its kind and the general progress of the phenomenon. This reservation has been made in a correspondence with Signor Palmieri, the director of the Mount Vesuvius Observatory, on the occasion of some strictures passed on the location of the Thomson electrometer used in these observations.

NO general meeting of the Association for the Improvement of Geometrical Teaching will be held in January, 1879. Considerable progress has been made by the sub-committees appointed in January, 1878, and draft syllabuses will soon be submitted to members of the Association.

M. VALENTIN has been snowed up during more than fourteen days in the observatory on the top of the Puy-de-Dome, where he takes the meteorological readings. The telegraph connecting Puy-de-Dome with Clermont laboratories being out of order no telegram has been received from him for a lengthened period. No anxiety is felt for his safety, he having been well furnished with provisions and fuel. A similar accident has befallen General de Nansouty, the director of the Pic du Midi Observatory. His telegraphist having descended to Bagnères was unable to ascend again and the General was left to his fate. As he is rather old and of delicate health heroic efforts were made by the peasantry to reach him, which they did on December 24; the telegraphic line was repaired and telegrams recorded as usual in the *Bulletin International*. General de Nansouty refused to relinquish his post, and he is spending his winter as usual on one of the highest peaks of France.

THE *Neue Wiener Zeitung* states that an electric light has been tried on a locomotive on the Vienna Railway system. The

apparatus was designed by Mr. Whitehead, the inventor of the celebrated torpedo, and is said to have worked satisfactorily.

A CORRESPONDENT writes to us that in looking through some of the drawings and prints, &c., of Old London, belonging to Mr. J. E. Gardner, F.S.A., of Park House, he came upon the following interesting handbill:—

London, 1775

Proposals
for a
Short course of lectures
on
Fossils
by
Emanuel Mendes da Costa.

The course will consist of only
TWELVE LECTURES.
A public Introductory lecture will be given *gratis* to
any one who chuse to come.

To begin *on Wednesday, 7 June, at noon*, at the Author's appartments at a shoemaker's opposite Arundel Street in the Strand and the future Lecture Hours will be determined by the subscribers.

The conditions are
One Guinea the course.
To be paid on Subscribing.
Single lectures at two shillings and sixpence each.

Subscriptions are taken in at Mr. Elmsley's, Bookseller, opposite Southampton St., Strand; Mr. White, bookseller in Fleet-street; Mr. Humphreys, dealer in shells and other curiosities in St. Martin's Lane, near Charing Cross; and by the author at his said appartments.

N.B.—The Introductory lecture will be repeated on
Thursday evening at six o'clock.

IT is proposed to hold an anthropological exhibition at Moscow in the coming summer, together with a general meeting of anthropologists from all parts of the world.

WE have on our table the following works:—"History of the Steam Engine," R. H. Thurston, Kegan Paul and Co.; "Études et Lectures sur l'Astronomie," Camille Flammarion, G. Villars, Paris; "Catalogue des Étoiles Doubles et Multiples," Camille Flammarion, G. Villars, Paris; "Sport and Work on the Nepaul Frontier," "Maori," Macmillan and Co.; "Mathematical Problems," J. Wolstenholme, Macmillan and Co.; "The Fairy-Land of Science," Arabella B. Buckley, E. Stanford; "Das Leben," Philipp Spiller, Gerstmann, Berlin; "Wanderings in Patagonia," Julius Beerbohm, Chatto and Windus; "Natural History of Victoria," Frederick McCoy, Trübner; "Fourth Annual Report of the Imperial Mint;" "Extra Physics and the Mystery of Creation," Hodder and Stoughton; "From Kulja Across the Tian Shan to Lob Nor," Col. N. Prejevalsky, Sampson Low and Co.; "The Heart of Africa," Dr. Georg Schweinfurth, Sampson Low and Co.; "The Philosophy of Science, Experience, and Revelation," John Coutts, F. Pitman; "The Native Flowers and Ferns of the United States," Parts 13, 14, 15, 16, Thomas Mechan, L. Prangola, Boston; "The Principles of Light and Colour," Edwin B. Babbitt, Trübner and Co.

THE additions to the Zoological Society's Gardens during the past week include a White-whiskered Paradoxure (*Paradoxurus leucomystax*) from East India, presented by Mr. W. G. Wilson; a Common Barn Owl (*Strix flammea*), British, presented by Mr. W. Davies; a Common Coot (*Fulica atra*), British, presented by Mr. F. H. O'Donoghue; two Philantomba Antelopes (*Cephalophus maxwelli*) from West Africa, two Egyptian Jerboas (*Dipus ægyptius*) from Egypt, purchased; three River Jack Vipers (*Vipera rhinoceros*) from West Africa, deposited.

CIRCULATING DECIMALS

THE properties of circulating decimals mentioned by Mr. R. Chartres and by Mr. E. P. Toy in NATURE (vol. xviii. pp. 291, 541) are particular cases of very general laws relating to the periods of circulating decimals of which, as they are not stated with any approach to completeness in any work on arithmetic with which I am acquainted, it may be worth while to give a brief explanation.

Consider the process of converting a vulgar fraction into a circulating decimal; take for example $\frac{1}{39}$. The work is—

$$\begin{array}{r} 39)\ 1{\cdot}00\ ({\cdot}\dot{0}2564\dot{1} \\ 78 \\ \hline 220 \\ 195 \\ \hline 250 \\ 234 \\ \hline 160 \\ 156 \\ \hline 40 \\ 39 \\ \hline 10 \end{array}$$

which may be more concisely and better arranged thus:—

$$\begin{array}{rl} 39)\ 1 & ({\cdot}\dot{0} \\ 10 & 2 \\ 22 & 5 \\ 25 & 6 \\ 16 & 4 \\ 4 & \dot{1} \end{array}$$

10, 22, 25, 16, 4 being the remainders and the corresponding quotient figures being written at the side. From this it is clear that—

$$\tfrac{1}{39} = {\cdot}\dot{0}2564\dot{1},\ \tfrac{10}{39} = {\cdot}\dot{2}5641\dot{0},\ \tfrac{22}{39} = {\cdot}\dot{5}6410\dot{2},\ \tfrac{25}{39} = \dot{6}4102\dot{5},$$
$$\tfrac{16}{39} = {\cdot}\dot{4}1025\dot{6},\ \tfrac{4}{39} = {\cdot}\dot{1}0256\dot{4},$$

and the numbers 1, 10, 22, 25, 16, 4 form a cycle such that if we divide any one of them by 39 we obtain the others as remainders in this order, and all the fractions give rise to the same period, though the beginning is made in each case at a different place in the period.

The following are three other divisions arranged in the same manner:—

$$\begin{array}{rl} 39)\ 2 & ({\cdot}\dot{0} \\ 20 & 5 \\ 5 & 1 \\ 11 & 2 \\ 32 & 8 \\ 8 & \dot{2} \end{array} \qquad \begin{array}{rl} 39)\ 38 & ({\cdot}\dot{9} \\ 29 & 7 \\ 17 & 4 \\ 14 & 3 \\ 23 & 5 \\ 35 & \dot{8} \end{array} \qquad \begin{array}{rl} 39)\ 37 & ({\cdot}\dot{9} \\ 19 & 4 \\ 34 & 8 \\ 28 & 7 \\ 7 & 1 \\ 31 & \dot{7} \end{array}$$

The four divisions thus give the values of the periods of the fractions $\frac{1}{39}, \frac{2}{39}, \frac{4}{39}, \frac{5}{39}, \ldots \frac{38}{39}$, *i.e.*, of all the proper fractions in their lowest terms, having 39 as denominator. In this case, therefore, there are four distinct periods, or, say, four periods each containing six figures; one of these, viz., that to which $\frac{1}{39}$ belongs, may be called the leading period.

In general if q be any number prime to 10, and if all the proper fractions in their lowest terms having q for denominator be converted into decimals there will be f periods each containing a digits, and a and f will be connected by the relation $af = \phi(q)$, where $\phi(q)$ denotes the number of numbers less than q and prime to it. If q be a prime, $\phi(q) = q - 1$.

It is to be observed that if we divide r and $q - r$ respectively by q the digits of the periods will in the two cases be complementary, *i.e.*, the sum of each corresponding pair will be 9. Thus in the case of 39

$$\tfrac{1}{39} = {\cdot}\dot{0}2564\dot{1} \qquad \tfrac{2}{39} = {\cdot}\dot{0}5128\dot{2}$$
$$\tfrac{38}{39} = {\cdot}\dot{9}7435\dot{8} \qquad \tfrac{37}{39} = {\cdot}\dot{9}4871\dot{7}$$

and $9 + 0 = 9$, $7 + 2 = 9$, &c. Also, the sum of each pair of corresponding remainders is q; *e.g.*, in the divisions for $\frac{1}{39}$ and $\frac{38}{39}$, $\frac{2}{39}$ and $\frac{37}{39}$, the sum of each pair of corresponding remainders is 39.

If, as in the case of 39, the remainder $q - 1$ does not belong to the leading period, the periods may be arranged in pairs, the periods in each pair being complementary to one another. If

the remainder $q - 1$ does belong to the leading period, each period will contain an even number of digits, and the first half and second half of each period will be complementary. Thus, for $q = 73$ there are nine periods: $\dot{0}136\ 986\dot{3}$, $\dot{0}273\ 972\dot{6}$, $\dot{0}410\ 958\dot{9}$, &c., and in each the two halves are complementary. If there is but one period corresponding to q, of course the remainder $q - 1$ must belong to this period, so that in this case the two halves are always complementary. Returning to the period of $\frac{1}{39}$, we see that it is such that if we multiply it by 4 we obtain the same period, only beginning with the last digit, that if we multiply it by 16 we obtain the same period beginning with the last digit but one, and so on. Thus, from knowing that the last figure of the period is $\dot{1}$, and that the last remainder is 4, we can obtain the period; for $4 \times 1 = 4$ so that the last figure but one must be 4, the last two figures must therefore be 41, multiply this by 4 we have 164, so that the previous digit must be 6, and so on. This process amounts to multiplying the 1 by 4, multiplying the 4 by 4, giving 6 and 1 over, multiplying the 6 by 4 and adding the 1, giving 25, *i.e.* 5 with 2 over, and so on, until the period is completed.

In general, in converting $\frac{1}{q}$ into a circulating decimal, if k be the last digit of the period, and r the last remainder $10r - 1 = kq$, so that the last remainder $= \frac{1}{10}(kq + 1)$ and $k = 9, 3, 7$ or 1 according as q ends in 1, 3, 7, or 9. This is, in fact, the property mentioned by Mr. Chartres and Mr. Toy; the class of relations to which it belongs, and the reason for their existence, is evident from what has been said above.

The most direct manner in which the foregoing principles can be applied to abbreviate the labour of division does not consist in multiplying the digits by the remainder from the end but from the beginning. For example, in finding the decimal equivalent to $\frac{1}{17}$ the first four digits are $\cdot 0588$ and the remainder is 4; therefore $\frac{1}{17} = \cdot 0588\frac{4}{17}$, multiplying by 4, we have $\frac{4}{17} = \cdot 2352\frac{16}{17}$ whence $\frac{1}{17} = \cdot 05882352\frac{16}{17}$; we could then find the next four digits by multiplying the four digits last found by 4 and reducing the fraction $\frac{64}{17} = 3\frac{13}{17}$, so that the next multiplication would be a multiplication of the whole period already found by 13; but as in this case the remainder does not recur after eight digits (if it did recur after eight digits the remainder would be $\frac{1}{17}$ not $\frac{16}{17}$), it must consist of sixteen digits, and the next eight are the complements of the first eight, and are therefore 94117647.

The principle of the method is to continue the division till a relatively small remainder occurs and then to multiply the figures already found by this remainder, and so on continually till all the figures are obtained. This is the method that has been generally employed in finding the reciprocals of large numbers when the whole period was required. There are several points to be attended to in order that the process may be simplified as much as possible, but these I pass over. The greatest saving of labour afforded by the principle is when a 5 or 2 occurs as remainder early in the division, as then we obtain all the remaining digits as fast as the hand can write them by division by 2 or 5 in the respective cases, without the occurrence of any fractions. Thus, for example,

$$\frac{1}{61} = \cdot\dot{0}1639344262295$$
$$0819672131147540983606557377049180327868852459\dot{} ;$$

if we perform the division till we come to the quotient digit 5 we then have a 5 remainder, and all the other digits are obtained by halving the figures from the commencement, viz., 1639. . . . The quotient can also be completed rapidly by division whenever a remainder occurs that is a submultiple of one that has previously occurred. Thus in the case of $\frac{1}{61}$, the remainder after the first 6 is 24 and after the first 8 is 12, so that the figures that follow the 8, viz., 1967 . . ., are obtained at once by halving those that follow the 6, viz., 3934. . . .

In the *Messenger of Mathematics* for April, 1878, I published the following note:—

"Write down a 5, divide it by 2 giving 2 with 1 over, divide 12 by 2 giving 6, divide 6 by 2 giving 3, divide 3 by 2 giving 1 with 1 over, divide 11 by 2 giving 5 with 1 over, divide 15 by 2 giving 7 with 1 over, and so on till the figures repeat. We thus obtain the figures 52631578947368421, and these with a cipher prefixed are the period of $\frac{1}{19}$, viz.—

$$\frac{1}{19} = \cdot\dot{0}52631578947368421\dot{}.$$

"If we start with 50 and halve in the same manner, prefixing two ciphers, we obtain the period of $\frac{1}{199}$, viz.—

$$\frac{1}{199} = \cdot\dot{0}05025125628140703517587939698492462311557788944723618090452261306532663316582914572864321608040201\dot{}.$$

"Similarly, if we start with 500 and halve as before, we obtain, after prefixing three ciphers,—

$$\frac{1}{1999} = \cdot\dot{0}005002501250625312656328164082041020510 \ldots,$$

and, generally, the process gives the reciprocal of 1 followed by any number of 9's.

"If we start with 20, 200, 2000, &c., and divide continually by 5 instead of by 2, prefixing one, two, three, &c., ciphers, we obtain the periods of the reciprocals of 49, 499, 4999, . . . For example,—

$$\frac{1}{49} = \cdot\dot{0}2040816326530612244897959183673469387755\dot{1}$$
$$\frac{1}{499} = \cdot\dot{0}02004008016032064128256513026052104208 4 \ldots.$$

"The process is very expeditious, the figures of the periods being obtained as fast as the hand can write them."

The results stated in this note were obtained as follows: the object was to find the divisors for which the first remainder was 5, so that the halving should begin from the first significant figure; these numbers are seen at once to be 19, 199, 1999 . . . Similarly the first remainder is 2 for the divisors 49, 499, 4999 . . . It should be mentioned that these are particular cases of Mr. Suffield's method of synthetic division.

If q be prime and there be only one period corresponding to q (as is the case for $q = 7, 17, 19, 23, 29, 47, 49, 59, 61, 97$, &c.), the $q - 1$ fractions $\frac{1}{q}, \frac{2}{q}, \ldots \frac{q-1}{q}$ have all the same period, viz., the $q - 1$ digits that form the period of $\frac{1}{q}$ are such, that if we multiply them by $2, 3, 4, 5 \ldots q - 1$, we always reproduce these same digits in the same cyclical order, but beginning at a different place. The case of the period of 7, viz., 142857, which is such that, multiplying it by 2, we have 285714, by 3 we have 428571, &c., is well known, and is often given as a puzzle; but the general result is a very remarkable one, *e.g.*, it is remarkable that it should be possible to write down 96 digits, such that their first 96 multiples should consist of the same digits in the same cyclical order. In the foregoing remarks I have confined myself entirely to the statement of the principles connected with the results referred to in NATURE, and to those which arise directly from them.

J. W. L. GLAISHER

SCIENTIFIC SERIALS

American Journal of Science and Arts, December, 1878.—In the opening paper Gen. Warren considers that the Minnesota Valley and the Mississippi Valley above the Ohio have been, as a rule, formed since the deposition of the glacial drift, which exists in the banks of the river, and that the Winnipeg basin drained out southward along it; also, that the loess deposits extending up to the neighbourhood of Savannah are later than the last glacial drift, &c. The hypothesis of southern elevation and northern depression (probably reversed sometimes and repeated) is relied on to explain the effects.—Prof. Dana, continuing his valuable paper on some points in lithology, contends for basing distinction in *kinds* of rocks on difference in chemical and mineral constitution as regards chief constituents, and offers a classification in eight divisions.—The principle that when the entropy of any isolated material system has reached a maximum the system is in a state of equilibrium, is developed by Mr. Gibbs as a foundation for the general theory of thermodynamic equilibrium.—Mr. McGee distinguishes crania of the mound-builders of the Mississippi Valley from those of modern Indians by a greater development of the posterior molars.—An interesting paper by the Rev. C. Hovey, on discoveries in western caves, describes, *inter alia*, the remarkable acoustic properties of Echo River passage-way (in the Mammoth Cave), where a strong vocal impulse is prolonged with sustained vigour for fifteen seconds or more; also a locality discovered last April in the Wyandot Cave, in which "pits, miry banks, huge rocks, are overhung by galleries of creamy stalactites, vermicular tubes intertwined, frozen cataracts, and all, in short, that nature could do in her wildest and most fantastic mood." There is a row of musical stalactites, very broad and thin, on which a chord can be struck, or a melody played by a skilful hand.—Prof. Harrington analyses the Chinese official almanac, issued annually in Decem-

ber, and consisting of two parts, an astronomical and an astrological.

The Journal of the Russian Chemical and Physical Societies of St. Petersburg (vol. x. No. 8) contains the following papers:—On the chlorides of benzol, by Th. Beilstein and A. Kourbatoff.—On the preparation of glycol, by S. Stempnevsky.—On allyl-dipropylcarbinol, by P. and A. Saytzeff.—On pseudopropyl-acetylene, by F. Flavitzky and P. Kriloff.—Remarks by F. Flavitzky on M. Eltekoff's paper on the action of water upon the chlorides of ethylenes and similar compounds in the presence of oxide of lead.—Observations on nitrophenols, by M. Goldstein.—On the nature and the derivates of cholesterine, by M. Valitzky.—On the neutral products of the oxidation of cholesterine, by P. Latschinoff.—On the polarisation of electrolytes, by R. Colly.

SOCIETIES AND ACADEMIES

London

Royal Society, December.—"Note on the Influence exercised by Light on Organic Infusions," by John Tyndall, D.C.L., F.R.S., Professor of Natural Philosophy in the Royal Institution.

Early last June I took with me to the Alps fifty small hermetically sealed flasks containing infusion of cucumber, and fifty containing turnip infusion. Before sealing they had been boiled for five minutes in the laboratory of the Royal Institution, They were carefully packed in sawdust, but when unpacked the fragile sealed ends of about twenty of them were found broken off. Some of these injured flasks were empty, while others still retained their liquids. The eighty unbroken flasks were found pellucid, and they continued so throughout the summer. All the broken ones, on the other hand, which had retained their liquids, were turbid with organisms.

Shaking up the sawdust, which I knew must contain a considerable quantity of germinal matter, I snipped off the ends of a number of flasks in the air above the sawdust. Exposed to a temperature of 70° or 80° F., the contents of all these flasks became turbid in two or three days.

The experiment was repeated; and after the contaminated air had entered them, I exposed the flasks to strong sunshine for a whole summer's day; one batch, indeed, was thus exposed for several successive days. Placed in a room with a temperature of from 70° to 80° F., they all, without exception, became turbid with organisms.

Another batch of flasks, after having their sealed ends broken off, was infected by the water of a cascade derived from the melting of the mountain snows. They were afterwards exposed to a day's strong sunshine, and subsequently removed to the warm room. In three days they were thickly charged with organisms.

On the same day a number of flasks had their ends snipped off in the open air beside the cascade. They remained for weeks transparent, and doubtless continue so to the present hour.

I do not wish to offer these results as antagonistic to those so clearly described by Dr. Arthur Downes and Mr. Thomas Blunt, in the *Proceedings* of the Royal Society for December 6, 1877. Their observations are so definite that it is hardly possible to doubt their accuracy. But they noticed anomalies which it is desirable to clear up. On July 10, for example, they found 9 hours' exposure to daylight, 3½ hours of which only were hours of sunshine, sufficient to effect sterilisation; while, on July 29, "a very hot day, with much sunshine," 11 hours' exposure "9 of which were true insolation," failed to produce the same effect. Such irregularities, coupled with the results above recorded, will, I trust, induce them to repeat their experiments, with the view of determining the true limits of the important action which those experiments reveal.

Chemical Society, December 19.—Dr. Gladstone, president, in the chair.—The following papers were read:—Researches on the action of the copper zinc couple on organic bodies, part ix.—Preparation of zinc methyl, by Dr. Gladstone and Mr. Tribe. (During the reading of this paper Dr. Frankland took the chair.) Methyl iodide in contact with the copper zinc couple is converted at the ordinary temperature, in from three to thirty days, into a crystalline mass of zinc methiodide. By distillation zinc methyl is obtained; the yield in one case was 99·27.—Dr. Debus made some remarks on the formula of glyoxylic acid. The author considers the formula of this acid to be $C_2H_2O_3$, in opposition to Perkin, who from quantitative experiments came to the conclusion that the true formula was $C_2H_4O_4$.—Mr. Wills gave a short communication on the production of oxides of nitrogen by the electric arc in air. The author finds that nitric acid was formed in four experiments equivalent to ·54, ·55, ·6, and ·7 gramme per hour, and points out the importance of this observation with reference to the proposed use of the electric light in dwellings.—On the action of alkaline hypobromite on oxamide, urea, and potassium ferrocyanide, part ii., by W. Foster.—On two new hydrocarbons obtained by the action of sodium on turpentine hydrochloride, by Dr. Letts. The principal point in this paper is the fact that the author has obtained a solid hydrocarbon having the formula $C_{10}H_{17}$, which he designates solid turpenyl.—On the formation of baric periodate, by S. Sugiura and C. F. Cross.—On erbium and yttrium, by T. S. Humpidge and W. Burney. The authors wished to determine the specific heats of these metals, but failed to obtain them in coherent masses. They determined the atomic weight of pure erbium to be 171·61.

Meteorological Society, December 18.—Mr. C. Greaves, president, in the chair.—P. Doyle, F.S.S., J. M. Gray, Lord Hampton, G.C.B., M. Jackson, A. Proctor, G. Simpson, and E. C. Tisdall were elected Fellows of the Society.—The following papers were read:—Abstract of the meteorology of the Bombay Presidency, by C. Chambers, F.R.S., communicated by Sir G. B. Airy, K.C.B., F.R.S., Astronomer-Royal.—Experiments with Lowne's anemometer, by Capt. William Watson, F.M.S.—Meteorology of Bangkok, Siam, by J. Campbell, Staff Surgeon, R.N.—Results of meteorological observations taken at Calvinia, South Africa, by Kaufmann I. Marks, F.M.S.

Royal Microscopical Society, December 11.—Dr. C. J. Hudson, vice-president, in the chair.—Mr. John Harrison and Dr. Alabone were elected Fellows of the Society.—Dr. Hudson described a new species of Rotifer, *Oecistes sphagni*, coloured drawings of which were exhibited. He also exhibited a number of beautiful transparent diagrams of rare species of Infusoria which he described seriatim.—Mr. F. H. Ward read a paper on a new microspectroscope without a slit, and described this and other accessory apparatus to the instrument.—Mr. F. Crisp read a paper on Hoffmann's new camera lucida, in which he described this and other recent forms of the apparatus, figures of some being drawn upon the board. Another form of camera lucida, by Dr. Russell, of Lancaster, was described and figured by Dr. Millar, and a description of a new one by Swift was also given by Mr. Ingpen.—Mr. C. Stewart read a short communication from Mr. A. D. Michael announcing the discovery of the male of *Cheyletus venustissimus*. Attention was called to a new glycerine immersion lens received from America, by Mr. Ingpen.—Mr. Beck, in reference to a suggestion for a universal unit of microscopical measurement, gave his decision in favour of divisions of the millimetre, and presented to the Society a micrometer ruled with this, and also in $\frac{1}{1000}$ inch for ready comparison.

Geological Society, December 18, 1878.—Henry Clifton Sorby, F.R.S., president, in the chair.—Rev. Frederick Charles Lambert, Robert Plant, and Ernest Swain were elected Fellows of the Society.—The following communications were read:—On remains of *Mastodon* and other vertebrata of the miocene beds of the Maltese Islands, by Prof. A. Leith Adams, F.R.S. The author recognised the following Maltese formations:—Upper Limestone.—Maximum thickness over 250 feet, passing into a sandy rock, and that into a hard red limestone. Fossiliferous, containing four Brachiopoda, several Lamellibranchs and Gasteropods, and twenty-five Echinodermata (ten being peculiar). Sand Bed.—Maximum thickness about 60 feet, variable in character, characterised by vast abundance of *Heterostegina depressa*; fifteen vertebrata. The Marl Bed.—Maximum thickness over 100 feet, but sometimes almost wholly thinned out. Organic remains rarer than in the sand bed. The Calcareous Sandstone.—Maximum thickness rather over 200 feet. Contains bands of nodules, of which the second is rich in organic remains. Hence come the noted teeth of Squalidæ. Among its invertebrate fauna are many Pecteus, with other Lamellibranchs, Gasteropods, and Brachiopods. Also twenty-two species of Echinodermata. The Lower Limestone.—Maximum thickness over 400 feet. *Scutella subrotunda* and *Orbitoides despansus* are abundant in the upper part, and it is generally fossiliferous. In a nodule-seam in the calcareous sandstone in the Island of Gozo two rather imperfect teeth of a *Mastodon* have been found. Both are penultimate molars. They agree most

nearly with the teeth of *Mastodon angustidens*, but the characters are not sufficiently well preserved to differentiate the species with certainty. The same formation has furnished teeth of a *Phoca*, to which the specific name *rugosidens* has been given by Prof. Owen. Large teeth referable to the Phocidæ are found in the nodule seams of the calcareous sandstone and in the sand bed; the marl bed has also furnished a portion of a jaw. The Woodwardian Museum contains a part of a jaw of *Squalodon*, evidently from a nodule-seam of the calcareous sandstone (found by Scilla circ. 1670). The sand bed and calcareous sandstone have furnished remains of more than one species of *Delphinus*, and large-sized Cetacean vertebræ are found in nearly all the beds, especially the sand bed. *Halitherium* has been obtained from the sand bed, marl bed, calcareous sandstone, lower limestone, and (?) upper limestone. One specimen of *Ichthyosaurus gaudensis*, Hulke, has been furnished by the calcareous sandstone; the same has also furnished *Melitosaurus champsoides*, *Crocodilus gaudensis*, and *Sterrodus melitensis*. *Myliobates toliapicus* and allied species have come from all the deposits except the upper limestone. *Otobates subconversus* from the sand bed and marl. The squalidæ are abundant from all the deposits except the first. There are ten species belonging to the following genera:—*Carcharodon*, *Carcharias*, *Oxyrhina*, *Hemipristis*, *Corax*, *Odontaspis*, *Lamna*. Remains of *Notidanus*, *Platax*, and *Diodon* have also been found.—Dinosauria of the Cambridge greensand, Parts I.-VII., by Prof. H. G. Seeley, F.L.S. F.G.S. The author stated that this paper was founded upon the collection of more than 500 dinosaurian bones preserved in the Woodwardian Museum, for the opportunity of studying which he was indebted to the kindness of Prof. T. McKenny Hughes. He described the conditions under which the specimens occur, and accounted for the apparently worn state of the bones as the results of exposure to the air, and subsequent maceration.—I. Note on the axis of a dinosaur from the Cambridge greensand.—II. On the vertebral characters of *Acanthopholis horridus*, Huxley, from the base of the chalk-marl near Folkestone.—III. On the skeleton of *Anoplosaurus curtonotus*, Seeley.—IV. On the axial skeleton of *Eucercosaurus tanyspondylus*, Seeley.—V. On the skeleton of *Syngonosaurus macrocercus*, Seeley.—VI. On the dorsal and caudal vertebræ of *Acanthopholis stereocercus*, Seeley.—VII. On a small series of caudal vertebræ of a dinosaur, *Acanthopholus eucercus*, Seeley.

CAMBRIDGE

Philosophical Society, November 18.—Prof. Liveing, president, in the chair.—The following communication was made to the Society:—Some results of the two last total solar eclipses, by Dr. A. Schuster. Every scientific investigation passes through a preliminary stage, in which a general survey of the facts is taken, and by means of which the most hopeful line for future inquiry is determined. Eclipse observations may be said to have just passed through that preliminary stage. The present is therefore a fitting time for a general survey of what has been done, and a discussion of what remains to be done. Eclipse observations may be divided into three classes: spectroscopic observations, polariscopic observations, and general observations on the outline and shape of the corona, which can best be carried on by means of good photographs. 1. Spectroscopic observations.—The spectrum of the corona consists: of a continuous spectrum, in which the dark Fraunhofer lines are faintly seen; of the spectrum of hydrogen gas, and of an unknown line in the green. The pressure of a continuous spectrum indicates the presence of solid or liquid particles, and is most likely partly due to matter falling into the sun. During the last eclipse the first systematic attempt to determine the height to which the continuous spectrum extends was made by Prof. Eastman, assisted by Mr. Pritchett. The result was rather remarkable, for although the corona was not equal in intensity in the four directions, the spectrum disappeared nearly at the same distance all round the sun. The importance of obtaining photographs of the spectrum was pointed out. The various attempts that have been made were mentioned, and the result of the Siamese photographs was compared with that of a photograph of the spectrum obtained by Dr. Henry Draper during the late eclipse. The comparison proves that during the late eclipse, the line spectrum was much fainter. All observers agree on this fact, and Prof. Young's opinion, which is decisive on that point, was quoted. The idea of connecting this fact with the minimum of sun-spots through which we are at present passing is obvious. 2. Polariscopic observations.—Polariscopic observations tend to show that close to the sun the polarisation is small, that it increases up to a distance of a few minutes, and then rapidly diminishes. The author has made a calculation as to what the polarisation ought to be, and has come to the result that in whatever way the scattering matter is distributed, as long as it vanishes nowhere, the polarisation ought rapidly to increase with the distance from the sun. The only way to account for the discrepancy between this result and the actual fact is by assuming that as we move away from the sun, more light is reflected in the ordinary way and less light is scattered. Matter falling into the sun and being gradually broken up by the heat would account for all the facts. 3. General outline of the corona.—It has often been remarked that the corona shows an approximate symmetry round the sun's axis. The author supports the view that the greater extension in the direction of the sun's equator is due to meteor streams which approximately circulate in that plane. He quotes in support of this a fact noticed by him during several eclipses, which indicates that a certain departure from this symmetry takes place in such a way that the corona is wider and more extended on one side of the axis than on the other, and he gives evidence that this departure from symmetry takes place in a direction fixed in space. The statement made by several observers that there is a connection between sun-spots and the sun's corona has induced the author to look carefully over photographs and drawings of the corona made during the last eight eclipses. He has found that during this time the general outline has varied gradually and systematically in a cycle corresponding to that of the sun-spots. The following hypothesis, which seems to account for many facts, was brought forward by the author. A meteor stream is circulating round the sun in a very eccentric orbit. A number of meteors in their perihelion passage are falling into the sun, owing to the increased chances of collision amongst themselves, disintegration owing to rise of temperature and entry into the solar temperature. The local increase of temperature caused by the fall must give rise to currents on the surface of the sun, and may give rise to cyclones which we call sun-spots. If the meteors have a period, so that every eleven years an increased quantity passes the perihelion, a greater number of sun-spots would form, and at the same time we should observe a difference in the shape of the corona, which may well be of such a nature as is actually observed. Dr. Schuster also exhibited to the Society Grant's small calculating machine, for the multiplication of eight figures by eight; he explained its construction, and compared it with that of Thomas of Colmar, which is in general use. Grant's machine is much smaller than Thomas's, but does not perform subtraction directly, as is the case with the latter.

BOSTON, U.S.A.

American Academy of Arts and Sciences, December 11, 1878.—Hon. Charles Francis Adams in the chair.—Prof. Alexander Graham Bell presented a paper upon the use of the telephone in tracing equi-potential lines and surfaces. The results of previous observers, especially those of Prof. Adams, were referred to, and Prof. Bell showed that these lines could be traced more readily with a telephone than with a galvanometer. He made use of a steel-band telephone, which could be clasped about the head, leaving the hands free to perform the experiments. In this way the lines were traced in solids and liquids. By the use of metal exploring rods the equi-potential lines could be traced in the earth about one's feet, or in the neighbourhood of metallic deposits, and might lead to the discovery of metallic deposits or peculiarities in the homogenity of the earth.—Prof. John Trowbridge read a paper upon the results of measurements conducted by himself and Prof. W. H. Hill, of the United States Torpedo Station, at Newport, R.I., upon the heat produced by the rapid magnetisation of iron, nickel, and cobalt. The nickel and cobalt contained from $\frac{2}{10}$ to $\frac{5}{10}$ of 1 per cent. of iron, which was inappreciable in the electro-dynamic experiments. The work done was measured in metre grammes, and gave the result that the molecular heating of equal volumes of iron, nickel, and cobalt can be expressed in metre grammes as follows:—Iron = 2381·43, cobalt = 1906·50, nickel = 1112·11.

PARIS

Academy of Sciences, December 16, 1878.—M. Fizeau in the chair.—The following papers were read:—Observations on M. Pasteur's note on alcoholic fermentation, by M. Berthelot. He describes an arrangement he made for effecting simultaneous

hydrogenation and oxygenation of sugar (by electrical means); he notes that there was a slight production of alcohol.—Study of ordinary and compound engines, &c. (continued), by M. Ledieu.—Report on Mr. Wharton's marine compass, with needle of nickel. Its trial in the navy is recommended.—On the reptiles of primary times, by M. Gaudry. This relates to permian fossils found at Autun. In the vertebræ of Actinodon the parts of the centrum, already in great part formed, but not united, indicate the passage of the imperfect vertebrate to the perfect. M. Gaudry refers to two new reptiles, *Pleurononia pellati* and *Euchyrosaurus rochei;* the latter's name indicates the fact of its having been more adroit with its fore-limbs than reptiles of the present.—Reply to M. Sire's observations on a gyroscopic apparatus, by M. Gruey.—On a new phenomenon of static electricity, by M. Duter. He repeated his experiment, with vessels of the same volume, but with different thicknesses of glass. The variations of volume were nearly in inverse ratio of the squares of the thicknesses.—Artificial production of nepheline and amphigene, by the method of igneous fusion and reheating at a temperature near fusion, by MM. Fouqué and Levy.—Third note on vaccinal infection; elaborative *rôle* of the lymphatic ganglions, by M. Raynaud.—The memoir by Sadi Carnot, "Reflexions sur la Puissance motrice du Feu," published in 1824, and regarded as the origination of the new science of thermodynamics, had very little publicity. His brother, M. H. Carnot, has issued a new edition, with notes (hitherto unpublished), which show that S. Carnot foresaw, with much distinctness, the consequences that would result from his ideas. A copy of the work, with the MS., was presented to the Academy.—M. Mouchez presented drawings of heavenly bodies, by M. Trouvelot (United States).—On the solar spots and protuberances observed with the equatorial of the Roman College, by P. Ferrari. Little more than two tables relating to the second half of 1877.—On the summation of series, by M. André.—On elimination, by M. Mansion.—On the different properties of the mode of distribution of an electric charge on the surface of an ellipsoidal conductor, by M. Boussinesq.—On the spectrometric measurement of high temperatures, by M. Crova. Take, as term of comparison, the flame of a moderator lamp, and let it be 1,000 on the (arbitrary) optical scale of temperature. Then measure with the spectrophotometer the ratio of the intensities of two radiations λ and λ' in the source of unknown temperature and in the lamp-flame. The quotient of these two ratios will be above or below 1,000 according as the temperature of the source in question is above or below that of the lamp-flame. M. Crova gives several examples of his measurements, and thinks the method applicable to measuring the temperature of the sun and stars; also of various industrial hearths.—Specific heat and heat of fusion of palladium, by M. Violle.—Influence of temperature on rotatory magnetic power, by M. Joubert. This relates chiefly to flint (regarding which there has been some discrepancy). M. Joubert finds that the rotatory power increases with rise of temperature, and about $\frac{1}{10}$th of its value, in passing from the ordinary temperature to that of fusion. His methods are described. He succeeded also in measuring the rotation in a body under the sole influence of terrestrial magnetism alone.—On the densities and the coefficients of dilatation of liquid chloride of methyl, by MM. Vincent and Delachanal.—On the oxidation of some aromatic derivatives, by M. Etard.—On the nature of certain accessory crystallised products, in industrial treatment of petroleums of Pennsylvania, by MM. Prunier and David. These rank parallel (mostly) with those extracted from coal oils or derivatives by pyrogenation from benzine.—Researches on urea, by M. Picard.—On hæmocyanine, a new substance from the blood of the poulp, by M. Fredericq. This contains copper, and seems to play a similar *rôle* in respiration to the hæmoglobine in vertebrates.—Influence of different colours of the spectrum on development of animals, by M. Yung. The experiments were on eggs of frog, trout, and Lymnea. Violet is the most favourable light, next comes blue, then yellow and white; red and green seem hurtful. Darkness does not prevent development, but retards it.

December 23, 1878.—Explosion of fuze materials, by M. Dupuy de Lome. This relates to a recent accident to M. Zédé when experimenting with a mixture of gun-cotton and nitrate of ammonia. The mode of combustion suddenly changed under a very slight increase in the tension of the gas.—Formation of leaves and order of appearance of their first vessels in Gramineæ, by M. Trecul.—Craniology of the Papuan race, by M. de Quatrefages. A *résumé* of the seventh volume of his and M. Hamy's work, "Crania Ethnica."—Experiments on the movements of liquid molecules of current waves, considered in their mode of action on the progress of ships, by M. de Caligny.—Mr. Norman Lockyer communicated his paper recently read to the Royal Society.—M. Damon was elected free member, in place of the late M. Belgrand.—On a process for measuring with precision the variations of level of a liquid surface, by M. le Chatelier. A point immersed in the liquid is raised gradually till its extremity is tangent to the surface. The moment at which this is passed is indicated by deformations of the liquid surface, and these deformations are observed by means of light thrown on the surface, reflected, and observed with a lens, the focal plane of which passes through the end of the point. So long as the point is under water one sees a circle uniformly illuminated, but immediately the point emerges a black spot appears in the circle. The method gives very delicate measurements, and one application designed is a very sensitive manometer for detecting weak currents of air (as in mines).—On the determination of the imaginary roots of algebraic equations, by M. Farkas.—On the theory of perturbations of comets, by M. Mathieu.—Results of solar observations during the third quarter of 1878, by M. Tacchini. The calm was increased. Of 100 days of observation, 90 were without spots. He thinks the minimum will probably be passed in 1879. In the zones of maximum frequence of the protuberances there is a minimum of the faculæ, and *vice versâ.* There is a difference in distribution of the protuberances at the epochs of maximum and minimum of spots. There were no metallic eruptions or elementary spectra, &c.—On a new thermograph, and on a general method of integration of any numerical function, by MM. Pictet and Cellerier. Knowing the tension of a vapour, one may determine *à priori* the corresponding temperature.—Magnetic rotation of the plane of polarisation of light under the earth's influence, by M. Becquerel. M. Joubert's experiment was a repetition of M. Becquerel's.—On a new phenomenon of static electricity, by M. Duter. He repeated M. Govi's experiment with mercury (which had left doubts), and got contraction as in other cases.—On four singular epochs of the annual course of meteorological elements, by M. Ragona.—Preparation of cobaltocyanide of potassium and some derivatives, by M. Descamps.—Action of trimethylamine on sulphide of carbon, by M. Bleunard. He describes some of the properties of sulphocarbonate of trimethylamine, and its combinations with the mineral acids.—On the chromatic function in the poulp, by M. Fredericq. The changes of colour in the animal's skin are analogous to those produced by the vaso-motors in the human face; they express various emotions, especially anger or fear. The deep coloured phase represents the state of activity of the muscles of the chromatophores; the phase of decoloration, the passive state of retraction of these bodies.—On the excretory apparatus of *Solenophorus megalocephalus*, by M. Poirier. Previous accounts he finds erroneous.—New researches on suspension of the phenomena of life in the embryo of the hen, by M. Dareste. A continuation of his former experiments, but with use of different temperatures. The results were conformable to what he expected.—On the tertiary strata of Britanny.

CONTENTS

THURSDAY, JANUARY 9, 1879

AMERICAN SURVEYS AND EXPLORATIONS

ATTENTION has frequently been called in these columns to the progress of American exploration. Only a few months ago[1] reference was made to the want of concert among the different surveying expeditions, to the consequent loss of labour and reduplication of work, and to the desirability of consolidating the whole exploratory service under one connected organisation. It is satisfactory to know that an important movement in this direction is now in progress, and that Congress has called in to its assistance the advice of the most eminent scientific authorities in the States.

Readers of NATURE may remember that a few years ago (1874) a discussion was raised in Congress as to the alleged repetition of the survey of the same area of territory by independent expeditions, and that a committee of inquiry was appointed to take evidence on the subject and report. The result of that inquiry was a recommendation that the Engineer Department should be restricted to such surveys as might be necessary for military purposes; but "that all other surveys for geographical, geological, topographic, and scientific purposes should be continued under the direction of the Department of the Interior." It was easy to see from the evidence given before this Committee that a good deal of personal feeling had been evoked by the conflict of interests among the various surveying corps. The Engineer Bureau, in particular, with its well organised military equipment and its just pride in the large amount of exploratory work it had accomplished, seemed to resent the existence of the civilian expeditions as an infringement of its own proper sphere of operations. We may suppose that it was proportionately chagrined by the decision of the Congress Committee.

There was thus no great love between the rival surveyors in the beginning, and heaven seems to have decreased it on better acquaintance. With their plotting and counter-plotting, of which there has, no doubt, been plenty, we have of course nothing to do. Last summer the subject came up again before Congress. Representative Hewitt moved a resolution there, referring the question of the Geological and Geographical Surveys of the Territories for consideration and report by the National Academy of Sciences. It was known that double surveying had been carried on to a large extent, notwithstanding the information elicited and recommendations given by the Congressional Committee of 1874. One officer, indeed, was alleged to have duplicated surveys to the extent of more than 100,000 square miles, at a cost to the public exchequer of nearly half a million of dollars. The object of the resolution in Congress is said to have been to consolidate the power of the military surveys; but certainly nothing could be more impartial and sweeping than the law passed last June. It was to the following effect:—"The National Academy of Sciences is hereby required, at their next meeting, to take into consideration the methods and expenses of conducting all surveys of a scientific character under the War or Interior Department, and the surveys of the Land Office, and to report to Congress, as soon thereafter as may be practicable, a plan for surveying and mapping the territories of the United States on such general system as will, in their judgment, secure the best results at the least possible cost; and also to recommend to Congress a suitable plan for the publication and distribution of the reports, maps, and documents, and other results of said surveys."

[1] NATURE, vol. xviii. p. 694.

The Academy, in accordance with this requirement, appointed a committee to consider the question. The weight of authority of this Committee may be judged from the names of its members: O. C. Marsh, James D. Dana, William B. Rogers, J. S. Newberry, W. P. Trowbridge, Simon Newcomb, Alexander Agassiz. The finding arrived at by such a group of men must command respect all over the Union, as it will on this side of the Atlantic. At a meeting of the Academy held in New York on November 6th, the result of the deliberations of the Committee was presented in the shape of a formal report, which, being approved and adopted, was forwarded to the President of the Senate on the 26th of the same month.

In this Report the various surveys of the public domain are broadly grouped into two divisions: 1. Surveys of mensuration; and 2. Surveys of geology and economic resources of the soil. Each of these divisions is discussed somewhat in detail.

1. Under the first group are included no fewer than five different and independent organisations: 1. The Coast and Geodetic Survey. 2. The surveys carried on by the War Department to the west of the 100th meridian. 3 and 4. The topographical portion of the work carried on by the two surveys under the Interior Department. 5. The survey for land-parcelling under the Land Office. Between these various kindred works no concert or co-ordination of any kind exists. In the language of the Report, "their original determinations of position are independent, their systems of surveys discordant, their results show many contradictions, and involve unnecessary expenditure." On the one hand the geographical reconnaissances of the Engineers and the Interior Department are too sketchy to serve for the subdivision of public lands; on the other hand, the land-parcelling surveys are of correspondingly slight topographical or geographical value. The National Academy insists that as all these surveys must be based upon accurate determinations of position, they can never be effectively and economically conducted until they are united into one system conducted under the same head. On a review of the powers and capabilities of the different surveying staffs, the Academy has come to the conclusion that the Coast and Geodetic Survey is, practically, best prepared to undertake the charge of the unified system proposed to be established. It recommends that this survey be transferred from the Treasury to the Department of the Interior, and that, with its modified and extended functions, it should hereafter be known as the United States Coast and Interior Survey, with a Superintendent appointed by the President, and reporting directly to the Secretary of the Interior. The duties of this branch of the public service, besides those of the present Coast and Geodetic Survey, should include a rigid geodetic survey of the whole public domain; a topographical survey, including detailed topographical work, as well as

rapid reconnaissances like those now carried on by the War and Interior Departments; and, lastly, surveys for the parcelling of public land.

2. Having regard to the enormous area of territory yet to be explored and surveyed, its vast mineral wealth, its agricultural and pastoral resources, its stores of timber, its capabilities of soil, the Academy believes that the best interests of the country require that, for purposes of intelligent administration, a thorough knowledge must be obtained of the geological structure, natural resources, and products of these regions. It therefore recommends the establishment of an independent organisation, with a Director appointed by the President, to be placed under the Department of the Interior, and to be styled the United States Geological Survey. The duties of this Survey would include the investigation of the geological structure and of the economic resources of the public domain.

This consolidation of all the surveying work, sanctioned and paid for by Congress, would of course involve radical changes in some of the Departments. The Bureau of Engineers, in particular, would be required to give up all surveying work except what might be necessary for merely military purposes, and for such engineering operations as the rectification of rivers, irrigation and drainage, reclamation and protection of alluvial land. The various geographical and geological surveys west of the 100th meridian, now carried on by the War and Interior Departments, would be discontinued, though of course they would, in some cases, be resumed under the proposed new organisation.

Three distinct branches of the public service are thus proposed to be established for dealing with the public domain:—First, the United States Coast and Interior Survey, charged with the accurate mapping of the country; second, the United States Geological Survey, for the investigation of the geological structure and natural resources of the domain; third, the Land Office, having charge of the subdivision and sale of the public lands, and entitled, therefore, to call upon the Coast and Interior Survey for all necessary surveys and measurements, and upon the Geological Survey for all information as to the value and classification of lands.

Considerable liberty is proposed to be given to the chiefs of the two surveys as to the nature and extent of their publications. They are each to present an annual report of operations, and provision is to be made for the issue of such maps, charts, reports, discussions, treatises, and other documents as they may deem to be of value. Most liberal provision is likewise recommended to be made for the distribution of the reports of the Surveys. Besides the number of copies required by Congress for its own use, 3,000 copies are proposed to be published for scientific exchanges by the heads of the surveys and for sale. The special reports are to be issued in uniform quarto size, liberty being left to each director to choose such a form for his chartographic publications as shall combine the most effective style with the greatest economy. All specimens collected by the two surveys when no longer required for the investigations in progress are to be transferred to the National Museum.

Such in brief are the recommendations made by the National Academy in response to the requirement of Congress. That they are eminently wise and thoroughly practicable must be freely admitted by all capable of forming an opinion on the subject. It is simply impossible that things can go on as they are. Each one of the Surveys now in progress has done good work; several of them most admirable work. But work as good could be got with less labour and at less cost. This cannot be effected without combination; and the Academy has pointed out with great clearness and judgment how the combination may be achieved. It is not to be expected that changes of this kind can be carried out without irritating some of the individuals whose position is thereby affected. But save the severance of the Bureau of Engineers from all control of the surveys there need be comparatively little disturbance of the work now going on. Dr. Hayden, who with his staff has done so much in recent years for American geology, would doubtless take a high command under the new system; and it may be hoped that his position will be so secured as to enable him to devote his whole time to the scientific work for which he has shown himself to be so admirably qualified. Mr. Powell and his colleagues might continue their interesting and important Colorado investigations. To Mr. Clarence King fresh fields of research lie open where he may win laurels as bright as those he now wears. To all these officers in so far as they have at present geographical and topographical work to carry on, the allocation of all such duties of mensuration to a special geodetic survey should be a welcome relief, as it will set them free for their own special investigations. The Academy in its Report contemplates the possibility of officers, both of the army and navy, being desirous to volunteer for employment in these surveys, and recommends that when their services are not otherwise required they should be permitted to take part in the general survey. In this way a connection with the engineers might be re-established, and we may be sure that every engineer officer of capacity would be welcome, and would take a good position under the Department of the Interior.

The Report of the Academy, on being presented to Congress, was, on the 2nd ultimo, referred to the Committee on Appropriations, and ordered to be printed. As Congress rises at the beginning of March, some action may be expected to be taken on the matter before that date. It will be a subject for sincere congratulation among all well-wishers of American science and general progress, should the decision be in the direction pointed out by the National Academy of Sciences.

ARCH. GEIKIE

KERNER'S "FLOWERS AND THEIR UNBIDDEN GUESTS"

Flowers and their Unbidden Guests. By Dr. A. Kerner. With a Prefatory Letter by C. Darwin, M.A., F.R.S. The translation revised and edited by W. Ogle, M.A., M.D. (London: C. Kegan Paul and Co., 1878.)

THIS charming book is the record of an extension, in a somewhat different direction, of the researches of Darwin, Hermann Müller, and others, on the assistance rendered by insects in the cross-fertilisation of flowers. Attention has hitherto been directed almost exclusively

to the contrivances by which insects, while feeding on or sucking the honey from the nectary, are almost compelled to be the involuntary agents in the transmission of the pollen from the anther to the stigma in a different flower of the same species. In almost all cases, however, it is only a comparatively small number of insects that would be adapted, by the size of their body, the length of their proboscis, and other points in their structure, to insure the cross-fertilisation of any particular species of flower. It is obvious that the visits of all other insects, which would consume the honey without aiding in the transmission of pollen, not only can be of no advantage, but must be positively injurious to the plant, by preventing the visits of the useful insects. It is this point that Prof. Kerner has studied with great industry and acumen; and he has compiled a number of most interesting illustrations of the contrivances presented in the structure of the flower, which not only force the useful insects to enter it in the particular way which shall be of most advantage to it, but keep out all others.

Dr. Kerner is dissatisfied with the vague use in botanical works of the words "self-fertilisation" and "cross-fertilisation," and proposes to substitute them by others which shall have a more definite meaning. He uses the term "autogamy" for the fecundation of a flower by the pollen from the andrœcium of the same flower; "geitonogamy" for the fecundation of a flower by pollen from other flowers on the same plant; and "xenogamy" for fecundation by the pollen from a flower on another plant; while the term "allogamy" would include the last two as contrasted with autogamy. The only previous attempt at a strict scientific terminology with which I am acquainted is by Delpino,[1] who distinguishes four kinds of fecundation: homoclinic homogamy, fecundation by pollen from the andrœcium of the same hermaphrodite flower; homocephalic homogamy, fecundation by pollen from the andrœcium of a different flower of the same inflorescence; monoicous homogamy, fecundation by pollen from the andrœcium of a flower belonging to a different inflorescence on the same plant; and dichogamy, fecundation by pollen from the andrœcium of a flower on a different plant. Prof. Kerner's terms will at all events be admitted to have the merit of being the simplest, and will probably be generally adopted in future.

It may be stated as a general rule that, while the visits of winged insects are beneficial, those of wingless insects are mostly injurious, since these consume the nectar without immediately afterwards visiting another flower so as to favour allogamy. Hence the majority of the contrivances which Kerner describes are for the purpose of excluding from the flower wingless insects, and in particular ants, the great enemies of flowers, and the aphides which attract ants. The flora of the mountain valleys of the Tyrol furnishes almost inexhaustible material for working out a problem of this kind; and of this material Dr. Kerner, who holds the position of professor of botany in the University of Innsbrück, has availed himself to the full.

Simplest among the contrivances for excluding useless, while admitting useful insects, are the more or less dense collections of hairs which cover up the entrance to the nectary in so many flowers, rendering the road inaccessible to very small insects, while presenting no obstacle to larger insects which can brush them aside or pierce them with their proboscis. Beautiful instances of this arrangement are described and illustrated in the work before us in the cases of *Anchusa arvensis*, *Lonicera alpigena*, *Veronica chamædrys* and *officinalis*, *Malva rotundifolia*, *Monotropa hypopitys*, *Menyanthes trifoliata*, and many others. Sometimes these coverings take the form of appendages to the corolla which protect the openings to the nectaries with flaps, as in *Gentiana nana* and *Soldanella alpina*. Again, access to the nectary by small insects is often prevented by the parts of the flower being bent, dilated, or crowded together, the same contrivances resulting also in forcing those insects which are useful to enter and leave the flower in such a way as to be of the greatest service in the transmission of pollen. Thus, in *Nigella* each spoon-shaped petal is hollowed out into a kind of pit, into which nectar is secreted in abundance. At the point where the handle of the spoon is continuous with the bowl the petal gives off an excrescence which covers in the whole nectar-cavity like a lid, closing it completely, and no insect can possibly rifle the nectar unless it be strong enough to lift up this lid. Ants are by this means entirely excluded, while the common honey-bee is able to lift the lid with ease, in doing which he must inevitably rub against either anther or stigma, according to the stage of development of the flower. In his "Entdeckte Geheimniss der Natur" (1793) Sprengel describes, with wonderful accuracy, the proterandrous flower of *Nigella*, and the mode in which the structure of the various parts secures allogamy by the agency of bees. He observed the flaps over the nectaries, but suggests no other interpretation of them than to serve as a protection against rain. A similar occlusion of the nectary against small insects is effected in *Cynoglossum* by the outgrowths from the mouth of the corolla, in *Campanula* and *Phytemna* by the strap-shaped lower parts of the filaments, in *Ranunculus glacialis* and many Mesembryanthemaceæ and Cactaceæ by the mass of crowded filaments, in *Pentstemon* by the fifth barren stamen.

These protective and prohibitive arrangements are frequently placed, not inside, but outside, the flower, one very common form being the hairs, glandular or not, with which the calyx, bracts, or upper part of the stem are so frequently clothed; and the purpose is still more effectively answered when the glandular hairs or the stem itself exude a viscid secretion. In such plants as *Cistus ladaniferus*, *Listera ovata*, *Geranium sylvaticum*, *Euphrasia viscosa*, *Lychnis viscaria*, and a large number of species belonging to the orders Caryophyllaceæ, Saxifragaceæ, Labiatæ, and Scrophulariaceæ, it is almost impossible for the flower to be visited by any but winged insects. Prof. Kerner believes that the numerous insects which are found adhering to the viscid peduncles and stems of these plants are not digested so as to serve the plant with nutriment, but that the object of their destruction is simply to prevent their reaching and rifling the nectary. An explanation is thus offered of the well-known fact that the same species will frequently assume a hairy habit when growing on land, and a glabrous habit when growing in water, water-plants being almost invariably destitute of hairs. An admirable instance of this

[1] *Nuovo Giornale Botanico Italiano*, vol. viii., 1876, p. 146.

law is furnished by *Polygonum amphibium*. Kerner shows that the nectaries of this plant are entirely unprotected against the incursions of "unbidden guests." When growing in water this is no disadvantage, because none but flying insects can reach the flower. But when growing on land the nectar would be liable to be rifled by small creeping insects that would carry it away without performing any compensating service to the plant, and in such circumstances an innumerable quantity of glandular hairs make their appearance on the epidermis of the leaves and stem which effectually bar the way against the unwelcome visitors. "If the ground on which a *Polygonum* has grown for years in dryness, so as to have become covered with these trichomes, again be flooded, and the stems and peduncles again therefore be encircled with water, the trichomes with their viscidity disappear, and the epidermis again becomes smooth and even." I find this statement difficult to reconcile with a dictum laid down further on in the volume—and, as it appears to me, laid down hastily without sufficient warrant—that "the so-called process of 'adaptation' is never a direct one, never comes simply in response to a want. In other words, external conditions can never occasion an inheritable change of form, whether advantageous or the contrary, can neither determine the development of an organ nor its abortion."

Although glandular hairs or viscid secretions are the most common contrivances for preventing the access to the nectary of useless insects, they are by no means the only ones. The same object is attained by the prickles which cover the upper portion of the stem or the peduncles, and the spines into which the involucre of many Compositæ is converted. The waxy or even the glabrous epidermis in some plants prevents creeping insects from reaching the flowers. Even the latex or milky juice of such orders as Euphorbiaceæ, Convolvulaceæ, and Cichoriaceæ is pressed into the service. Kerner placed various kinds of ants on plants that were full of milky juice, such as the common lettuce. No sooner did they reach the uppermost leaves or peduncles than their feet cut through the tender epidermis of those parts, causing the latex to flow, which immediately glued the little animals to the stem so that they were totally unable to escape, and most of them miserably perished. The extra-floral nectaries, on the leaves or other parts of the plant, of *Viburnum tinus* and *opulus*, *Impatiens bicornis*, and many Leguminosæ, serve a similar purpose of diverting creeping, but not winged insects from the flower; since an insect crawling up the stem would always reach these secretions of nectar before the flower.

Some plants have to be protected from animals of a larger size, ruminants and other herbivorous quadrupeds. Some are altogether so protected by their prickly stem and leaves, or by the nauseous or unwholesome secretions of their tissues. But unpalatable secretions are much more common in the petals than the leaves; and with many plants the leaves are eagerly devoured by grazing animals or by caterpillars, while the flowers are left entirely untouched. While the comparatively large size of the flowers of alpine plants no doubt has for one object the attraction of hymenoptera and lepidoptera from a distance, the large area occupied by them in comparison to the leaves—the very character which renders many of them such favourite ornaments of our rockeries and flower-beds—doubtless also serves to protect them from destruction by goats and other mountain quadrupeds.

Space does not allow me even to refer to many other singular and interesting relationships pointed out by Prof. Kerner. It is of course quite possible that further examination may modify some of his conclusions in their detail. For example his belief that the main object of the viscid secretion on the leaves of *Pinguicula* is to prevent the access of creeping insects to the flower hardly appears consistent with the fact that most species of the genus flower early in the spring, while the secretion continues its activity through the summer and autumn. But the book is a perfect mine of original research, and is indispensable to all who are interested in the many problems connected with the fertilisation of flowers.

Dr. Ogle's translation is, with but little exception, easy and graceful. His editorial notes are useful, and he has adopted the praiseworthy practice—since the work is intended for non-scientific as well as for botanical readers—of explaining in foot-notes the meaning of technical terms used by the writer. In a future edition this practice might with advantage be extended. Such a term as "epiblasteme" does not carry its own meaning with it; and even botanists not well read up in recent literature would be puzzled by it. Or perhaps a glossary would be more useful. Three large-sized lithographic plates crowded with detail add greatly to the lucidity of the descriptions.

ALFRED W. BENNETT

FLAMMARION ON DOUBLE STARS

Catalogue des Étoiles Doubles et Multiples en Mouvement relatif certain. Par Camille Flammarion. (Paris: G. Villars, 1878.)

IN this compact volume of less than two hundred octavo pages M. Flammarion has collected together the large number of measures of double and multiple stars, exhibiting change in the relative positions of the components, which have been made by various observers since the time of the father of double-star astronomy, Sir W. Herschel. Those who have been occupied in the study of this branch of the science will be well aware of the difficulty and trouble attending the preparation of a complete history of any of these objects from the measures being scattered through a great many astronomical works, some of them not always easily accessible, and M. Flammarion has not yet attached his name to any volume which is likely to compare with the present one in usefulness.

The author's authorities are about one hundred in number, and he refers to them by abbreviations, a list of which precedes his catalogue, but it is to be regretted that he has not also prefixed the titles of the volumes whence the various measures have been taken, and the more so as there are indications that the original authorities have not been invariably consulted. Thus a number of Capt. Jacob's measures made with the Lerebours equatorial at Madras and published in the first catalogue in the volume of Observations 1848-52, are omitted in M. Flammarion's lists, though he has others which appear in the second catalogue in the same volume, formed after the substitution of a new object-glass. In the case of π Lupi, where he regrets "que les étoiles australes soient

si négligées," he has omitted all Capt. Jacob's measures subsequent to 1848, and as instances where some measures are wanting, may be mentioned γ Argûs, ρ Herculis, δ Herculis, τ Ophiuchi, 70 Ophiuchi, ε Equulei 61 Cygni, θ Indi, &c., &c.

After exhibiting the measures of each object, M. Flammarion, in the great majority of cases, appends his own conclusions with respect to the cause of the relative changes of position, which have generally been carefully considered, though there are some few in which we should hardly be disposed to follow him. But the reader having nearly all that is known of the different objects before him, in M. Flammarion's summary, will be able to form his own inferences. If an observer he will be guided thereby to a selection of objects most worthy of his attention, or most requiring further measures for the elucidation of the cause of altered position.

In a provisional examination of the volume ample proof is afforded of the care taken by the author in his work, which has no doubt been as he describes it long and laborious. There are a few such oversights as H_2 3678 for H_2 4087; and under Procyon, misled by a measure of Secchi's in 1856 as printed, he refers to a companion at 83°·6 and 33″·16; this measure, however, really belonged to Powell's distant companion, and instead of 33″·16 the distance should be 331″·6, as it is given in another page of the same volume of Memoirs of the Roman Observatory. There is no reference to some of Argelander's determinations of proper motion, as in the case of a distant companion of γ Leonis, upon which M. Flammarion enters into some detail. Omissions like this, however, are perhaps unavoidable in the first preparation of such a work, but the author will doubtless have his attention called to them, and will be able to make his second edition a still more inclusive manual of double-star astronomy, than even this first impression.

Through the kindness of Leverrier, M. Flammarion was allowed the use of one of the equatorials at the Observatory of Paris during the year 1877 for the re-measurement of a number of the double stars; these measures applying to about 130 objects are given at the end of the preface to this volume: amongst them we note the close pair of 40 Eridani, a rapidly revolving star which has not received the attention it deserves from observers.

M. Flammarion's work will doubtless soon find its way into the hands of every one who is interested in the double and multiple stars, and he will certainly experience the satisfaction of receiving the well-earned thanks of many amateurs who have no convenient access to large astronomical libraries and to whom his volume will be a valuable *vade-mecum*.

OUR BOOK SHELF

The Mollusca of the Firth of Clyde; being a Catalogue of Recent Marine Species Found in that Estuary. By Alfred Brown. (Glasgow: Hugh Hopkins, 1878.)

ALTHOUGH the recent mollusca of this district have during the last few years received a good deal of attention, especially from the labours of M'Andrew, Barlee, and Merle Norman, still the various memoirs detailing the results of these labours were only to be found widely scattered through a number of scientific periodicals, and Mr. Brown has in this neatly printed work given us not only a *résumé* of the labours of the naturalists we have referred to, but also of all those who have collected on the Firth of Clyde, and joined these to the labours of Mr. David Robertson and his own. The result is, so far as the testaceous mollusca go, a large and apparently accurate catalogue, which will show not only what has been done but also among the nudibranchs and cuttle-fish what is yet to be done. The notes under the heading of Habitat in this catalogue are often most interesting, giving details not only of the exact localities for the species, but notes also of their local names.

Wanderings in Patagonia; or, Life among the Ostrich-Hunters. By Julius Beerbohm. Map and Illustrations. (London: Chatto and Windus, 1879.)

THE title of this book is somewhat misleading, as the author's "Wanderings" were of a very limited extent, embracing only a small portion of the south-east coast region of Patagonia. Its most important feature is the account given of the life of the ostrich-hunters, and it adds little to our knowledge of Patagonia in addition to what has been told us by Musters and the one or two others who have really "explored" more or less of the wild region. The author's story is pleasant to read.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Tempel's Comet

THE well-known comet- and nebula-finder of the observatory of Arcetri, Tempel, has just made an observation of great interest in reference to his Comet No. II. of 1873, which, as astronomers know, has an orbit between Earth and Jupiter. It has no tail, but a nebular surrounding, which Tempel observed to be gradually diminishing in luminousness without losing bulk, and finally has entirely disappeared, leaving the comet perfectly distinct, but with a slight scintillation or rather an appearance of being composed of several masses having motion in the rest of the nucleus; probably an optical effect due to our own atmosphere, but which is at all events seen quite distinctly enough to make it certain that the disappearance of the nebulous surrounding is not due to failure of the telescope to show it.

The comet was last observed on December 18, at 6h. 53m. 12s. mean time of Arcetri in Right Ascension 23h. 3m. 14·15s., and in South Declination 19° 15′ 54″·8. It was seen on the 21st but briefly, and no observation could be made. Since then the continually cloudy sky has prevented it from being seen, but Tempel is confident of being able to see it through January. It is now amongst the asteroids.

W. J. STILLMAN

Florence, January 1

The Cosine Galvanometer

IN NATURE, vol. xix. p. 98, my name appears in a way that might lead the reader to infer that I was the inventor of the "cosine galvanometer." My knowledge of this useful instrument was derived from Prof. Trowbridge, of Cambridge, U.S., who described it in 1871 in the *American Journal of Science*, vol. cii. p. 118. In my "Physical Manipulation" I omitted to mention Prof. Trowbridge's name, supposing that his connection with the instrument was too well known to render it necessary.

EDWARD C. PICKERING

Harvard College Observatory, Cambridge, U.S., December 20, 1878

Force and Energy [1]

II.

IN passing it may be noticed that the plus sign thus deduced for a tensive force is otherwise convenient because tension results in a positive increase of the dimensions in the direction of the tension of the body through which the tension is transmitted.

[1] Continued from p. 196.

With regard now to the investigation of the equilibrium or the acceleration of momentum of a body founded on a knowledge of the forces acting upon it, that is, of the various rates of transference of momentum between it and other bodies through its surfaces, we must evidently give signs to the various surfaces of the body, surfaces which are parallel, and face opposite ways, being given opposite signs. If we multiply the transferences of momentum taking place through the different surfaces each by the sign of the surface through which it takes place, and add all these products together, the sum will be the acceleration of momentum of the body. Thus two equal tensive forces acting through parallel opposite faces would keep the body in balance. Or two numerically equal forces, the one tensive and the other compressive, acting through parallel surfaces facing the same way, would also keep the body in balance. If the phrase "intensity of force" be used to mean the force per unit area through any surface, so that it is simply a generalisation of the two common phrases "intensity of pressure" and "intensity of tension;" then if each small element of the surface of a body be given its proper sign and multiplied by the intensity of force through that small element, this force being also given its proper sign, and if all such products for the whole surface be summed up, the total will be the acceleration of momentum of the body. The direction of this acceleration will be shown by the sign of this total, the sign having reference to the relative position of the surfaces which have been arbitrarily called positive and negative. Thus let two tensions, *i.e.*, positive forces, be applied to two parallel opposite faces, and let the force applied to the positive face be greater than that applied to the negative face; then the body will suffer a positive acceleration of momentum; that is, an acceleration in the direction from the negative face towards the positive face. The faces perpendicular to the positive and negative faces must be given the signs $+\sqrt{-1}$ and $-\sqrt{-1}$.

Thus a pair of positive forces applied to faces with the signs $+1$ and $+\sqrt{-1}$ cannot possibly balance each other. But a positive force applied to a + face can be balanced by a tangential, or shearing, force applied to a $\sqrt{-1}$ face. Because the shearing force has either the sign $+\sqrt{-1}$ or $-\sqrt{-1}$, and multiplied by the sign of the face, gives either $+1$ or -1, as the sign of the product. Surfaces oblique to what is chosen as the positive direction must be considered as partly scalar and partly vector, as also forces oblique to the surfaces through which they act, or rather oblique to their direction of transmission. Oblique surfaces must be multiplied by oblique forces according to the ordinary rule of vector multiplication. This system of notation requires no further explanation, I think, to those who are likely to approve of it.

It has become lately a common habit to look upon those things which are conserved, that is, those which have an enduring existence, as objectively real; while those which may come into existence and go out of it again are considered as objectively unreal. Whether this is a correct philosophic habit or not, it has certainly tended to create suspicion as to the objective reality of all mechanical quantities. A gradually extending recognition of the relativity of these quantities is apt to lead on to a reluctant apprehension that all so-called physical facts are mere formal logical deductions from arbitrary definitions. The dark shadow of distrust first fell upon momentum because the fact that it is distinctly a relative quantity is most easily recognised, and thus became earlier a part and parcel of our familiar ideas. Then somebody suddenly recalled to mind the distinction, according to definition, between external and internal kinetic energy, and found that the external kinetic energy which it had been fondly hoped had some lingering flavour of the ABSOLUTE still clinging to it, was no more than a part of the internal kinetic energy of a larger group of bodies; and it became clear at a glance that energy, that grand ABSOLUTE REALITY which, being once borne into existence by triumphant modern science is now far too carefully conserved by its enthusiastic worshippers to allow of there being any risk of its dropping again out of existence, is just as purely relative in its nature as the velocity which has to be squared in order to calculate its amount. It had been thought that because a velocity has a direction and the square of a velocity has no direction, therefore we might calmly and fearlessly contemplate the total or partial destruction of momentum, steadfast in the assurance that energy would still live for us. And thus with much waiting in fluttering hope and trembling fear upon the brink of the Unseen Universe, and becoming impatient at the non-arrival of any clear intimations of immortality for ourselves, or for energy, or even for matter itself—which is clearly neither more real nor more unreal than her faithful spouse energy—a cloud of dismal despair seemed to be settling on the heads of the scientific nations, when a stern but cheering voice was heard from Munich bidding us be satisfied with our finite human faculty of perceiving relations only, and promising us that, if we would only not aspire to divine knowledge of the absolute, we might KNOW even now and also hereafter.

While admitting fully the relativity of all the physical facts which we may learn, I think it would be very unfortunate if we were to allow ourselves to confuse this with the idea that all mechanics is a mere phantasmagoria conjured up by a process of formal logical deduction from a basis of abitrary definitions. The clearest exponent of this theory of formality in mechanics that has come to my notice is Dr. V. A. Julius, in his letters on "Time" to NATURE, vol. xvi. pp. 122, 420. The argument may be thrown into the form of four short propositions and a conclusion, all of which are derived by purely formal reasoning from the ordinary definitions of the various quantities involved, and which a friend of mine pretends make out a "clear demonstration of the utter absurdity, futility, and falsity of all mechanics."

1. All motions and velocities are simply relative. Within a given isolated system, nothing with reference to the motions of its parts can be known beyond the motions of these parts relatively to the centre of inertia of the system.

2. Relatively to any other system, or single body, the velocity of the centre of inertia of this first system is, by definition, simply the mean of the velocities of its parts. The sum of the velocities of the parts relatively to the centre of inertia of this first system is, therefore, always zero.

3. Within one portion of this system, therefore, there cannot be any loss of average velocity without there being a simultaneous equal gain of average velocity in some other portion.

4. The changes which can possibly take place in the system with regard to velocity consist, therefore, in balanced exchanges of relative momentum between its parts, and, therefore, the equality of "action and reaction"—whether calculated with reference to rate of transference of momentum, or with reference to rate of transference of energy, *i.e.*, rate of doing work—is a purely formal deduction from the definition of the centre of inertia.

Conclusion. A purely formal deduction from an arbitrary definition is just as likely not to agree with reality as to agree with it. Q.E.D.

The fallacy of the argument lies in the artful omission of a few words in 3, which are necessary to make the meaning quite explicit. At the end of 2, the sum set equal to zero is that of the velocities *relatively to the centre of inertia of the system* itself. These, therefore, are the velocities referred to in 3. Therefore, in 4, the exchanges of momentum that are balanced are those of momentum measured relatively to the centre of inertia of the system itself; and it does not at all follow, by *pure logic*, that such a balanced exchange of momentum relatively to this centre does not produce an acceleration of velocity of the centre of inertia relatively to some body outside this system. Of course, if we add this outside body to the first system, then pure logic will compel the exchanges of momentum throughout this new combined system and measured relatively to the centre of inertia of the new combined system to balance. But pure logic does not *necessitate* the exchange of momentum within one part of the system relative to the centre of inertia of that part being unaccompanied by a simultaneous exchange of momentum between that part and some other part, or every other part. Thus the fact of conservation of momentum is not, that when two bodies exchange momentum, the amounts lost and gained measured relatively to the centre of inertia of the two, are numerically equal,—that would be a mere truism—but that the amounts lost and gained measured relatively to a third body are equal to each other. This latter is a physical fact, only to be proved by experiment, not by logic. The statement that action and reaction between two bodies are equal, does not mean anything in particular; but the statement that the action of a force between two bodies does not accelerate the velocity of their centre of inertia relatively to a third body is a statement of experimental fact. The mechanics of a system of two bodies might be built up by means of formal reasoning alone; but not that of a system of three, or of more, bodies without the experimental establishment of the law of conservation of momentum.

And the more complicated the system be, the larger the number of possible combinations of three bodies within it, the greater is the number of experiments or observations we can make to prove that the conservation of momentum is a general physical fact. The larger the number of such observations becomes, the further removed is the doctrine of the conservation of momentum from the character of a logical deduction from definitions.

Still, of course, the doctrine has only to do with relative velocities and relative accelerations of velocities. It loses, however, none of its reality and truthfulness on account of this. Why should not relations be capable of being real, even if not permanent? We are indeed incapable of conceiving anything as real which does not owe its reality in our conception simply to its relations to other things. If objective reality is in any way the opposite of relativity, then, certainly, so far as our knowledge goes, there is no such thing as objective reality. Our notions of momentum and of force, then, are relative to three bodies, and not to two bodies, and this seems to me to be an important point. The ELEMENTARY notion of momentum derived from DEFINITION is relative to TWO bodies only; but the PRACTICAL notion derived from EXPERIENCE is relative to three bodies at least, or to a complicated system of bodies. It should not be forgotten that the physical realities among which we live owe their existence to the complexity of nature. Throughout the complexity there are certain simple invariable relations, and these are the physical laws of nature. The law of conservation of momentum is this: the momentum of one system relative to another system remains unchanged by exchanges of momentum between the parts of the former system. Otherwise stated it is: exchanges of momentum may and do take place between the parts of a system without these exchanges being necessarily accompanied by an exchange of momentum between this system and any other system.

Energy is, of course, a quantity of as relative a character as momentum, although its relativity is not of just the same kind. Energy in general is usually defined as the power of doing work. Curiously enough this definition is frequently followed closely by the statement that a system may possess a very large amount of energy, and yet if there are no differences of potential within it no work can be done by it. The correct statement of what is meant by this last has often been given, viz., that in this case no work can be done by one part of the system upon another part of the same system. But still more often is the inaccuracy indulged in of saying that energy of one kind or another may be transformed into work. Now work is not energy and has no kind of similarity to energy, and therefore energy can never be converted into work. When energy is transferred from one body to another the first does work upon the second, the amount of work done being measured by the amount of energy transferred. The rate at which energy is transferred is the rate of doing work, or the horse-power. The doing of work or more shortly WORK, is the transference of energy from one body to another, but is not the energy itself. The confusion has never entered into the practical use of the word "work," which has always really been applied in the sense here explained, although very probably a good deal of confusion of ideas among both practical and theoretical men, may have been caused by the above noted incorrect statement that energy and work are convertible. The confusion is of the same sort as if we were to use the word force in the sense I have advocated and confuse it with acceleration of momentum. During some transferences of energy there is an invariable transformation of energy. If during the transference, the whole of the energy transferred is also simultaneously transformed, then the rate of doing work is also equal to the rate of transformation, and the amount of work done is numerically equal to the amount of energy transformed. But the phrase "work done" is only used when transference takes place. When a portion of one kind of energy in a body is converted into energy of another kind without any energy leaving the body, it is not the custom to say that work has been done. Work is only done by one body upon another, so that work is the TRANSFERENCE, not the TRANSFORMATION of energy. To say that so much energy has been spent in doing an equivalent amount of work is a convenient and quite allowable mode of saying that this amount of energy has been transferred from the working body without specifying what has become of the energy; that is, without specifying into what other body the energy has been transferred, and without specifying in what form the energy has appeared in the other body. But to say that the energy is converted into work is quite a different thing, and altogether wrong.

When a body possesses in two parts of it two quantities of heat at two different temperatures, the amount of work which the one part has the power of doing on the other in consequence of this difference of temperature is not nearly equal to the whole amount of heat energy in the two parts. Thus the energy in a body is not the power measured quantitatively, possessed by its parts of doing work on each other.

If in a collection of bodies there be a certain one body with a certain amount of kinetic energy, calculated from its velocity, relative to the centre of inertia of the group, that one body might deliver up the whole of this kinetic energy by direct impact upon another body which had zero velocity relative to that centre of inertia, provided these two bodies were exactly alike in certain particulars as to mass and shape. But if there did not exist in the group any body which had this particular relation of shape and velocity to the first, then this first could not possibly deliver up all its kinetic energy, so as to get its velocity relative to the centre of inertia of the whole group reduced to zero. It is thus clear that the internal kinetic energy of a collection of masses is not measured by the amount of kinetic energy calculated from the velocities relative to the centre of inertia of the collection that can be transferred from one part to another.

Also, if another body, or another group of bodies, existed apart from this first group, and possessed a velocity of centre of inertia either zero, or of any other value, relative to the centre of inertia of the first group, the kinetic energy of this first group, measured either relatively to its own centre of inertia, or to that of the other group, or to the centre of inertia of the two combined, could only be wholly transferred to this second group, provided that this second group had very special and very ingeniously contrived relations with regard to mass and configuration to the first group. Thus the kinetic energy of any collection is not measured by the power it may possibly have of doing work upon bodies outside the collection. And quite evidently the same may be said of any other kind of energy possessed by the body.

For each kind of energy we have more or less accurate means of comparing quantitatively different amounts of that kind of energy, and thus of measuring the amount of that kind of energy possessed by a body in terms of the quantity which is adopted as unit of that kind of energy. We have also means of converting different amounts of any one kind into most other kinds of energy; and since in several carefully-made experiments upon the conversion of different kinds of energy there has on the whole been a very fair agreement in the ratios furnished by these experiments between the adopted units of the different kinds, we have come to believe in the truth of the law of conservation of energy—the more especially since this belief is supported by theoretical reasoning based on the hypothesis of the truth of the conservation of momentum. This latter theoretical reasoning, however, we have, hitherto, at any rate succeeded in applying only to transferences of kinetic energy of visible motion, and to the thermodynamics of perfect gases.

But taking this principle of conservation of energy for granted as true, we have the means of measuring the amount of energy of any kind possessed by a body in terms of the adopted unit for kinetic energy of visible motion.

ROBERT H. SMITH

(*To be continued.*)

The Unseen Universe—Paradoxical Philosophy

WILL you permit me to ask through your columns how the idea of the authors—that the present universe is developed out of our unseen universe, which unseen universe is itself developed out of another, and so on in an endless vista up to the unconditioned—works when applied to the present universe as itself developing a lower universe?

The present universe must be a conditioning as well as a conditioned universe, or there would be a breach of the principle of continuity, and there must, on the same principle, be an endless vista of such lower universes.

Have we any hint of any lower universe? Ought we not to have more than a hint? Ought we not to be fully conscious that our own universe is developing and sustaining such a lower universe, to the living intelligent beings in which we are, in fact, supernatural agents, as the angels in the universe above us are to ourselves?

I think that the authors have expanded their idea in one direction only, and I have not seen any reviews of their books applying this idea in the other direction. If, however, this application has been made, I shall be glad to be referred to the passages containing it. W. A. T. HALLOWES

New University Club, St. James's Street, S.W. January 4

Atmospheric Electricity

THE traces afforded by the self-registering electrometer at this observatory show that the conditions of the atmospheric electricity at Kew were very similar during the recent frosts to those observed at Montsouris by M. Descroix. We have, however, in the automatic instrument the great advantage of continuous registration, and therefore our information is not limited to the results afforded by seven observations daily.

The whole period of the frost was characterised by extremely high tension which with us averaged and frequently exceeded the amount which sufficed to derange the French instrument.

The absolute maximum tension recorded equalled 600 volts, and occurred about 4 P.M. on December 16.

The most noticeable feature in the curves of electrical disturbance during the period is that of the daily range of the instrument having attained a maximum usually between 8 to 10 P.M., the tension reaching over 400 volts at the time on the 17th, 18th, and 21st, and over 500 on the 22nd ult.

The fall in tension on the 25th was irregular and the value became almost zero at 6 A.M. on the 26th, for the whole of which day it continued low. Negative electricity was recorded for the first time from 1 to 3 A.M. on the 29th.

Undoubtedly the value of the tension of the atmospheric electricity, as measured by the Thomson electrometer is, as M. Descroix states, only a relative one. We have determined experimentally that with the same instrument the indicated tension is largely influenced by the distance of the nozzle of the water-dropping collector from the wall of the building in which the instrument is placed, and in accordance with a suggestion of Sir W. Thomson, we replace during the passage of thunderstorms our ordinary discharge-tube by a very short one, so as to get the scale of tensions within the range of the electrometer.

Kew Observatory, January 6 G. M. WHIPPLE

Electrical Phenomenon

I HAVE just read in NATURE (vol. xix. p. 182) an account of a strange electrical phenomenon observed at Teignmouth. In connection with it the following incident may be of some interest:—When in Switzerland, not long since, I made with some friends the ascent of Monte Rosa. The weather was unsettled, and on gaining the summit we saw a thunderstorm advancing in our direction from the Italian valleys, and not wishing to turn ourselves into lightning-conductors we deemed it wise to retire from the summit. We had retreated a very short distance along the *arête* when the storm-clouds swept up upon us; the fine snow fell so thick that we could hardly see one another, and we were all suddenly attracted by a peculiar ticking or fizzing from our hair; when I held up my axe the ticking was most distinctly heard from the top of it. The thunder ceased, and we felt that we were acting as points, through which the ground electricity was flowing off into the cloud; if it had been dark, the bluish light observed at Teignmouth might have been visible.

As at Teignmouth, so on Monte Rosa; it was freezing hard when the phenomenon was observed. W. S. GREEN

Alta Terrace, Monkstown, Cork

Time and Longitude

As the questions I propounded under this head in NATURE, vol. xviii. p. 40, have been again alluded to by Mr. E. L. Layard, I may remark that they receive a complete answer in the "Geographical Reader," by C. B. Clarke, M.A. (Macmillan and Co., 1876). At p. 19 he says: "At the town of Sitka, in Alaska, half the population are Russians who have arrived from Russia across Asia; half the population are Americans who have arrived *viâ* the United States. Hence, when it is Sunday with the Russians it is Saturday with the Americans; the Russians are busy on Monday while the Americans are in church on Sunday to the great interruption of business."

It is evident, then, that our new year first commenced in Alaska at 9 A.M. Greenwich time on December 31. Each of our days commences at the same hour and lasts forty-eight hours; the year exists for 366 days. LATIMER CLARK

January 4

Magnetic Storm of May 14, 15

THE magnetic storm of May 14, 15, which was observed simultaneously in England, China, and Australia, and which made itself felt in the telegraph wires of Persia and India, was also perfectly observed in America. Mr. G. F. Kingston, director of the government observatory at Toronto, Canada, has kindly forwarded to me a tracing of his magnetograms, and I find that all the principal inflexions of the declination, as well as of the components of the intensity, bear a striking resemblance to those recorded at the Stonyhurst observatory. The correspondence between the two vertical force curves on May 14 is very remarkable for such distant stations. Comparing the times of the principal minimas in the V.F. trace, and of the chief maximum of the declination, we have the following results in Toronto mean time:—

	Principal V.F. min. P.M.		Secondary V.F. min. P.M.		Decl. Max. P.M.
Toronto Observatory ...	6 17	...	4 0	...	6 39
Stonyhurst Observatory	6 42	...	4 20	...	6 54
	0 25	...	0 20	...	0 15

The disturbing force would thus appear to have been felt somewhat earlier in Canada than in Europe.

The extent of the extreme oscillation of the V.F. magnets cannot be compared, as that at Stonyhurst was too sensitive, and was consequently thrown off its balance; but the rapid movement of the declination needle immediately preceding the maximum was almost identical in England and in Canada, the Stonyhurst curves showing a rise of 28′ 39″ in less than twenty minutes, and that of Toronto an increase of 26′ 53″ in the same time.

It is important to note that I have used the terms maximum and minimum in reference to increase and decrease of ordinate, but it so happens that an increase of ordinate signifies a decrease of H.F. and V.F., and also of W. declination in the Toronto curves, whilst it shows an increase of all these elements in the magnetograms of Stonyhurst. S. J. PERRY

Stonyhurst Observatory, December 28, 1878

Blowpipe Experiment

I BEG to inform you of the following curious results which may be considered of sufficient interest to lead to further investigation of the subject.

Having received a quantity of blowpipe charcoal from Freiberg, about two months ago, I placed two sticks in a "stoneware" jar full of pure water in order to saturate them therewith, so that small squares cut with a saw and placed on aluminium plate as a support, might stand the blowpipe heat longer. I also found that thus treated there is little or no black sawdust, which dirties the hands, &c., more than anything else in blowpipe operations.

Having also placed in the same jar of water two "aluminium spoons" (thick rods about five inches long), I was surprised to find that after the charcoal had sunk to the bottom on saturation, the aluminium rods were covered with semi-opaque roundish *crystals* (part being perfectly transparent) near the surface of the water, and also at the very bottom where the spoons rested on the jar.

Thinking the crystals might be due (although I could not tell how with such a deliquescent substance) to some phosphoric acid I had previously fused upon the aluminium spoons, I cleaned them thoroughly and placed them in fresh pure water with the charcoal about a fortnight ago, and they are again covered with the same kind of crystals. I now carefully scraped the crystals off the aluminium rods with a penknife and placed them on an agate slab, where, when dry, they had a perfectly white, sugary appearance, with some minute transparent fragments. Taking up some of these opaque white fragments upon a hot bead of boric acid, I submitted them to the action of the blowpipe, and found—

(*a*) That they at first emitted a slight yellow pyrochrome, so that they could not be due to *potash*.

(*b*) The green pyrochrome of the boric acid was unaltered (no *soda*).

(*c*) The substance floated in the bead in bluish-white, fat-like, amorphous fragments like alumina or opaque silica as *tabasheer*, but—

(*d*) On continued heating, the fragments gradually disappeared, leaving bubbles, until in half an hour, with fresh boric acid, there was simply a transparent bead left; exactly the behaviour of minute fragments of diamond in boric acid.

Silica is absolutely, and alumina nearly, insoluble in boric acid before the blowpipe.

One conclusion, therefore, seems this: that a slow solution of charcoal in the water takes place, and that crystals of carbon are deposited upon the aluminium.

London, December, 1878 W. A. Ross

Observations on the Microphone

WITH regard to an explanation of the action of the microphone I observed a fact which, though it was already known from some anterior experiments with strong galvanic currents, has not been remarked, as far as I know, with this instrument. On connecting the current from six Grove cells with the microphone (the telephone not being in the circuit) composed of the three carbon rods, the vertical one assumes a vibratory motion between its supports, which causes a very audible sound, especially when placed on a sounding-board.

I think this experiment may serve as another illustration of the well-known fact, discovered by Ampère, of the repulsive action between the subsequent parts of a rectilinear current. Most of the experimental proofs bearing on this point leave some doubt as to a true demonstration, because a dilatation from the heating effect of the very strong currents used with metallic bodies may interfere, and are considered, for example, to explain the experiments of Forbes and Gore.[1] But in my case, with a substance which has a very small coefficient of dilatation, I think the vibratory motion may be considered as an electrodynamic effect. As a supporting demonstration, I suspended with insulated metallic wires near another, three horizontal pieces of carbon (3 cm. long) in such a way that they could move freely away, and the two outer ones were connected with a battery of twenty Grove cells; *immediately* on closing the circuit a repulsion ensued between them and an oscillating motion set in, whilst bright sparks appeared between the contacts.

This experiment may throw some light on a recent controversy which has arisen between Mr. Varley and Prof. Hughes.[2] The latter insists on a change in contact resulting from alternating varying forms of the molecules or their spheres of action, in accordance with the sonorous vibrations. Mr. Varley points out a quite distinct cause. By using a contact-breaker moved with the hand he discovered, on applying a microscope with a 350 times magnifying power when the circuit was closed or opened, a grey cloud issuing between the nearest carbon-points. This seems to prove that little particles of carbon are loosened by an effect of trituration on the contact-surface, the cohesion being lessened by the heating effect of the current. This cloud of microscopic dust serves as a vehicle to the current (when the carbon piece is vibrating), and its resistance of course is easily modified by the impinging sound-vibrations. Now my experiment, though with a strong current, supports this fact, and shows that the vertical carbon is actually vibrating under its influence, and may prepare the above-mentioned condition, or at least render it very ready to change its contact in accordance with sound-waves acting on it with more or less force.

Perhaps it will be observed that a microphone acts very well in transmitting sound when even the weak current from a couple of Leclanché cells is used, but then, also, we may admit a propensity (through the influence of this current) of the vertical carbon rod to get into a vibratory condition, which the sonorous vibrations will easily actuate according to their own period, if really it is not already vibrating at microscopical distances.

Prof. Hughes alludes to au experiment which, as he thinks, gives an evident support to his theory. In a sealed glass tube are inclosed five loose pieces of carbon with terminals to admit a current. He remarks that, properly pressed mere mechanical shaking produces no variation of the current except that due to a constantly increasing resistance caused by abrasion of the carbon contacts, whilst under the influence of sonorous vibrations a varying current is produced, because the tube in this case is varying its length, and the molecules undergo proportional change of form. I think this proof may be as well, and with more probability explained by the facts mentioned above. The tube contains four or five *loose* pieces of carbon, and besides these some air, which, as it is in a closed space, will press from all sides on the carbon parcels when it is put in vibration by sound, and therefore alter in a mechanical way their distances, the more because the surfaces in contact are rough ones. In conclusion I will observe that the audible vibration of the vertical carbon rod in the microphone certainly elucidates the facts discovered by Blyth concerning sound transmitted only with coal cinders forming a receiving and a transmitting apparatus in a galvanic circuit, and on which he insists in a recent communication to this journal (NATURE, vol. xix. p. 72). L. BLEEKRODE

The Hague, December 8, 1878

[1] Forbes, *Phil. Mag.*, t. xvii. p. 358. Gore, *ibid.*, t. xv. p. 519.
[2] *Telegraphic Journal*, October 1 and 15, 1878.

Shakespeare's Colour-Names

IN the name of scientific accuracy and fair criticism I protest against Mr. Murphy's letter in NATURE, vol. xix. p. 197. His remarks proceed on the perfectly gratuitous assumption that *all* eagles have blue eyes. As this is not a fact (the only live ones I have examined had both of them green eyes), I have no hesitation in asserting that when Shakespeare wrote "An eagle, madam, hath not so green, so quick, so fine an eye as Paris hath," he did so, after having seen an eagle or eagles, and that when he said green he "evidently" meant green, and not blue.

Edinburgh, January 4 A. CRAIG-CHRISTIE

YOUR correspondent, Mr. J. J. Murphy, in his letter, NATURE, vol. xix. p. 197, overlooks the fact that *blue* is quite as inappropriate as *green* to describe the eye of an eagle. Shakespeare would never have used either epithet; the word he made use of was doubtless *keen*. Green has been substituted by the mistake of some transcriber of the play working by ear, and not by eye. I only wonder the correction has not been made long ago by some commentator. ROBERT BREWIN

Exeter, January 4

The Meteor Shower of January 2

AFTER a very heavy fall of rain, sleet, and then snow (equalling in the aggregate 1·472 inch), on the evening and night of January 1, the clouds partly cleared away on the ensuing morning, and during a watch of twenty minutes (6·14 to 6·34 A.M., January 2) in a sky fully two-thirds overcast, fourteen meteors were seen, all of them belonging to the special shower in Quadrans. This radiant was evidently very active at the time I saw it, and in a cloudless sky, must have supplied meteors at the rate of more than one per minute (for one observer). The paths were short and quick without streaks or trains. Radiant point at 230° + 51°, but not very exactly found owing to the clouds and haze through which several of the meteors were indistinctly seen. Three or four were as bright as 1st mag. stars. W. F. DENNING

Ashleydown, Bristol, January 2

OUR ASTRONOMICAL COLUMN

MISSING NEBULÆ.—In Mr. Ellery's Report, to which reference was made last week, it is stated that "two nebulæ, H 4223 and H 1561, widely separated from each other, and described by Herschel as prominent objects, cannot now be found, although careful search has been made for them." The first of these nebulæ is near the cluster Dunlop 413: in the "General Catalogue" it is called "a remarkable object," but being very large and faint, it might, perhaps, be suspected that its invisibility in the Melbourne reflector is owing to the same cause that has led to the Pleiades-nebula, and other similar diffused objects (as G. C. 132, 4570, 5051) being overlooked in very large telescopes though obvious in much smaller ones. But in the case of H 1561 no such supposition is admissible. It was observed by Sir John Herschel on five occasions, in sweeps made between December, 1834, and February, 1836; when best seen it was termed pretty bright, from 25″ to 35″ in diameter, gradually brighter towards the centre, and situate to the south of, though very near to, three stars of the eleventh

magnitude. Taking a mean of the five observations of position, and reducing to 1880, its R.A. is 7h. 35m. 8·0s. and N.P.D., 159° 0′ 46″.

We do not hear of recent observation of the nebula situated near ζ Tauri, which was discovered by Chacornac, and which was sufficiently bright from 1855, October 19, to 1856, January 27, to "cause surprise that it had not been inserted by Mr. Hind upon his ecliptic charts," though it was not perceived on the meridian even with a refractor of 25 centimetres from 1853, December 3, to 1854, December 17. In 1855–56 the nebula was projected upon a star of the eleventh magnitude, the position of which for 1880 is in R.A. 5h. 30m. 16·0s. and N.P.D. 68° 51′ 29″. Chacornac says: "Elle offrait une forme presque rectangulaire, dont le plus grand côte soutendait un angle de trois minutes et demie, et le plus petit un arc de deux minutes et demie." D'Arrest, 1863, September 82, could not perceive any nebulosity about the star, nor, 1165, January 25, "cœlo valde eximio." He notes that the star is double, estimated distance 40″. It precedes ζ Tauri 12·5s., and is north of it 4′ 28″.

Some years since the approximate places of three nebulosities remarked with the comet-seeker at Cambridge, U.S., by Mr. G. P. Bond, but subsequently missed, were published. One seen 1850, February 27, in R.A. 0h. 47m. 41s. ± 1m., N.P.D. 26° 36′ ± 19′, could not be found 1863, September 9. A faint and rather large nebula, seen 1850, December 30, was not to be found, 1863, August 17; R.A. 14h. 37m. ± 3m., N.P.D. 67° 0′ ± 30′, and a third nebulosity resembling a comet, observed 1850, November 7, in R.A. 23h. 50m. 46s., N.P.D. 123° 24′, requires verification; the place is for 1850.

GEOGRAPHICAL NOTES

UNDER the direction of the United States Hydrographic Office, Lieut.-Commander F. M. Green, U.S.N., and the officers under his command, have during the last four years been engaged in determining exactly secondary meridians of longitude by means of the submarine telegraph cables in the West Indies and South America. The result of the West India work in 1874, 1875, and 1876 was the determining the latitude and longitude of a large number of points in the West Indies with the utmost possible exactness; and during the past year this work, of the greatest value to geographical and geodetical science, has been continued by making a chain of telegraphic measurements from the Royal Observatory at Lisbon, by way of Madeira, St. Vincent, Pernambuco, Bahia, Rio de Janeiro, and Monte-Video to Buenos Ayres, there connecting with the observatories of Cordova and Santiago. This chain is perfect, with the exception of one link on the coast of Brazil, where the cable was broken, necessitating the procuring of new cable from England; but its completion will be effected before the computation of the observations already made can be finished. The method used for determining the latitude was in all cases that of the zenith telescope, brought to great perfection by the United States Engineers and the Coast Survey; that for differences of longitude, the comparison by repeated telegraphic signals of two chronometers at the ends of the telegraph cable, determining their errors both before and after the comparison by numerous transits of stars over the meridian. All that is needed to make the work of the last year perfect and complete is the telegraphic determination of the differences of longitude between the Greenwich and Lisbon observatories, and the completion of the imperfect link on the Brazilian coast, both of which will be done during the coming year. Until the observations have been carefully discussed, the results as compared with former determinations cannot be known exactly, but a preliminary computation indicates that the longitude of the coast of Brazil is laid down about three or three and a half miles too far west, this westerly error being indicated in a less degree in the longitudes of Madeira and St. Vincent.

LORD AUGUSTUS LOFTUS has recently forwarded to the Foreign Office, from St. Petersburg, a translation of a Russian letter from Cabul, descriptive of the journey of General Stoletoff's mission from Samarcand, which supplies some notes of interest respecting the country traversed. The road selected for reaching the Oxus was through Huzar, Shirabad, and Chushkogosar, which was traversed in five days. On this route the mission passed through the famous defile known in ancient times under the name of the "Iron Gates," and now called Burghasse Khana. The mission crossed the Oxus in very primitive boats, and marching by night, passed over a sandy arid steppe, and next morning reached Kurshiak settlement, situated in a cultivated country. They made three stages before reaching Mizar and Sheriff, where great crowds thronged the streets, and gazed with curiosity on the people from the distant north. After leaving Tashurgan, the party reached the spurs of the Hindu Kush, and journeyed to Cabul during twenty days. Ascending at first in gentle slopes, the Hindu Kush gradually rises higher and higher, forming, amidst its frequent passes, terraces of increasing height. After traversing a series of such terraces, the mission reached the elevated Bamian Valley (8,500 feet), near which are the Kalu and Great Tran Passes (13,000 feet). Passing the famous Bamian idols, chiselled on the face of the rock, they emerged from the last-named pass, and then descended from the Ugly Pass into the Cabul Darya Valley, at a place three days' journey from the capital of Afghanistan.

THE French papers published last week news from the Gaboon settlements stating that the Ogowe exploring mission had arrived in Libreville, the head city of the colony. A telegram read at the last sitting of the Paris Geographical Society announced that M. Brazza, the chief of the mission, had arrived in Lisbon with some of his subordinates, on his way to Paris. It was expected he would arrive in time for the meeting of the Society on Tuesday. The exploration of the mission has lasted three years, and many highly important results are said to have been obtained.

AT the last sitting of the Paris Geographical Society M. de Lesseps read a telegram received from Capt. Roudaire, stating that he had found nothing but compressed sand when boring to a depth of 30 metres in the Gabes Isthmus, so that no real difficulty prevented the opening of it for the intended Saharan Sea.

ON the authority of Mr. Oscar Dickson, it is stated that the Nordenskjöld expedition is wintering forty miles north of Cape East in Behring Straits. This news has been given to American whalers by a party of trustworthy natives, and a number of whalers are said to be wintering with the *Vega*.

THE just issued October *Bulletin* of the Paris Geographical Society contains a long paper by M. Léon Rousset, giving the results of a journey in the upper basin of the Yellow River and the region of the loess which overspreads so large an area of China, and of which Richthofen makes so much in his great work on China. M. Dutreuil de Rhins contributes a very useful account of Annam and the province of Hué; M. H. Harrisse discusses the question of the burial-place of Columbus, and M. d'Abbadie concludes his useful description of the instruments to be used in travel.

A LITTLE work on Afghanistan has just been published by Dr. Josef Chavanne, the author of the excellent work on the Sahara. It is written with special reference to the

present war, and gives a detailed description of the country and its geographical character, as well as of its inhabitants, from an ethnographical as well as a social point of view. The natural resources and military power of Afghanistan are described, and particular attention has been paid to an account of the communication between India and Afghanistan, the lines of operation, and the numerous mountain passes. There are several illustrations and an excellent map. It is published by Hartleben, of Vienna.

IN a recent voyage from Melbourne to the Fiji Islands, the steamer *Ariel* called at Lord Howe Island, where twenty-five people in all were found. The island is mountainous, of volcanic origin, but well-wooded, about five miles long, and from one and a half to two miles broad, and is situated some 400 miles east of Sydney. The communication of the inhabitants with the outer world is nowadays very uncertain, as whalers but rarely visit them.

THE MUSICAL ASSOCIATION[1]

THE question, In what way does science enter into the subject of music? is one that by no means admits of an easy answer. If we were to put it to various persons interested in music in different ways we should find their opinions most vague and contradictory. A university scholar, or a physical lecturer, would make the science of music consist entirely in the doctrines of acoustics; while, on the other hand, we should find some of the most eminent musical professors telling us that these had nothing to do with music at all, but that science meant the study and application of the rules of musical composition. Or possibly it might even be held that a skilful manipulation of the violin, or an appropriate management of the voice in singing, or an intelligent phrasing of pianoforte passages, or other refinements of execution, constituted all the science that musicians need aspire to.

A quarter of a century ago such a question would have excited no interest. People in general were satisfied to take the art as they practically found it, and troubled themselves but little as to the principles on which it was based. But the march of knowledge has changed the aspect of the matter. Modern philosophical investigation has included music in the universality of its aims, and the musician, however conservative, must submit to a searching inquiry as to the real nature of the stuff in which he deals.

The great work of Helmholtz, published in 1863, gave the first real stimulus to scientific musical inquiry; and although many years passed before it became much known in this country it at length aroused attention, and some of the most intelligent students of the art began to see that there was really something to be inquired into—the first step towards accurate knowledge of any kind. They observed the beneficial operation of the learned societies, where papers on the subjects they embraced were brought forward; and the idea occurred to them that an association of a similar character for music would not only enable the scientific questions connected with it to be publicly discussed, but might also be made conducive to the welfare of the art in a practical point of view. The idea was mentioned to one of the most eminent men of science (now president of the Royal Society), who, warmly approving it, issued the following circular:—

"50, *Grosvenor Place, April* 8, 1874

"DEAR SIR,—It has been suggested by several leading persons interested both in the theory and practice of music, that the formation of a society similar in the main features of its organisation to existing learned societies would be a great public benefit. Such a musical society might comprise among its members the foremost musicians, theoretical as well as practical, of the day, the principal patrons of art, and also those scientific men whose researches have been directed to the science of acoustics and to kindred inquiries. Its periodical meetings might be devoted partly to the reading of papers upon the history, the principles, and the criticism of music, partly to the illustration of such papers by actual performance, and partly to the exhibition and discussion of experiments relating to theory and construction of musical instruments, or to the principles and combinations of musical sounds.

"With a view to ascertain the opinions of persons interested in these subjects, and to attempt a more precise definition of the objects and constitution of such a society, it is proposed to hold a meeting here, at which your presence is requested on Thursday, April 16, at 2.30 P.M.

"I am, dear sir, yours faithfully,

"(Signed) W. SPOTTISWOODE"

This led to the formation of the Association whose proceedings are mentioned at the head of this article. The rules were judiciously framed, so as to avoid the rocks on which former musical societies had been shipwrecked; and the society has now gone successfully through four sessions. We learn from the report just issued, at the commencement of the fifth year, that the finances are prosperous, that the meetings are well attended, that the officers are zealous and efficient, and that a series of good papers are forthcoming for the future; from all which it may be fairly inferred that the institution has taken a permanent position.

The character of the society is, of course, best displayed by the contents of its *Transactions*. We cannot pretend to review the thirty-six papers (some of them very elaborate) contained in the four volumes before us; it will be an easier course to indicate briefly, in the first instance, what are the "subjects connected with the art and science of music" which more especially deserve "investigation and discussion," and then to see how far the papers actually presented to the Association have fulfilled the object aimed at in its title.

Giving precedence to science, one may conceive that the "Principles and Phenomena of Acoustics" would claim attention. It is true, as has already been hinted, that some eminent practical musicians repudiate the relevancy of these inquiries, and discourage their study, on the ground that a knowledge of acoustics is unnecessary to the practical musician, whether composer or performer.[2]

But fortunately the general spread of education sufficiently disposes of arguments of this kind. There are, and no doubt always will be, persons who are satisfied with the minimum amount of knowledge to enable them to earn their daily bread, but it is to be hoped the number is decreasing every day. A man who lives by an art will, if his mind be properly constituted, be in no wise reluctant to learn all he can about it, even though the knowledge may not be immediately convertible into money. Musicians must, in spite of the disparaging opinion of some of their leaders, be treated as intelligent beings, who have minds capable of enlightenment and instruction, and surely there is nothing unreasonable in assuming that the philosophical principles on which their art depends must present some interest to them, if laid before them in an intelligible form. The doctrine that such knowledge should be confined to cultivated amateurs, and forbidden to professional musicians, is simply a libel on the intelligence of those to whom we owe enjoyment of so high an order. If, then, these principles are to be studied, the science of acoustics must necessarily form the basis of the study. The splendid

[1] Proceedings of the Musical Association for the Investigation and Discussion of Subjects connected with the Art and Science of Music. Vols. i. to iv. First Session, 1874-5; Second Session, 1875-6; Third Session, 1876-7; Fourth Session, 1877-8.

[2] It is a remarkable example of this view that in a new elaborate and voluminous English "Dictionary of Music," now in course of publication, the word *Acoustics* finds no place.

investigations of Helmholtz as to the nature of musical sounds and musical sensations form a fund of knowledge of the most interesting and instructive kind, and illustration and discussion of such topics would be by no means out of place before the society. We believe that the great fundamental fact of the compound nature of musical sounds, which now has become as firmly established as any physical fact can be, is hardly yet understood, or its great significance appreciated by the great mass of the persons who have to do with its effects every day of their lives.

It happens, however (no doubt for good and sufficient reasons), that the more abstract principles of acoustics have received but little attention in the society. We only notice three papers which come within this category, and these on quite subsidiary points, namely, "On our Perception of the Direction of a Source of Sound," by Lord Rayleigh; "On the Sensitiveness of the Ear to Pitch and Change of Pitch," by Mr. A. J. Ellis; and "On the Musical Inventions and Discoveries of the late Sir C. Wheatstone," by Prof. W. G. Adams.

But the science of acoustics is a very different thing from the theory of music. There is much misunderstanding on this point; many people confuse the two, whereas the former is in reality only the introduction to the latter. A student may be well acquainted with all the scientific facts and theories relating to the production and transmission of musical sounds, and yet know nothing of the mode in which these data bear on music itself. Helmholtz, who, with wonderful knowledge and sagacity, appears to have anticipated almost every possible view of the subject, has fully expressed this distinction not only in the substance of his great work, but in its very title-page. He calls it "Die Lehre von den Tonempfindungen *als physiologische Grundlage* für die Theorie der Musik," thereby declaring that the acoustical doctrines he so admirably lays down are not to be considered as forming of themselves a theory of music, but are merely intended to *serve as a basis* for such a theory. Starting from these data, it becomes necessary to consider the influence they have on the varied and complicated forms and rules which guide the structure of musical composition, as, for example, the construction of the ordinary scale, the nature of chromatic notes, tonality, the combinations and progressions of harmony, the rules of melodial counterpoint, musical form, and so on. A crowd of most interesting questions arise as to how far all these practical matters have been influenced by the physical properties of musical sounds, or how far they are the result of free artistic invention. Helmholtz devotes the second part of his work to the discussion of these and kindred questions, on which, aided by a competent knowledge of music, his great reasoning powers have enabled him to throw much new light. But this part of his labours has been hitherto almost a sealed book to musicians; it is difficult, often elaborate, and sometimes obscure, and the interpreters who have so ably popularised his acoustical researches have stopped short before venturing on what was to physicists a less familiar region. Yet this is by far the most important section of the work, from a musical point of view; it is, in fact, the real "Theory of Music," the true musical philosophy, in which the proper application of science to music is to be found; moreover, unlike abstract acoustics, it touches closely on the practice of the musical art, and the habits of thought of its professors. There are few teachers of musical composition who do not to some extent attempt to found their instruction on natural principles, or what they think to be such; but the theories thus propounded are for the most part crude, vague, and founded on merely empirical fancies, having no philosophical origin, and such as will not stand the test of scientific investigation or strict logical reasoning; and hence we can hardly wonder at the fact that they rather obstruct than aid the efficiency of musical instruction.

This subject, therefore, the "Application of Scientific Data and Scientific Reasoning to the Theory of Music," is one which offers every inducement for the higher order of musical study, and its discussion is eminently in place in such a society as that before us. The results of the modern investigations are so new, and in many respects so antagonistic to the ideas hitherto prevailing among musicians, that it is not to be expected they will be at once fully understood or favourably received. Already a considerable amount of opposition has been manifested to them; it is reasonable and proper that they should be fairly considered, and it is in the highest degree desirable that they should be clearly explained. The subject has not been neglected at the meetings of the Association, for, although no systematic treatment of it has yet been attempted, we find no less than eight papers on various points of theoretical detail. Four of these are on intonation and temperament (a favourite theme with musical mathematicians, but somewhat unpalatable to practical men, who consider the out-of-tune equal division of the octave "good enough for them"); a fifth aims at exposing the fallacies and inconsistencies of certain of the old theoretical systems; another treats of the philosophical nature of intervals and of the construction of the scale; another expounds some elementary views on harmony; and the eighth exhibits various numerical calculations on musical ratios, &c.

Another point that furnishes a most profitable topic of study is *musical history*. It is impossible to look far into music without becoming aware how largely modern form and structure are derived from what has gone before, and the careful examination of this clears up many points of theory for which no other sufficient explanation can be found. Nothing could be more in place for a "musical association" than historical papers, not as mere matters of antiquarian curiosity, but as bearing on the various changes of musical form. We only, however, find two historical papers, one, an instructive essay, by Sir Frederick Ouseley, on the "History of Ecclesiastical Music in Western Europe," the other an interesting monograph, by Mr. Cummings, on "Purcell."

The *construction of musical instruments* offers a large and varied source of interest, combining the laws of acoustics, the application of mechanical skill and invention, and the adaptation to practical musical use. There are six papers on this, relating to stringed and brass instruments, drums, and the voice.

Finally, there are abundance of topics connected with *the practice of the musical art* which admit of discussion in such a society; for although, in a scientific journal, it is our chief province to point to the subjects in which science takes part, yet it would be a misuse of the society to let these predominate to the prejudice of the more practical matters which come home more directly to professional men, and we consider it a good evidence of the flourishing condition and prospects of the society that these practical points have received so large a share of attention. By far the larger number of the papers have been of this practical kind, relating to musical notations and nomenclature, criticism, practical standards of pitch, the analysis of great musical works, pianoforte playing, the cultivation of sacred music, the connection of music with language, the laws of expression, modes of tuition, and musical libraries. A paper on the last-named subject led to a memorial to the British Museum, and elicited an answer explaining the facilities which that institution affords for musical reference and study.

The Association deserves the support and co-operation of every one interested in the cultivation of music either theoretically or practically, and we cordially wish it the permanent success it seems in a fair way to attain.

W. POLE

DISCUSSION OF THE WORKING HYPOTHESIS THAT THE SO-CALLED ELEMENTS ARE COMPOUND BODIES[1]

II.

Application of the above Views to Calcium, Iron, Lithium, and Hydrogen

Calcium

IT was in a communication to the Royal Society made now some time ago (*Proc.*, vol. xxii. p. 380, 1874), that I first referred to the possibility that the well-known line-spectra of the elementary bodies might not result from the vibration of similar molecules. I was led to make the remark in consequence of the differences to which I have already drawn attention in the spectra of certain elements as observed in the spectrum of the sun and in those obtained with the ordinary instrumental appliances.

Later (*Proc.* Roy. Soc., No. 168, 1876) I produced evidence that the molecular grouping of calcium which, with a small induction-coil and small jar, gives a spectrum with its chief line in the blue, is nearly broken up in the sun, and quite broken up in the discharge from a large coil and jar, into another or others with lines in the violet.

I said "another," or "others," because I was not then able to determine whether the last-named lines proceeded from the same or different molecules; and I added that it was possible we might have to wait for photographs of the spectra of the brighter stars before this point could be determined.

I also remarked that this result enabled us to fix with very considerable accuracy the electric dissociating conditions which are equivalent to that degree of dissociation at present at work in the sun.

In Fig. 3 I have collected several spectra copied from photographs in order that the line of argument may be grasped.

First we see what happens to the non-dissociated and the dissociated chloride. Next we have the lines with a weak voltaic arc, the single line to the right (W L

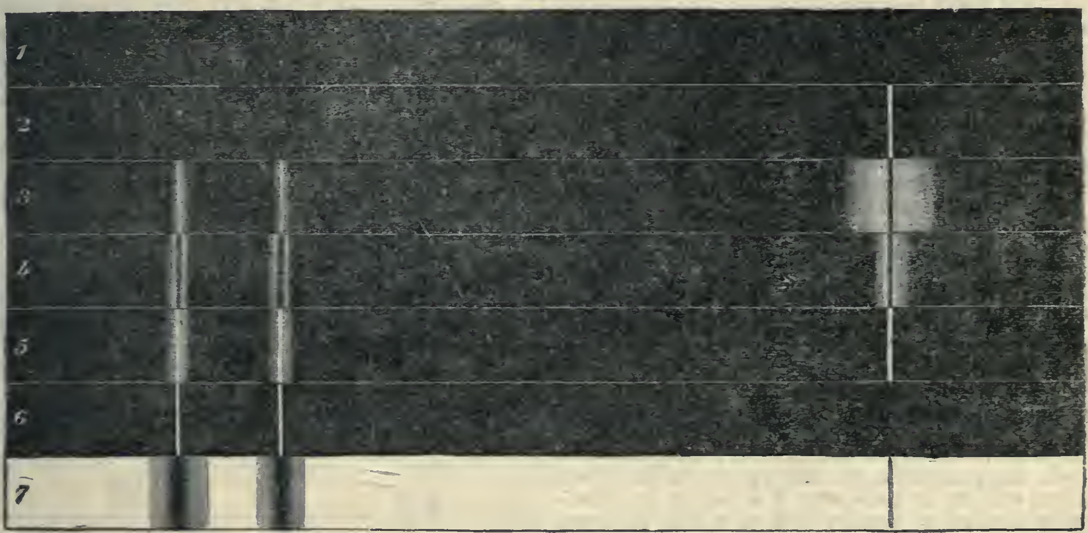


FIG. 3.—The blue end of the spectrum of calcium under different conditions. 1. Calcium is combined with chlorine ($CaCl_2$). When the temperature is low, the compound molecule vibrates as a whole, the spectrum is at the red end, and no lines of calcium are seen. 2. The line of the metal seen when the compound molecule is dissociated to a slight extent with an induced current. 3. The spectrum of metallic calcium in the electric arc with a small number of cells. 4. The same when the number of cells is increased 5. The spectrum when a coil and small jar are employed. 6. The spectrum when a large coil and large jar are used. 7. The absorption of the calcium vapour in the Sun.

4226·3) is much thicker than the two lines (W L 3933 and 3968) to the left, and reverses itself.

We have next calcium exposed to a current of higher tension. It will be seen that here the three lines are almost equally thick, and all reverse themselves.

Now it will be recollected, that in the case of known compounds the band structure of the true compounds is reduced as dissociation works its way, and the spectrum of each constituent element makes its appearance. If in 3 we take the wide line as representing the banded spectrum of the compound, and the thinner ones as representing the longest elemental lines making their appearance as the result of partial dissociation, we have, by hypothesis, an element behaving like a compound.

If the hypothesis be true, we ought to be able not only to obtain, with lower temperatures, a still greater preponderance of the single line, *as we do;* but with higher temperatures, a still greater preponderance of the double ones, *as we do.*

I tested this in the following manner:—Employing photography, because the visibility of the more refrangible lines is small, and because a permanent record of an experiment, free as it must be from all bias, is a very precious thing.

Induced currents of electricity were employed in order that all the photographic results might be comparable.

To represent the lowest temperature I used a small induction coil and a Leyden jar only just large enough to secure the requisite amount of photographic effect. To represent the highest, I used the largest coil and jar at my disposal. The spark was then taken between two aluminium electrodes, the lower one cup-shaped, and charged with a salt of calcium.

In the figure I give exact copies of the results obtained. It will be seen that with the lowest temperature only the single line (2) and with the highest temperature only the two more refrangible lines (6) are recorded on the plate.

This proved that the intensity of the vibrations was quite changed in the two experiments.

Perhaps it may not be superfluous here to state the reasons which induced me to search for further evidence in the stars.

[1] Paper read at the Royal Society, December 12, 1878, by J. Norman Lockyer, F.R.S. Continued from p. 201.

It is abundantly clear that if the so-called elements, or more properly speaking their finest atoms—those that give us line spectra—are really compounds, the compounds must have been formed at a very high temperature. It is easy to imagine that there may be no superior limit to temperature, and therefore no superior limit beyond which such combinations are possible, because the atoms which have the power of combining together at these transcendental stages of heat do not exist as such, or rather they exist combined with other atoms, like or unlike, at all lower temperatures. Hence association will be a combination of more complex molecules as temperature is reduced, and of dissociation, therefore, with increased temperature there may be no end.

That is the first point.

The second is this :—

We are justified in supposing that our "calcium," once formed, is a distinct entity, whether it be an element or not, and therefore, by working at it alone, we should never know whether the temperature produces a single simpler form or more atomic condition of the same thing, or whether we actually break it up into X + Y, because neither X nor Y will ever vary.

But if calcium be a product of a condition of relatively lower temperature, then in the stars, hot enough to enable its constituents to exist uncompounded, we may expect these constituents to vary in quantity; there may be more of X in one star and more of Y in another; and if this be so, then the H and K lines will vary in thickness, and the extremest limit of variation will be that we shall only have H representing, say X in one star, and only have K representing, say Y in another. Intermediately between these extreme conditions we may have cases in which, though both H and K are visible, H is thicker in some and K is thicker in others.

Prof. Stokes was good enough to add largely to the value of my paper as it appeared in the *Proceedings* by appending a note pointing out that "When a solid body such as a platinum wire, traversed by a voltaic current, is heated to incandescence, we know that as the temperature increases not only does the radiation of each particular refrangibility absolutely increase, but the proportion of the radiations of the different refrangibilities is changed, the proportion of the higher to the lower increasing with the temperature. It would be in accordance with analogy to suppose that as a rule the same would take place in an incandescent surface, though in this case the spectrum would be discontinuous instead of continuous. Thus if A, B, C, D, E denote conspicuous bright lines of increasing refrangibility in the spectrum of the vapour, it might very well be that at a comparatively low temperature A should be the brightest and the most persistent; at a higher temperature, while all were brighter than before, the relative brightness might be changed, and C might be the brightest and the most persistent, and at a still higher temperature E."

On these grounds Prof. Stokes, while he regarded the facts I mentioned as evidence of the high temperature of the sun, did not look upon them as *conclusive* evidence of the dissociation of the molecule of calcium.

Since that paper was sent in, however, the appeal to the stars to which I referred in it has been made, and made with the most admirable results, by Dr. Huggins.

The result of that appeal is that the line which, according to Prof. Stokes' view, should have prevailed over all others, as Sirius is acknowledged to be a hotter star than our sun, is that, if it exists at all in the spectrum, it is so faint that it was not recognised by Dr. Huggins in the first instance.

In Sirius, indeed, the H line due to one molecular grouping of calcium is as thick as are the hydrogen lines as mapped by Secchi, while the K line, due to another molecular grouping, which is equally thick in the spectrum of the sun, has not yet made its appearance.

In the sun, where it is as thick as H, the hydrogen lines have vastly thinned.

While this paper has been in preparation, Dr. Huggins has been good enough to communicate to me the results of his most important observations, and I have also had an opportunity of inspecting several of the photographs which he has recently taken. The result of the recent work has been to show that H and *h* are of about the same breadth in Sirius. In α Aquilæ while the relation of H to *h* is not greatly changed, a distinct approach to the solar condition is observed, K being now unmistakably present, although its breadth is small as compared with that of H. I must express my obligations to Dr. Huggins for granting me permission to enrich my paper by reference to these unpublished observations. His letter, which I have permission to quote, is as follows :—

"It may be gratifying to you to learn that in a photograph I have recently taken of the spectrum of α Aquilæ there is a line corresponding to the more refrangible of the solar H lines [that is K], but about half the breadth of the line corresponding to the first H lines.

"In the spectra of α Lyræ and Sirius the second line is absent."

Prof. Young's observations of the chromospheric lines, to which I shall afterwards refer, give important evidence regarding the presence of calcium in the chromosphere of the sun. He finds that the H and K lines of calcium are strongly reversed in every important spot, and that in solar storms H has been observed injected into the chromosphere seventy-five times, and K fifty times, while the blue line at W. L. 4226·3, the all-important line at the arc-temperature, was only injected thrice.

Further, in the eclipse observed in Siam in 1875, the H and K lines left the strongest record in the spectrum of the chromosphere, while the line near G in a photographic region of much greater intensity was not recorded at all. In the American eclipse of the present year the H and K lines of calcium were distinctly visible at the base of the corona, in which for the first time the observers could scarcely trace the existence of any hydrogen.

To sum up, then, the facts regarding calcium, we have first of all the H-line differentiated from the others

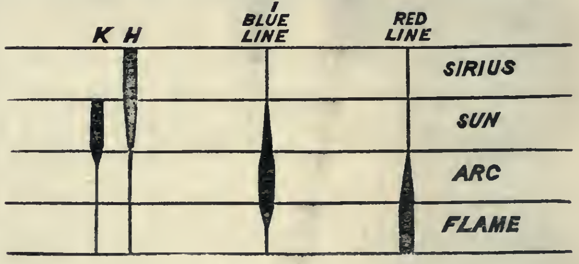


Fig. 4.—The Molecular Groupings of Calcium.

by its almost solitary existence in Sirius. We have the K-line differentiated from the rest by its birth, so to speak, in α Aquilæ, and the thickness of its line in the sun, as compared to that in the arc. We have the blue line differentiated from H and K by its thinness in the solar spectrum while they are thick, and by its thickness in the arc while they are thin. We have it again differentiated from them by its absence in solar storms in which they are almost universally seen, and finally, by its absence during eclipses, while the H and K lines have been the brightest seen or photographed. Last stage of all, we have calcium, distinguished from its salts by the fact that the blue line is only visible when a high temperature is employed, each salt having a definite spectrum of its own, in which none of the lines to which I have drawn attention appear, so long as the temperature is kept below a certain point.

Iron

With regard to the iron spectrum I shall limit my remarks to that portion of it visible on my photographic plates between H and G. It may be described as a very complicated spectrum so far as the number of lines is concerned in comparison with such bodies as sodium and potassium, lead, thallium, and the like, but unlike them again it contains no one line which is clearly and unmistakably reversed on all occasions. Compared, however, with the spectrum of such bodies as cerium and uranium the spectrum is simplicity itself.

Now among these lines are two triplets, two sets of three lines each, giving us beautiful examples of those repetitions of structure in the spectrum which we meet with in the spectra of almost all bodies, some of which have already been pointed out by Mascart, Cornu, and myself. Now the facts indicate that these two triplets are not due to the vibration of the same molecular grouping which gives rise to most of the other lines. They are as follows. In many photographs in which iron has been compared with other bodies, and in others again in which iron has been photographed as existing in different degrees of impurity in other bodies, these triplets have been seen almost alone, and the relative intensity of them, as compared with the few remaining lines, is greatly changed. In this these photographs resemble one I took three years ago, in which a large coil and jar were employed instead of the arc, which necessitated an exposure of an hour instead of two minutes. In this the triplet near G is very marked, the two adjacent lines more refrangible near it, which are seen nearly as strong as the triplet itself in some of the arc photographs I possess, are only very faintly visible, while dimmer still are seen the lines of the triplet between H and *h*.

There is another series of facts in another line of work. In solar storms, as is well known, the iron lines sometimes make their appearance in the chromosphere. Now, if we were dealing here with one molecular grouping, we should expect the lines to make their appearance in the order of their lengths, and we should expect the shortest lines to occur less frequently than the longest ones. Now, precisely the opposite is the fact. One of the most valuable contributions to solar physics that we possess is the memoir in which Prof. C. A. Young records his observation of the chromospheric lines, made on behalf of the United States Government, at Sherman, in the Rocky Mountains. The glorious climate and pure air of this region, to which I can personally testify, enabled him to record phenomena which it is hopeless to expect to see under less favourable conditions. Among these were injections of iron vapour into the chromosphere, the record taking the form of the number of times any one line was seen during the whole period of observation.

Now two very faint and short lines close to the triplet near G were observed to be injected thirty times, while one of the lines of the triplet was only injected twice.

The question next arises, Are the triplets produced by one molecular grouping or by two? This question I also think the facts help us to answer. I will first state by way of reminder that in the spark photograph the more refrangible triplet is barely visible, while the one near G is very strong. Now if one molecular grouping alone were in question this relative intensity would always be preserved however much the absolute intensity of the compound system might vary, but if it is a question of two molecules we might expect that in some of the regions open to our observation we should get evidence of cases in which the relative intensity is reversed or the two intensities are assimilated. What might happen does happen; the relative intensity of the two triplets in the spark photograph is grandly reversed in the spectrum of the sun. The lines barely visible in the spark photograph are among the most prominent in the solar spectrum, while the triplet which is strong in that photograph is represented by Fraunhofer lines not half so thick. Indeed, while the hypothesis that the iron lines in the region I have indicated are produced by the vibration of one molecule does not include all the facts, the hypothesis that the vibrations are produced by at least three distinct molecules includes all the phenomena in a most satisfactory manner.

Lithium

Before the maps of the long and short lines of some of the chemical elements compared with the solar spectra, which were published in the *Phil. Trans.* for 1873, "Plate IX.," were communicated to the Society, I very carefully tested the work of prior observers on the non-coincidence of the red and orange lines of that metal with the Fraunhofer lines, and found that neither of them were strongly if at all represented in the sun, and this remark also applies to a line in the blue at wave-length 4,603.

The photographic lithium line, however, in the violet, has a strong representative among the Fraunhofer lines.

Applying, therefore, the previous method of stating the facts, the presence of this line in the sun differentiates it from all the others. For the differentiation of the red and yellow lines I need only refer to Bunsen's spectral analytical researches, which were translated in the *Phil. Mag.*, December, 1875.

In Plate IV. two spectra of the chloride of lithium are given, one of them showing the red line strong and the yellow one feeble, the other showing merely a trace of the red line, while the intensity of the yellow one is much increased, and a line in the blue is indicated. Another notice of the blue line of lithium occurs in a discourse by Prof. Tyndall, reprinted in the *Chemical News*, and a letter of Dr. Frankland's to Prof. Tyndall, dated November 7, 1861. This letter is so important for my argument, that I reprint it entire from the *Philosophical Magazine*, vol. xxii. p. 472:—

"On throwing the spectrum of lithium on the screen yesterday, I was surprised to see a magnificent blue band. At first I thought the lithic chloride must be adulterated with strontium, but on testing it with Steinheil's apparatus it yielded normal results without any trace of a blue band. I am just now reading the report of your discourse in the *Chemical News*, and I find that you have noticed the same thing. Whence does this blue line arise? Does it really belong to the lithium, or are the carbon points or ignited air guilty of its production? I find there blue bands with common salt, but they have neither the definiteness nor the brilliancy of the lithium band. When lithium wire burns in air it emits a somewhat crimson light; plunge it into oxygen, and the light changes to bluish white. This seems to indicate that a high temperature is necessary to bring out the blue ray."

"POSTSCRIPT, Nov. 22, 1861.—I have just made some further experiments on the lithium spectrum, and they conclusively prove that the appearance of the blue line depends entirely on the temperature. The spectrum of lithic chloride, ignited in a Bunsen's burner flame, does not disclose the faintest trace of the blue line; replace the Bunsen's burner by a jet of hydrogen (the temperature of which is higher than that of the Bunsen's burner) and the blue line appears, faint, it is true, but sharp and quite unmistakable. If oxygen now be slowly turned into the jet, the brilliancy of the blue line increases until the temperature of the flame rises high enough to fuse the platinum, and thus put an end to the experiment."

These observations of Profs. Tyndall and Frankland differentiate this blue line from those which are observed at low temperatures. The line in the violet to which I have already referred, is again differentiated from all the rest by the fact that it is the only line in the spectrum of

the sun which is strongly reversed, so far as our present knowledge extends. The various forms of lithium, therefore, may be shown in the following manner.

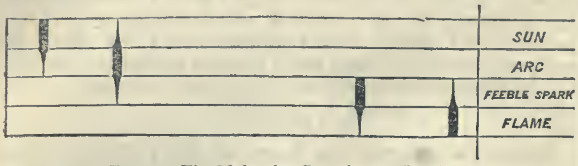


FIG. 5.—The Molecular Groupings of Lithium.

It is remarkable that in the case of this body which at relatively low temperature goes through its changes, its compounds are broken up at the temperature of the Bunsen burner. The spectrum, *e.g.* of the chloride, so far as I know, has never been seen.

Hydrogen

All the phenomena of variability and inversion in the order of intensity presented to us in the case of calcium can be paralleled by reference to the knowledge already acquired regarding the spectrum of hydrogen.

Dr. Frankland and myself were working together on the subject in 1869. In that year (*Proc.*, No. 112) we pointed out that the behaviour of the *h* line was *hors ligne*, and that the whole spectrum could be reduced to one line, F.

"1. The Fraunhofer line on the solar spectrum, named *h* by Angström, which is due to the absorption of hydrogen, is not visible in the tubes we employ with low battery and Leyden-jar power; it may be looked upon, therefore, as an indication of relatively high temperature. As the line in question has been reversed by one of us in the spectrum of the chromosphere, it follows that the chromosphere, when cool enough to absorb, is still of a relatively high temperature.

"2. Under certain conditions of temperature and pressure, the very complicated spectrum of hydrogen is reduced in our instrument *to one line in the green* corresponding to F in the solar spectrum."

As in the case of calcium also, solar observation affords us most precious knowledge. The *h* line was missing from the protuberances in 1875, as will be shown from the accompanying extract from the Report of the Eclipse Expedition of that year:—

"During the first part of the eclipse two strong protuberances close together are noticed; on the limb towards the end these are partially covered, while a series of protuberances came out at the other edge. The strongest of these protuberances are repeated three times, an effect of course of the prism, and we shall have to decide if possible the wave-lengths corresponding to the images. We expect *à priori* to find the hydrogen lines represented. We know three photographic hydrogen lines: F, a line near G, and *h*. F is just at the limit of the photographic part of the spectrum, and we find indeed images of protuberances towards the less refrangible part at the limit of photographic effect. For, as we shall show, a continuous spectrum in the lower parts of the corona has been recorded, and the extent of this continuous spectrum gives us an idea of the part of the spectrum in which each protuberance line is placed. We are justified in assuming, therefore, as a preliminary hypothesis, that the least refrangible line in the protuberance shown on the photograph is due to F, and we shall find support of this view in the other lines. In order to determine the position of the next line the dispersive power of the prism was investigated. The prism was placed on a goniometer table in minimum deviation for F, and the angular distance between F and the hydrogen line near G, *i.e.*, $H\gamma$, was found, as a mean of several measurements, to be 3′. The goniometer was graduated to 15″, and owing to the small dispersive power, and therefore relatively great breadth of the slit, the measurement can only be regarded as a first approximation. Turning now again to our photographs, and calculating the angular distance between the first and second ring of protuberances, we find that distance to be 3′ 15″. We conclude, therefore, that this second ring is due to hydrogen. We, therefore, naturally looked for the third photographic hydrogen line, which is generally called *h*, but we found no protuberance on our photographs corresponding to that wave-length. Although this line is always weaker than $H\gamma$, its absence on the photograph is rather surprising, if it be not due to the fact that the line is one which only comes out at a high temperature. This is rendered likely by the researches of Frankland and Lockyer (*Proc.* Roy. Soc., vol. xvii. p. 453).

"We now turn to the last and strongest series of protuberances shown on our photographs. The distance between this series and the one we have found reason for identifying with $H\gamma$ is very little greater than that between $H\beta$ and $H\gamma$. Assuming the distances equal, we conclude that the squares of the inverse wave-lengths of the three series are in arithmetical progression. This is true as a first approximation. We then calculated the wave-length of this unknown line, and found it to be approximately somewhat smaller than 3,957 tenth-metres. No great reliance can be placed, of course, on the number, but it appears that the line must be close to the end of the visible spectrum.

"In order to decide if possible what this line is due to, we endeavoured to find out both by photography and fluorescence whether hydrogen possesses a line in that part of the spectrum. We have not at present come to any definite conclusion. In vacuum tubes prepared by Geissler containing hydrogen, a strong line more refrangible than H is seen, but these same tubes show between $H\gamma$ and $H\delta$, other lines known not to belong to hydrogen, and the origin of the ultra-violet line is therefore difficult to make out. We have taken the spark in hydrogen at atmospheric pressures, as impurities are easier to eliminate, but a continuous spectrum extends over the violet and part of the ultra-violet, and prevents any observation as to lines. We are going on with experiments to settle this point.

"Should it turn out that the line is not due to hydrogen, the question will arise what substance it is due to. It is a remarkable fact that the calculated wave-length comes very close to H. Young has found that these calcium lines are always reversed in the penumbra and immediate neighbourhood of every important sun-spot, and calcium must therefore go up high into the chromosphere. We draw attention to this coincidence, but our photographs do not allow us to draw any certain conclusions.

"At any rate, it seems made out by our photographs that the photographic light of the protuberances is in great part due to an ultra-violet line which does not certainly belong to hydrogen. The protuberances as photographed by this ultra-violet ray seem to go up higher than the hydrogen protuberances, but this may be due to the relative greater length of the line."

In my remarks upon calcium I have already referred to the fact that the line which our observation led us to believe was due to calcium in 1875, was traced to that element in this year's eclipse. The observations also show the curious connection that, at the time when the hydrogen lines were most brilliant in the corona, the calcium lines were not detected; next, when the hydrogen lines, being still brilliant, the *h* line was not present (a condition of things which, in all probability, indicated a reduction of temperature), calcium began to make itself unmistakably visible; and finally, when the hydrogen lines are absent, H and K become striking objects in the spectrum of the corona.

To come back to *h*, then, I have shown that Dr.

Frankland and myself, in 1869, found that it only made its appearance when a high tension was employed. We have seen that it was absent from among the hydrogen lines during the eclipse of 1875.

I have now to strengthen this evidence by the remark that it is always the shortest line of hydrogen in the chromosphere.

I now pass to another line of evidence.

I submit to the Society a photograph of the spectrum of indium, in which, as already recorded by Thalèn, the strongest line is one of the lines of hydrogen (*h*), the other line of hydrogen (near G) being absent. I have observed the C line in the spark produced by the passage of an induced current between indium poles in dry air.

As I am aware how almost impossible it is to render air perfectly dry, I made the following differential experiment. A glass tube with two platinum poles about half an inch apart was employed. Through this tube a slow current of air was driven after passing through a U-tube one foot high, containing calcic chloride, and then through sulphuric acid in a Wolff's bottle. The spectrum of the spark passing between the platinum electrodes was then observed, a coil with five Grove cells and a medium-sized jar being employed. Careful notes were made of the brilliancy and thickness of the hydrogen lines as compared with those of air. This done, a piece of metallic indium, which was placed loose in the tube, was shaken so that one part of it rested against the base of one of the poles, and one of its ends at a distance of a little less than half an inch from the base of the other pole. The spark then passed between the indium and the platinum. The red and blue lines of hydrogen were then observed both by my friend Mr. G. W. Hemming, Q.C., and myself. Their brilliancy was most markedly increased. This unmistakable indication of the presence of hydrogen, or rather of that form of hydrogen which gives us the *h* line alone *associated into* that form which gives us the blue and red lines, showed us that in the photograph we were not dealing with a physical coincidence, but that in the arc this special form of hydrogen had really been present; that it had come from the indium, and that it had registered itself on the photographic plate, although ordinary hydrogen persistently refuses to do so. Although I was satisfied from former experiments that occluded hydrogen behaves in this respect like ordinary hydrogen, I begged my friend Mr. W. C. Roberts, F.R.S., chemist to the Mint, to charge a piece of palladium with hydrogen for me. This he at once did, and I take this present opportunity to express my obligation to him. I exhibit to the Society a photograph of this palladium and of indium side by side. It will be seen that one form of hydrogen in indium has distinctly recorded itself on the plate, while that in palladium has not left a trace. I should add that the palladium was kept in a sealed tube till the moment of making the experiment, and that special precautions were taken to prevent the two pieces between which the arc was taken becoming unduly heated.

To sum up, then, the facts with regard to hydrogen; we have *h* differentiated from the other lines by its appearance alone in indium; by its absence during the eclipse of 1875, when the other lines were photographed; by its existence as a short line only in the chromosphere of the sun, and by the fact that in the experiments of 1869 a very high temperature was needed to cause it to make its appearance.

With regard to the isolation of the F line I have already referred to other experiments in 1869, in which Dr. Frankland and myself got it alone.[1] I exhibit to the Society a globe containing hydrogen which gives us the F line without either the red or the blue one.

[1] See also Plücker, *Phil. Trans.*, 1865, part 1, p. 21.

The accompanying drawing shows how these lines are integrated in the spectrum of the sun.

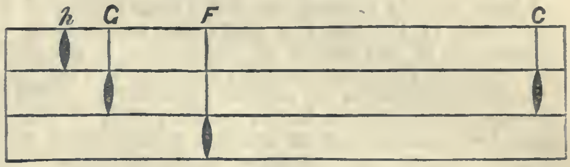


Fig. 6.

I have other evidence which, if confirmed, leads to the conclusion that the substance which gives us the non-reversed line in the chromosphere and the line at 1474 of Kirchhoff's scale, termed the coronal line, are really other forms of hydrogen. One of these is possibly more simple than that which gives us *h* alone, the other more complex than that which gives us F alone. The evidence on this point is of such extreme importance to solar physics, and throws so much light on star structure generally, that I am now engaged in discussing it and shall therefore reserve it for a special communication.

In the meantime I content myself by giving a diagram in which I have arranged the various groupings of hydrogen as they appear to exist, from the regions of highest to those of lowest temperature in our central luminary.

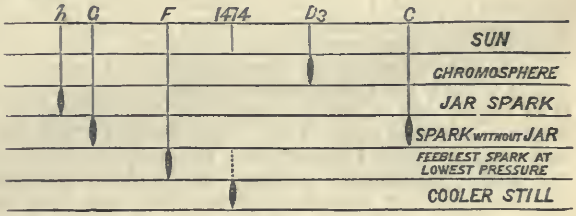


Fig. 7.

Summation of the above Series of Facts

I submit that the facts above recorded are easily grouped together, and a perfect continuity of phenomena established on the hypothesis of successive dissociations analogous to those observed in the cases of undoubted compounds.

The other Branches of the Inquiry

When we pass to the other possible evolutionary processes to which I have before referred, and which I hope to discuss on a future occasion, the inquiry becomes much more complicated by the extreme difficulty of obtaining pure specimens to work with, although I should remark that in the working hypothesis now under discussion the cause of the constant occurrence of the same substance as an impurity in the same connection is not far to seek. I take this opportunity of expressing my obligations to many friends who have put themselves to great trouble in obtaining specimens of pure chemicals for me during the whole continuance of my researches. Among these I must mention Dr. Russell, who has given me many specimens prepared by the lamented Matthiessen, as well as some of cobalt and nickel prepared by himself; Prof. Roscoe, who has supplied me with vanadium and cæsium alum; Mr. Crookes, who has always responded to my call for thallium; Mr. Roberts, chemist to the Mint, who has supplied me with portions of the gold and silver trial plates and some pieces of palladium; Dr. Hugo Müller, who has furnished me a large supply of electrolitically-deposited copper; Mr. Holtzman, who has provided me with cerium, lanthanum, and didymium prepared by himself; Mr. George Matthey, of the well-known metallurgical firm of Johnson and Matthey, who has provided me with magnesium and aluminium of marvellous purity; while to

Mr. Valentin, Mr. Mellor, of Salford, and other friends, my thanks are due for other substances.

I have already pointed out that a large portion of the work done in the last four years has consisted in the elimination of the effects of impurities. I am therefore aware of the great necessity for caution in the spectroscopic examination of various substances. There is, however, a number of bodies which permit of the inquiry into their simple or complex nature being made in such a manner that the presence of impurities will be to a certain extent negligable. I have brought this subject before the Royal Society at its present stage, in the hope that possibly others may be induced to aid inquiry in a region in which the work of one individual is as a drop in the ocean. If there is anything in what I have said, the spectra of all the elementary substances will require to be re-mapped, and re-mapped from a new standpoint; further, the arc must replace the spark, and photography must replace the eye. A glance at the red end of the spectrum of almost any substance incandescent in the voltaic arc in a spectroscope of large dispersion, and a glance at the maps prepared by such eminent observers as Huggins and Thalén, who have used the coil, will give an idea of the mass of facts which have yet to be recorded and reduced before much further progress can be made.

In conclusion I would state that only a small part of the work to which I have drawn attention is my own. In some cases I have merely, as it were, codified the work done by other observers in other countries. With reference to that done in my own laboratory, I may here repeat what I have said before on other occasions, that it is largely due to the skill, patience, and untiring zeal of those who have assisted me. The burthen of the final reduction, to which I have before referred, has fallen to Mr. Miller, my present assistant; while the mapping of the positions and intensities of the lines was done by Messrs. Friswell, Meldola, Ord and Starling, who have successively filled that post.

I have to thank Corporal Ewings, R.E., for preparing the various diagrams which I have submitted to the notice of this Society.

EXPERIMENTS IN ELECTRIC LIGHTING

MR. LOUIS SCHWENDLER, Superintendent Electrician of the Government of India, has been conducting a very careful series of experiments in London on electric lighting, with a view to decide upon the advisability of introducing this method of lighting into railway stations in India. He has just published a *précis* of his forthcoming report, and as the experiments were conducted on thoroughly scientific principles, and solely with a view to discover the most effective method, the results attained are extremely valuable, especially when so many systems are competing for public favour :—

First, with regard to quantity of light per unit of power, unit of speed, and unit of money (first outlay). To solve this question Mr. Schwendler tried four different dynamo-electric machines producing the electric current in *one* direction, viz., A, medium size, B, small; as supplied by Messrs. Siemens, Brothers, of London (construction :—Siemens, system :—Hefner von Alteneck). C, workshop pattern, as supplied by Messrs. Soutter and Lemonnier, of Paris (construction :—Gramme). D, with two sets of brushes as supplied by the British Telegraph Manufactory (construction :—Gramme). Mr. Schwendler finds these four machines all sufficiently practical for the production of the electric light, but, as a rule, the statements of their actual efficiency were not found to be in conformity with the results obtained from his own experiments. The quantity of light produced by these dynamo-electric machines had been over-rated, and the amount of power consumed underrated. But, notwithstanding this, he finds that the unit of light as produced in the electric arc (disintegration) by any of the four dynamo-electric machines is at least fifty times cheaper than the unit of light as produced by combustion, considering the expenditure of power only. This represents an enormous engineering margin in favour of the electric light.

Mr. Schwendler makes a most important proviso by stating that this relation only holds good as long as *one* dynamo-electric machine produces *one* electric light; he returns to this point in a subsequent part of his careful *précis.*

The three dynamo-electric machines B, C, and D, he found practically equal; the dynamo-electric machine A gives a much stronger light for a comparatively smaller expenditure of power. In round numbers it may be said that dynamo-electric machine A gives about double the quantity of light given by any of the other three machines, and that only about half as much power is expended to produce the unit of light. This favourable result, Mr. Schwendler states, is principally due to the comparatively small internal resistance of A, and its low speed.

Secondly, with reference to constancy and regularity of the electric light, Mr. Schwendler says that this appears to be still the weak point, and many improvements in this respect are possible and desirable. He has tried two different lamps :—*a.* The Serrin lamp, as supplied by Messrs. Soutter and Lemonnier, of Paris, and the British Telegraph Manufactory; *b.* The Siemens lamp, as supplied by Messrs. Siemens Brothers. The Serrin lamp, for any given adjustment, regulates the length of the arc only in *one* direction, *i.e.*, it diminishes that length. The actual consumption of the carbon points regulates the length of the arc in the other direction, *i.e.*, increases it. In the Siemens lamp the decrease of the length of the arc is effected exactly in the same manner as in the Serrin lamp, but the increase in the length of the arc is not only left to the consumption of the carbon points—a comparatively slow process—but is accelerated by the addition of a make and break arrangement, which separates the carbon points. Hence, from a theoretical point of view, the Siemens lamp is undoubtedly superior, since the length of the arc is rapidly adjusted in both directions, and consequently the working currents can increase to a very considerable degree without spoiling the dynamo-electric machine. But practically Mr. Schwendler finds the Siemens lamp somewhat difficult to manage, and although, when *once* well adjusted, it burns as regularly as the Serrin lamp, it is far more difficult to arrive at this adjustment. For practical use he prefers, therefore, the Serrin lamp, with those alterations and constructional improvements which his own experiments have suggested. A second cause of the irregularity of the electric light is the still imperfect state of the carbon points. Of late some great improvements have been made in the manufacture of artificial carbons, but much more is required, and this point deserves the closest attention. In his final report Mr. Schwendler will treat this subject in detail. To make the electric light more steady, Mr. Schwendler states, should be considered one of the most important questions to be solved.

Thirdly, Mr. Schwendler considers the question as to how to put up the light, its position, and mechanical details. Under this head he considers the method of dividing the electric light, *i.e.* producing by the *same electro-motor* a number of lights at different points of a given space. This method, which he does not consider as yet solved, appears to him impracticable from an engineering point of view. He refers to the immense loss of strength in thus dividing the light, increasing in enormous proportion with the increase of sub-division. Mr. Schwendler, after his careful, severe, and long-extended trials, comes to the decided conclusion that the electric light can alone compete with light produced by combustion, when produced of great intensity in *one*

point by *one* dynamo-electric machine. Endeavours to cut up the electric light into a large number of small lights, although of great interest, must, he thinks, invariably result in engineering failure, as nobody could afford to pay for the luxury received. Thus, in the opinion of so competent a judge, all methods hitherto employed for using this method of lighting in public, are failures, involving a waste of power and money, with inadequate result. Having satisfied himself of the difficulty and impracticability of the division of the electric light, Mr. Schwendler tried diffusion, *i.e.*, a few large lights (each light produced by one machine) are placed at different points of the space, and by optical means the light is diffused over a large area. This method he finds perfectly practicable. There is naturally a large amount of light lost (by absorption), but, he states, this loss will bear a constant ratio to the total light produced, nay probably may decrease with the intensity. The actual plan by which Mr. Schwendler proposes to do it, and has done it during the trial, is to construct a silvered glass reflector in which a powerful electric light burns, throwing direct and reflected rays up to a white ceiling or any other convenient white surface. A number of such arrangements is to be put up in the most convenient places, and where they have the greatest effect. The form and size of each reflector will depend on the locality where it is to be used.

Any repairs required in course of time Mr. Schwendler believes can easily be effected by an ordinary mechanic. Only one man, he states, is actually required in each station, to take charge of the steam engine, dynamo-electric machine, lamps, and reflectors. Mr. Schwendler appends to this *précis* several details with reference to the adaptation of electric lighting to Indian stations, and also on some of the scientific results obtained by his experiments. Altogether his report is likely to be the most important contribution to a thorough knowledge of the public utilisation of electric lighting that has been made since the question has been resuscitated during the last year or two; and we should advise all interested in the subject to wait for his report before taking decisive steps to the adoption of any particular system.

NOTES

THE Emperor of Germany has approved of the election of Mr. Darwin and Prof. Owen as Foreign Members of the Berlin Academy of Sciences.

WE are glad to learn that M. Raoul Pictet is quite restored to health. The University of Geneva has conferred upon him the honorary degree of Doctor, and he has just been made a Chevalier of the Legion of Honour by the French Government, in recognition of his eminent services to science, and especially of his successful experiments in the liquefaction of gases.

A COMPETITIVE examination is going on at the Paris Conservatoire des Arts et Métiers, for the appointment of a professor of physics and meteorology at the National School of Agriculture. The examination has been conducted on a new principle by a jury presided over by M. Boussingault. Each of the seven candidates has expounded before the jury his programme of lectures to be delivered, and each of them has in turn delivered a lecture on physics, and another on meteorology, after a preparation of four hours. The competition is open to all without any condition of age, qualifications, and nationality; but the jurymen are instructed to attend, in giving their verdict, to the degrees obtained by candidates and their previous work or discoveries.

THE Municipal Council of Paris has voted a subvention of 2,000 francs to M. Joseph Vinot, editor of the *Ciel*, for a series of popular lectures on astronomy, to be delivered at the Salle des Écoles, rue d'Arras. Admiral Mouchez, who was present at the last lesson, announced to the pupils, numbering from 400 to 500 that he will take measures to admit them to the observatory *seriatim*, in order to initiate them into the use of the large astronomical instruments so ably described by their professor. In one of the last reports read before the Paris Municipal Council it was stated that it would be necessary to establish somewhere in Paris an observatory of popular astronomy entirely devoted to the public exhibition of celestial phenomena, other establishments being entirely devoted to investigation.

PROF. S. P. THOMPSON has reprinted his valuable address on "Technical Education," given at the Social Science Congress last October. In this time of intense depression, when trade seems to be drifting from our shores, and people are wondering how it is that other nations are outstripping us in departments that used to be considered as peculiarly British, Prof. Thompson's remarks on the ignorance of our mechanics are peculiarly appropriate. One telling instance he gives of the lamentable want of intelligent skill that prevails among workmen and manufacturers in this country:—"I was recently informed by Prof. Graham Bell that he is about to return to America to resume his researches in telephony, his principal reason for quitting his native shores once more being that he found himself, in this country, unable to get his ideas carried out, unable to procure workmen capable of comprehending and carrying out new ideas, such workmen, in fact, as he was able to employ during his four years' residence in America. He pointed to the laboratory of Mr. Edison as an example of an institution to which there is no parallel in this country, though there are several in the States, a laboratory equipped with a staff of trained workmen, Americans, Germans, or Englishmen, whose business is not to work on old lines, but to carry out and put into practical form new and untried devices. No wonder inventions multiply when inventors have so powerful an aid as this to further their designs; and, mark this, Mr. Bell returns to set up a similar laboratory because he cannot find in his native country men whose technical training would qualify them for his particular work." In a note Mr. Thompson gives the following paragraph from a letter of Prof. Graham Bell to a friend in America which has been going the round of the American press:—"If you want to know why inventors are more numerous in America than they are here, come and live for six months in England. If you wish to know how it feels to be brimfull of ideas, and yet to be unable to have one of them executed, come to England. If you wish to know how it feels to have to wait for a month to have the simplest thing made, and then be charged a man's wages for two months, come to England. You will here be unable to see the interior of a workshop or to come into direct contact with your workmen, and the people seem incapable of working except in the ruts worn by their predecessors. They are absolutely incapable of calculating any new design without the most laborious oversight from the inventor, and their masters, instead of encouraging invention, do all they can to put a stop to it by refusing admission to the workshops and charging the most exorbitant prices for experimental work, avowedly because 'they don't want such kind of work,' 'it gives more trouble than it is worth;' and 'if you must have new things made you must expect to pay for them!' It is in vain that I say I am willing to pay anything to have my work done, and that what I object to is having to pay for not having it done. It is the same everywhere. Not only is your work not done, but you have to wait so long for the simplest things that your ideas cool, and you get quite exasperated at your inability to do anything." The moral of all this is obvious.

THE widow of the late Prof. E. Eichwald has presented the remarkable palæontological collections of her husband to the St. Petersburg University, which already possessed a part of them. These collections, collected by the late Prof. Eichwald since 1825, number no less than 30,000 specimens of fossils from

the various formations of Western Europe, from the Petchora Land, from the Aleutian Islands, Siberia, Crimea, &c.; several European geologists as well as the late Mr. Peabody, have many times negotiated for the purchase of them.

ON December 22, Prof. Forster, Director of the Bern Observatory, gave to the Bern Economical Society a very interesting report on weather-warnings. After a sketch of the development of these warnings during past years in Europe, he pointed out the importance of the "Service Agricole," established between France and Austria, by which daily telegrams are sent, advertising the coming weather; 85 per cent. of these prognostications having been perfectly true, 7 per cent, approximately true, and only 8 per cent. untrue. As to the introduction of such a service in Switzerland, it would meet with great difficulties because of the great variety of topographical features of the country and of the incomplete knowledge of the local climatic conditions. These last having been, however, carefully studied since 1864, the first steps towards the establishment of such a service were recently made by the Swiss Meteorological Commission, some of whom wished to undertake immediately to introduce weather warnings into Switzerland, whilst the majority of the Commission was for the adjournment of them for one or two years. The Society expressed their desire for the introduction of weather prognostics as soon as possible, and we may hope that shortly Switzerland also will have its weather warnings.

The Association Scientifique de France will commence at the Sorbonne its series of lectures for 1879. On January 16 M. Milne-Edwards, president of the Association, will be in the chair. M. de Lesseps will lecture on Central Africa. The lecture will be illustrated by dissolving views made with drawings sent by Gordon Pasha from the newly-annexed Egyptian provinces.

THE *Reale Istituto Lombardo di Scienze e Lettere* has recently published a list of subjects for prizes to be awarded in this and following years. Among these subjects are the following:—Nosological geography of Italy; critical history of the telephone; on the nature of miasma and contagion; on the direction of balloons; is the generative material of hydrophobia a virulent principle or an organic germ? history of the progress of anatomy and physiology in the present century, especially with regard to the doctrine of Gall; illustration of some facts of the macro- or microscopic anatomy of the human brain; motor centres of the cerebral system; statistics of motor force, hydraulic and steam, in and around Milan.

THE Borough engineer of Liverpool has issued a not very encouraging report on the result of his visit to Paris for the purpose of examining the systems of electric lighting in use there. They are much more expensive than gas in Liverpool, and more than 50 per cent. of the light is absorbed by the globes used. Nevertheless he recommends a trial in Liverpool to find if no more economical method can be discovered. We recommend to him a perusal of Mr. Schwendler's report referred to elsewhere.

WE have received from Spain two numbers of a fortnightly scientific journal, published at Barcelona, which we welcome as a hopeful sign of progress in that country. The *Crónica Científica*, as the journal is called, while containing considerable extracts from foreign journals, and reports of foreign science, has a fair proportion of original contributions from Spanish investigators.

WE have received the number of the *Anales* of the Argentine Scientific Society for November. Among the papers is the continuation of M. Carlos Berg's monograph on the *Hemiptera argentina*, and a paper by Dr. D. Tomás Peron on the bark of Quebracho Blanco (*Aspidosperma quebracho*). Among the honorary members of this Society we notice the name of "Dr. Carlos Darwin;" while "Juan Lubbok, Londres," is a corresponding member.

IN connection with the Yorkshire Naturalists' Union, a "Grand Exhibition" of natural history specimens and scientific apparatus will be held from January 10 to 16 at the Mechanics' Institute, Leeds. On January 10 the Exhibition will be opened by the President of the Union, Mr. H. C. Sorby, F.R.S. The exhibition will be opened every succeeding day from 10 A.M. to 10 P.M., and will include in the various departments of natural history a large number of objects, including some of the finest private collections in the county, whilst the Physical Science Department will contain all the latest scientific novelties, including the apparatus for the liquefaction of oxygen, &c. There will also be a quantity of apparatus seldom exhibited to the public, together with instruments used in important original researches. Every evening a series of demonstrations and short addresses, and selections of music will be given. Microscopes and aquaria will be constantly on view. In the basement will be shown the process of manufacture of scientific apparatus. Although the announcement of this exhibition smacks a little too much of Barnum and the penny show, still the exhibition and lectures seem likely to be productive of permanent good in the district.

THE International Piscicultural Exhibition which will take place at Berlin in the spring of next year promises to be extremely interesting. A number of English, Russian, American, and even Japanese and Chinese exhibitors have already promised to take part in it. The Crown Prince of Germany has undertaken the protectorate of the exhibition and takes a great interest in its success.

HERR C. RECLAM has recently made a detailed report of the first cremation which took place at Gotha a short time ago. He calculates the cost of each cremation at about 4*l.*, which in case the furnace is in continual use, so that between two processes it has not time to cool, would be reduced to 3*l.* It is hoped in Germany that the example of Gotha will soon be followed by other cities so that Milan and Gotha will no longer be the only cities where cremation takes place.

PROF. ASA GRAY, we learn from *Harper's Weekly*, announces the detection, after the lapse of a hundred years, of a plant obtained by Michaux in the mountains of North Carolina, and known as the *Shortia galacifolia*, the re-discovery also having occurred in M'Dowell County, in North Carolina, in a region east of the Black Mountains.

Science News for December 15 contains some "Later Notes on Texan Birds," by George B. Sennett, the result of a second journey to south-western Texas made last spring. The Notes chiefly refer to nesting habits of the more peculiar forms. The region was on the banks of the Rio Grande within a few miles of its mouth, with Lomila Ranche as the central point.

"VÖGELBILDER aus fernen Zonen" is the title of an atlas of foreign birds just published by Fischer, of Cassel, under the care of Dr. Ant. Reichenow. The first part, before us, contains three beautifully coloured plates of birds, artistically grouped amid suitable surroundings, each plate having explanatory text, in which each species represented is pretty fully described, giving not only the scientific but also the native, German, French, English, and other names of the birds. The three plates are devoted to parrakeets and allied kinds. The same publisher sends us a series of large zoological wall-plates, admirably adapted, it seems to us, to teaching zoology in a thoroughly scientific manner. The plates sent us are mainly

devoted to the lower forms, the specimens selected being enormously magnified, coloured, and exhibiting all details both of external and internal structure.

"THE Year Book of Facts in Science and the Arts," edited by James Mason (Ward, Lock, and Co.), is little better than a scrap-book of cuttings from various papers; we don't suppose it is seriously intended to represent the science of the past year.

MESSRS. HARDWICKE AND BOGUE have issued a second edition, "revised and corrected," of Mr. M. P. Edgeworth's work on "Pollen," noticed in our columns on its first publication (NATURE, vol. xvi. p. 499).

AT the last meeting, December 6, of the Russian Geographical Society, Col. Rykatchoff made a communication on the difficulty of organising observations on rain and storms.—Prof. Meller made a very interesting communication on the former connection between the Sea of Azov and the Caspian. The character of the geological formations on the tract between both seas proves without doubt that during the tertiary epoch the waters of both were connected; thus organic remains of Caspian origin are found within eighty-seven miles from the actual shores of the Sea of Azov.

AT the last meeting of the St. Petersburg Physical and Chemical Society, Prof. Beketoff made a communication on the atomic heat-capacity of hydrogen when mixed with palladium. He determined it as equal to 5·86, *i.e.*, very near to that of copper and silver.

THOSE of our readers who are in the habit of using chemical apparatus should get the Revised List just issued by Mr. Fletcher, of Warrington, who deserves credit for the very successful efforts he makes to introduce improvements into this department.

THE much-talked-of canal between Delaware and Chesapeake Bays, which will shorten the water-route from Baltimore to New York and Europe by 225 miles, is now at last to be constructed. Its length will be seventeen miles, and the cost is estimated at four million dollars. It will run through the Sassafras Valley and will have no locks.

THE additions to the Zoological Society's Gardens during the past week include a Punjaub Wild Sheep (*Ovis clyclōceros*) from India, presented by Col. W. R. Alexander; two Californian Quails (*Callipepla californica*) from California, presented by Mr. William Turquand; seven Brown Tritons (*Geotriton fuscus*), South European, presented by Prof. H. H. Giglioli, C.M.Z.S.; a Feline Dowrocouli (*Nyctipithecus felinus*) from South America, purchased.

FURTHER RESEARCHES ON THE SCINTILLATION OF STARS

SINCE last we reviewed M. Montigny's valuable researches on the scintillation of stars (vol. xviii. p. 292) he has again published some highly interesting details. The researches now in question refer specially to the changes of colour which characterise the scintillation of the *red* and *orange* stars. M. Montigny tried to solve the question whether the changes of colour in scintillation follow certain definite laws; whether, for instance, their relative frequency expressed in numbers, shows differences which depend on the nature of the star's own light, on the star's elevation above the horizon, or on the condition of the atmosphere.

In order to solve this complicated question, it was divided into its several parts. First of all M. Montigny investigated the influence of the star's own light and that of the condition of the atmosphere upon those colours which characterise the scintillation of the stars of the so-called third type. Our readers will remember that these are the stars which show black lines as well as dusky bands in their spectra; they are generally of a red or orange colour, and mostly variable. There are not many fine stars in this class, the most remarkable ones are about thirty in number, and M. Montigny has examined only the following fifteen:—β Andromedæ, α Ceti, ρ Persei, Aldebaran, Betelgeuze, α Hydræ, Arcturus, δ Virginis, δ Coronæ, α Serpentis, Antares, α Herculis, γ Aquilæ, and β and ϵ Pegasi. The evenings of observation now number 476, and reach from October, 1870, to February, 1878.

The way in which the observations were made was the following:—After each evening of observation not only the values for the intensity of scintillation were entered for each star, reduced to a distance of 60° from the zenith, but each single colour observed in the circular image was also noted down. Further, the observations made in wet weather were noted separately from those made during dry weather. Finally the various colours were entered on a table divided into seven columns, respectively headed—red, orange, yellow, green, blue-green, blue, and violet. The sum total of any column thus indicates the number of times which the colour in question was observed in a certain star. Arcturus, for instance, in 131 observations during moist weather, showed the red colour 130 times and blue 118 times. These numbers thus express the absolute frequency of these two colours. If we compare the number 130 for red, with the sum total of all colours shown by Arcturus during rainy weather, which is 491, then we obtain the relative frequency of red, which is 0·265, or multiplied by 1000 = 265. Therefore in 1000 changes of colour which appeared in Arcturus during rainy weather, red occurred 265 times, and blue 240 times.

In the following table we give the average frequencies of the different colours for the fifteen stars of the third type enumerated above; line A shows the frequencies observed in rainy weather, and B those observed in dry weather. The total of observations was 800 for A and 368 for B; the totals of the changes of colour observed were 2,982 for A and 1,368 for B.

	Intensity.	Red.	Orange.	Yellow.	Green.	Blue-green.	Blue.	Violet
A ...	60	272	194	239	57	4	230	3
B ...	43	278	213	222	63	5	216	4
Average	52	275	204	230	60	5	223	4

We observe in this table that (1) the relative frequency of red is far greater than that of any other colour in rainy weather as well as in dry; (2) red, green, and particularly orange are seen more frequently in dry weather than in wet; (3) the frequency of yellow and blue is on the contrary greater in wet weather than in dry.

Although the differences in the frequency of one and the same colour, according to the state of the atmosphere, are rather limited, they nevertheless indicate an important fact. It is also remarkable that the numeric differences in the complementary colours red and green on the one hand, and blue and yellow on the other, lie in the same direction. It is further worthy of notice that the greater frequency of blue in rainy weather agrees well with the fact that blue greatly predominates during such weather in the image of the star as shown by the scintillometer. This predominance of blue has also been frequently observed a short time previous to rainy weather.

The following table will be found interesting, as it contains the changes of colour and intensity of scintillation of the six brightest stars of the third type. In line I. are those of Betelgeuze, which is orange coloured, and the spectrum of which shows numerous broad bands, dissolvable into lines; line II. gives those of Aldebaran, pale red, whose changing spectrum has many well-defined lines and dark bands; line III. represents Arcturus, yellow-orange, with numerous dark lines not united into bands in its spectrum; line IV. gives those of α Hydræ, a yellow star with very dark lines in the spectrum; line V. those of Antares, red, with wide bands and very distinct lines; and line VI. those of α Herculis, yellowish red, with black lines and dark bands.

	Intensity.	Red.	Orange.	Yellow.	Green.	Blue.	Violet.
I. Betelgeuze ..	65	255	190	234	106	202	13
II. Aldebaran ...	62	255	186	232	104	210	13
III. Arcturus ...	61	253	120	246	130	219	32
IV. α Hydræ ...	55	284	162	253	113	188	—
V. Antares ...	53	266	121	245	130	219	33
VI. α Herculis ...	47	275	225	232	51	217	—
Average	57	265	167	240	106	209	15

These values show that the relative frequencies of the three principal colours, red, orange, and blue, remain within narrow limits for the six stars. Yet red seems to increase in frequency in the three last stars, two of which are of a decided red tint,

and whose intensity of scintillation is far smaller than that of the other three. With regard to the effect of the star's own colour it must be remarked that the relative frequency of that colour or of a nearly related one is often very great; thus, for instance, yellow is very frequent in the yellow star α Hydræ, and orange in the orange stars Betelgeuze and α Herculis. The total number of observations M. Montigny made of these six stars was 574.

For the sake of comparison M. Montigny has calculated the relative frequency of colours in two stars of the second type, to which, as our readers will remember, our own sun belongs too. The stars selected were Pollux, with a very characteristic spectrum, and Capella, which scintillates with great regularity. Both stars are yellow and their spectra show very thin dark lines. The average frequency of colours for these two stars is given separately, A in rainy weather, and B in dry weather, from a total number of 267 observations of Capella and 116 observations of Pollux.

	Intensity.	Red.	Orange.	Yellow.	Green.	Greenish blue.	Blue.	Violet.
A ...	88	281	88	280	86	7	250	8
B ...	63	299	41	304	122	26	194	14
Average	76	290	65	292	104	17	222	11

If we compare these results with the former ones we find the frequency of red and particularly that of yellow to be considerably greater in Capella and Pollux than in stars of the third type, while the frequency of orange is very much less, having decreased from 204 to 65. The influence of the weather was equally apparent in these stars; in dry weather red was more frequent, and in rainy weather blue. The relative values of green and violet are greater in these two stars than in the fifteen stars of the third type; probably these colours will become more important when the observations are extended in a larger measure to the stars of the two first types.

It is therefore proved beyond doubt, by the results above mentioned, that the changes of colour which characterise the scintillation of stars, are subject to general laws which are quite as regular and fixed as those which govern the changes in the intensity of the phenomenon as we pass from one type to another, or under the influence of rain and fine weather.

In one of the last numbers of the *Bulletin* of the Brussels Royal Academy of Sciences M. Montigny publishes the results of some researches concerning the influence of the aurora borealis on the scintillation of stars. We may return to this subject at a future date.

NOTES FROM NEW ZEALAND

MR. T. H. POTTS, of Ohinitahi, N.Z., sends us the following notes:—

Which Species of Pinus have Cones really Sessile?—In working up the habits of a collection of pines one has felt a difficulty in understanding why certain cones should be termed sessile; for example, the cone of *P. tuberculata* is described by Gordon in his "Pinetum;" also by Broun, in his "Forester," as "quite sessile." Why? Can such a description be correct at its early stage of life? It is then perched on a scaly foot-stalk, well developed; months elapse, its increasing bulk is protected with needle-pointed scales, its foot-stalk becomes curved, but is plainly visible; the mature cone, grey and glossy, clings tightly to its stem, it can scarcely be removed therefrom without tearing off a shell of bark adhering to the nasal scales; when wrenched off it shows a portion of its curved foot-stalk that has been embedded in the growing stem.

A very similar habit may be observed in *P. insignis.* Should not the cones of *P. tuberculata* and those of other species showing a similar habit, be described rather as *apparently sessile* than as "quite sessile"?

It may be mentioned that here *P. tuberculata* bears cones not only on the stem and main branches, but also on the soft green shoots of the outer branches, this would in part account for the foot-stalks becoming embedded in the growing bulk of the shoot.

P. insignis here bears cones of longer dimensions than those given by the authors before named; five specimens measure rather over seven inches in length, with a circumference of eleven inches.

Heredity.—One of my sons returning from a visit to the Chatham Isles, brought back with him a young pup of a famous colly breed. As soon as it was grown enough to run about it displayed an unusual excitement in the presence of horses by jumping upwards repeatedly towards their heads. As this trick or vice was unknown or unpractised by any of our dogs, it was, of course, soon remarked. On inquiry of a Chatham Island settler, I found this was a common trick in the colly family "Bell" sprang from; so Bell faithfully held to the habits of her race.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE following programme of the Natural Science Courses in Trinity College, Dublin, may be of interest to our readers:— In the Junior Sophister year natural science is represented by two courses—one in zoology, the other in botany. Students attending a course of lectures in natural science are examined three times during the term on the subjects of the lectures, and no student is allowed credit for his attendance who does not answer sufficiently. In the Senior Sophister year natural science is represented by one course in geology. In each term examinations are held of those students in the Junior and Senior Sophister classes who are qualified to become candidates for honours. At the Michaelmas examination in the Junior Sophister year prizes of 4*l.* and of 2*l.* are awarded by the Board, on the recommendation of the Honour Examiners, to the best answerers among the candidates. At the Hilary and Trinity examinations of the Junior and Senior Sophister years honours without prizes are awarded—of which honours there are two ranks. At the Michaelmas examination of the Senior Sophister year, examinations for Moderatorships are held. The Senior Moderators receive gold medals, and the Junior Moderators silver medals, which are given to them publicly before the University, by the Chancellor, at the commencements, when they are admitted to their degrees. The First Senior Moderator receives a large gold medal if specially recommended by the Court of Examiners. The subjects of examination for the Moderatorships in natural science are the following, each of which has equal weight:— 1. Physical Geography, Geology, and Palæontology. All the ordinary and honour courses of the Sophister years. A limited course to be announced each year. Course for 1879:—The Silurian Period. 2. General and Physiological Anatomy. A limited course to be announced each year. Course for 1879:— Circulation and Respiration. The Circulatory and Respiratory Organs in Mammals. The Respiration and Assimilation in Plants. 3. Zoology and Botany. All the ordinary and honour courses of the Sophister years. A limited course to be announced each year. Zoological Course for 1879:—The Non-Placental Mammals. Botanical Course for 1879:—The Fucoids. The Professors of Zoology, Botany, and Geology, give each a course of demonstrations and a course of lectures in each term, especially meant for Junior Sophisters. The Museum of Comparative Anatomy and Zoology is open, under the superintendence of the professor, to all students, to whom every facility is given for the prosecution of their studies. For the purposes of study fresh specimen of plants are, under the superintendence of the professor, to be had by the student from Mr. Frederick Moore, at the College Botanical Gardens, at Lansdowne Road; and a large number of mounted specimens of cryptogamic plants are also to be seen and examined in the herbarium. The Museum of Geology and Palæontology is open to the students attending the course.

THE Science and Art School of St. Thomas' Charterhouse Institution, Goswell Road (the largest in the United Kingdom), under the direction of the vice-chairman of the London School Board, commenced a new term on Monday evening the 6th inst. Since the commencement of the present session upwards of 600 of our elementary school teachers of London have taken advantage of the privileges offered by the classes. The attendance at the classes for experimental work in chemistry and physics has been very large.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, November, 1878.—Some time ago Prof. Thurston invented apparatus for re-determining the coefficients of friction of lubricated surfaces, and the laws governing such friction, for a wide range of temperatures, pressures, and velocities. The machines have been in use about five

years, and have furnished much useful information, some of which is here embodied in the opening paper.—Mr. Cooper investigates the driving power of leather belts.—Prof. Chase gives a series of verifications and confirmations of astronomical hypotheses and predictions, and a brief statement of the principles on which they are based.

Journal de Physique, November, 1878.—In a contribution to the theory of vowels, M. Bourseul, after pointing out that there are really as many distinct vowels as there are pitches of sound proper to the mouth, enumerates the vowels (ten in number) which he uses in speaking French. On examining the sounds of the mouth-cavity which correspond to them (apart from absolute pitch), he finds they fall into two divisions, one being in perfect accord with *do* major (*mi, do, sol, mi, do*), the other with *re* (*fa, si, re, fa, si*). This confirms in an unexpected way a principle discovered by the philologist, M. Rœhrig, in 1848. He noted the marked difference, or rather antagonism, of certain consonants and certain vowels, and he arranged the latter in two distinct classes, one comprising *a, o, ou*, the other *é, eu, u*. When studying the Tartar and Finnish tongues, he remarked that the vowels in a word of Tartaro-Finnish idiom were invariably of the same class. The derived languages have undergone alterations, whereby the traces of this original principle gradually disappears, still it may be recognised.—With regard to vibratory forms of liquids on circular metallic plates, M. Decharme finds that the widths of the striæ are inversely proportional to the square roots of the numbers of vibrations of the corresponding sounds.—Prof. Crova explains his important mode of comparison of the radiations emitted by calorific and dark sources; and in their continued paper of experimental researches on magneto-electric machines, MM. Mascart and Angot take up mixed machines, *i.e.*, those composed of magnets and electro-magnets.

December, 1878.—M. Deprez here describes the way in which he solves a problem relating to the work of steam in locomotive cylinders, viz., to trace, at a distance, a curve whose abscissæ and ordinates are respectively proportional to the path traversed by the piston and the pressure exercised by the steam on the piston. The solution rests on two principles—(1) Giving the sheet of paper (for the curve) a motion rigorously proportional to that of the piston; (2) Measuring at a certain point and *instantaneously*, the pressure of the steam on the piston.—M. Terquem communicates a paper on the use of plane liquid sheets (from bars and connecting threads) for experimental demonstration and measurement of the superficial tension. The determination in this way agrees with that by observation of the ascent of liquid in a capillary tube, whereas the process of counting drops gives perceptibly higher numbers.—M. Macé du Lépinay studies mathematically the subject of potential in electrodynamics and electromagnetism.

Verhandlungen des naturhistorischen Vereines der preussischen Rheinlande und Westphalens, thirty-fifth year, vol. v. part 1. (Bonn: Max Cohen and Son, 1878.)—Dr. Franz Leydig, herpetological reminiscences of Roesel von Rosenhof. Roesel was born in Nürnberg, 1705, and died there in 1759, and he wrote on amphibian reptiles, insects, crustacea, and spiders, worms and polyps, rotifers and infusoria.—Dr. Förster, a short monograph of some parasitical hymenoptera, in which many new genera and species are described.—P. Hesse, contribution to the molluscan fauna of Westphalia.—Dr. Theodor Wolf, on Cotopaxi and its last eruption on June 26, 1877, with two plates.—Dr. A. von Lasaulx, contribution to a knowledge of the igneous rocks (Eruptiogesteine) in the districts of the Saar and the Mosel, with two plates.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. xi. fasc. xvi. xvii.—This contains reports connected with the award of prizes, announcement of subjects for new prizes, &c.

Fasc. xviii.—On the integration of algebraico-differential equations of the first order and degree by means of linear functions, by Prof. Casorati.—On the dominant diseases of vines, by Drs. Garovaglio and Cattaneo.

SOCIETIES AND ACADEMIES

London

Royal Society, December 19, 1878.—"On the Chemical Composition of Aleurone Grains," by S. H. Vines.

In the first part of the paper an account is given of some experiments confirming those of Weyl (*Zeitschr. f. physiol. Chemie*, Bd. i.), which show that the reserve-proteids are stored up in the seeds of the blue lupin in the form of globulins (vitellin and myosin), and that the conglutin extracted from them by Ritthausen is a product of the alteration of these globulins by the reagents used in the process of extraction. Mr. Vines' micro-chemical observations further show that these globulins constitute the aleurone-grains.

In the second part Mr. Vines points out that, in addition to the globulins, the aleurone-grains contain a proteid which is soluble in distilled water. Such a solution of this substance does not become turbid in boiling; it gives a precipitate on the addition of a drop of nitric acid, soluble in excess; it gives the xanthoproteic and Millon's reactions; it gives an immediate precipitate with acetic acid and potassic ferrocyanide; and it gives a bright pink colour when treated with excess of strong caustic soda on the addition of a drop of dilute solution of cupric sulphate. This substance does not dialyse. These properties show that it belongs to the group of the peptones, and that it most resembles the *a* peptone of Meissner, or, adopting Kühne's nomenclature, hemialbumose, a name which may be provisionally applied to this substance also. The discovery of this substance is of interest in connection with the researches which have been made during the last few years into the existence of peptic ferments in the seeds and other parts of plants.

Linnean Society, Dec. 19, 1878.—Prof. Allman, president, in the chair.—A note on South African orchids, by W. Mansell Weale, was read by the secretary, and a series of drawings illustrating the above, and also other plants from the same region were likewise exhibited on the part of Mr. Weale. He points out that the supposed generic characters of *Mystacidium* and *Polystachyon*, founded on the "two-legged" caudicles of the pollinia, are fallacious.—In a short paper consisting of a description of some rare shells, by Mr. Sylvanus Hanley, *Melania Limborgi*, from British India, and *Leptomya gravida*, of uncertain habitat, were specially referred to as being unusual in several respects.—An interesting communication on the relations of *Rhabdopleura* was made by the president.—Messrs. F. M. Campbell, J. L. Hamilton, and J. J. McAndrew were elected Fellows of the Society.

Entomological Society, Dec. 4, 1878.—Mr. H. W. Bates, F.L.S., F.Z.S., president, in the chair.—The following elections took place: Mr. T. P. Newman as a Member, and Mr. J. Walker, R.N., as a Subscriber.—Mr. Stainton exhibited a series of specimens of *Glyphipteryx schœnicolella* taken by Mr. Threlfull near Witherslack.—Mr. Wood Mason exhibited and made remarks upon a stridulating beetle belonging to the *Rutelidæ*.—Prof. Westwood exhibited some insects and diagrams illustrative of so-called monstrosity, and contributed remarks thereon.—Mr. McLachlan exhibited a series of cases of the larvæ of Trichopterous insects forwarded to him by Dr. Fritz Müller of Santa Catharina, Brazil, one of which, Dr. Müller stated, had the peculiar habit of living on trees in the water that collects between the leaves of *Bromeliæ*, in which tadpoles, the larvæ of dragon-flies and other aquatic animals were also to be found. Mr. Bates stated that rain-water collects at the bases of the leaves of these plants and remains there for nine months out of the twelve. Dr. Müller had also forwarded a photograph of a number of cases which he considered to belong to some species of *Hydropsychidæ*, and in his own words, "make a funnel-shaped entrance to their houses with a net of which no spider need be ashamed." In reference to the opinion of Dr. Müller as to the homologies which appeared to exist between the neuration of various *Lepidoptera* and that of *Trichoptera*, Mr. McLachlan expressed his own belief that in a linear arrangement the orders *Lepidoptera* and *Trichoptera* should not be widely separated.—The Rev. A. Eaton exhibited a piece of "Kungu cake" from Lake Nyassa district, where, according to Livingstone and others, it is used extensively as food by the natives, who manufacture it from large quantities of a minute insect, conjectured to be a species of *Ephemeridæ*. From an exhaustive examination, however, Mr. Eaton found it to be a minute representative of the *Culicidæ*, probably belonging to the genus *Corethra*. In connection with the subject of insect-food as used by man, Mr. Distant remarked, he had learned from Mr. Chennell that *Erthesina fullo*, a very common eastern hemipterous insect was largely eaten by the Naga Hill tribes of North-eastern India.—Mr. Meldola in reply to some queries forwarded to him, as to the chemical composition of the bodies of insects, remarked that

the chitine, which comprised the horny external portion of the bodies of insects had been shown by analysis to contain about 6 per cent. of nitrogen; and as regards phosphates, Mr. Wm. Cole had burned some insects and found phosphoric acid in the ash.—Mr. Waterhouse forwarded for exhibition a living *Curculio*, found in an orchid house at Windsor, which was identified as one of the *Calandridæ*.—The Secretary read the report of the sub-committee appointed to consider the communication from the Board of Trade regarding the ravages of *Anisoplia austriaca* at Taganrog.—Mr. Butler communicated a paper on a collection of Lepidoptera from Cachar, North-east India.

Photographic Society, December 10, 1878.—James Glaisher, F.R.S., in the chair.—Papers were read, by Henry Cooper on a really reliable dry-plate process, by L. Warnerke on a case of the destruction of the latent image on washed emulsion, and its restoration, by W. Willis, jun., notes on the platinotype process, and by Edwin Cocking on the subjective and objective of pictorial photography.—Mr. Willis, in demonstrating his new platinum process, stated that ferric oxalate is sensitive to light, and then becomes ferrous oxalate; this, when dissolved in a hot solution of potassic oxalate, reduces the metal from chlorides and other salts of platinum. A sheet of paper is coated with a solution of ferric oxalate and potassic chloro-platinite, and then exposed to light under a negative; this produces a visible brownish ferrous image; it is then floated for a few seconds upon a hot solution of potassic oxalate and potassic chloroplatinite, the ferrous image becomes dissolved, and the combination thus formed reduces the platinum salt and forms the ultimate picture in metallic platinum.

Vienna

Imperial Academy of Sciences, November 14, 1878.—The following among other papers were read:—On a meteoric stone which fell at Dhulia, Hindostan, in November, 1877, by Dr. Brezina.

November 21, 1878.—On the behaviour of halogen derivatives of aromatic bodies towards water and lead oxide, by Professors Lippmann and Schmidt.—On the Clintonite group, by Prof. Tschermak and Herr Sipörz.—On the meteorite fall of Tieschitz, by Prof. Tschermak.

Paris

Academy of Sciences, December 30, 1878.—M. Fizeau in the chair.—The following papers were read:—Reply to M. Berthelot, by M. Pasteur. M. Trecul made some observations on the subject.—Borings undertaken by M. Roudaire, in view of the formation of an interior African sea, by M. de Lesseps. M. Roudaire writes on December 11, 1878, that having reached a depth of 18 metres, nothing but sand and water had been met with (no rocks). An explanation is offered of the exceptional tide of 2·50m. in the Gulf of Gabes.—M. Daubrée presented a map of the itinerary of Prof. Nordenskjöld in the glacial sea of Siberia, from August 7 to 27 last.—M. Cahours presented the three first vols. of the fourth edition of his treatise on general elementary chemistry. He indicates the additions and alterations made.—Report on the diplograph of M. Recordon, and his apparatus for use of the blind. This diplograph enables the blind person to produce ordinary writing and a seeing person to produce mechanically the characters the blind person can understand. It consists chiefly of two discs carrying respectively the signs and characters, and which are simultaneously applied to two sheets of paper, impressing the letter recognised by touch or by sight. M. Recordon is making a musical diplograph.—Harmotome and stilbite, by M. Gaudin. This is a study in atomic composition and arrangement (the two minerals contain 179 and 175 atoms respectively).—On electrochemical actions under pressure, by M. Bouvet. The decomposition of water by a current is independent of pressure. The quantity of electricity necessary to decompose a given weight of water is sensibly the same, whatever the pressure at which decomposition occurs. Oxygen and hydrogen, whatever the pressure, are liberated with equal facility, and there are no secondary phenomena causing recomposition, &c.—On the decomposition, at ordinary temperature, of an alkaline silicate by a salt of alumina (artificial hydrophane), by M. Monnier.—Determination, by M. Eylden's method, of the motion of the planet 103 Hera, by M. Callandreau.—On an interpretation of the imaginary values of time in mechanics, by M. Appell.—On an intuitive law according to which is distributed the weight of a solid circular disc, supported by an elastic horizontal base, by M. Boussinesq. The charge supported by each element of the base is that which would be directly over this element if we supposed the total charge distributed uniformly over the convex surface of a hemisphere, having the same base as the disc.—M. Joubert acknowledged M. Becquerel's priority in the experiment of magnetic rotation of the plane of polarisation under the earth's influence. The account had escaped his notice.—On a very precise way of observing the contact between the mercury and the ivory point in the basin in Fortin's barometer, by M. Goulier. The author has adopted a method similar to that indicated by M. Le Chatelier many years ago.—On the use of the telephone and microphone for scientific researches, by M. Hughes. This shows how delicate a means of observation these two instruments afford in researches relating to very weak currents, such as those from movement of a magnet before a helix. Several experiments are detailed.—On a new electric lamp, by M. Ducretet. The chief feature of this is the use of a column of mercury in which are immersed one or several crayons; the difference of density produces a thrust, which brings the crayons constantly and regularly to their point of application in proportion as they are consumed. One part of them becomes incandescent. An equal resistance in the circuit is insured, whatever the length and consumption of the crayons.—On the existence and conditions of formation of oxide of nickel, Ni_3O_4, by M. Baubigny.—On the nitrates found in beets and some other roots, by M. Barral. The greatest quantity of nitre per cent. of dry matter is found in the largest beets, and also in those that have least sugar. Beet is thus often given injuriously to cattle. In carrots, potatoes, and hay, 1 cc. (at the most) of bioxide of nitrogen was got in treating 5 to 10 grammes of dry matter, whereas for various beets the quantity never came below 14 cc.—Inertia of derivatives of chromium compared with the action of vanadium on salts of aniline in presence of chlorates in printing with aniline black, by M. Witz.—Analysis of raw sugars and saccharine matters; determination of water and all salts with mineral bases and organic acids, by M. Laugier.—On the harmlessness of borax in conservation of meat, by M. de Cyon. In M. Jourdes' process the borax is sprinkled lightly on the surface, and the meat retains its nutritive value. Prof. Panum, of Copenhagen, has proved the innocuity of borax and boric acid in meat-preserving.—Researches on the physiological action of *maté*, by M. Couty. It excites only, or at least primarily, the sympathetic system in those organs that are most independent of the nerve-centres; such as the intestines, the bladder, the accelerating nerves of the heart.—Poison of serpents, by M. Lacerda. The poison of certain serpents contains figured ferments showing remarkable analogies to bacteria.—On the function of chlorophyll in green Planaria, by M. Geddes. The gases they give off in sunlight contain 45 to 55 per cent. of oxygen, the rest nitrogen; hardly any carbonic acid.—Geological observations on Majorca and Minorca, by M. Hermite.

CONTENTS

THURSDAY, JANUARY 16, 1879

A SCOTTISH METEOROLOGICAL MOUNTAIN OBSERVATORY

IT is the opinion of those best versed in meteorological science, that much valuable information regarding the constitution of the earth's atmosphere, and the laws which determine the changes in the atmosphere, is to be obtained by observations at elevated stations. To quote the words of the distinguished French philosopher, Biot: "It is in the high regions of the air that meteors are formed, rain, snow, and hail. There the thunder rolls and the lightning traces its furrows. There the aurora displays its plume of light, and the aerolite shines and bursts. There are the upper currents which chariot the clouds. It is to these elevated regions that the inquirers of meteorological science ought to be directed."

For want of permanent stations at high levels, attempts have been made to explore the upper regions of the atmosphere in balloons. In the year 1862, Mr. Glaisher—at much personal peril—accomplished about thirty ascents with instruments which enabled him to ascertain with some precision the aerial temperature, humidity, clouds, and other phenomena, up to a height of several miles. More recently, Tissandier, in France, made twenty-four ascents in balloons, also with the result of obtaining valuable information on these points. Since February, 1877, Secretan, an enterprising optician in Paris, has been sending up small exploring balloons, for ascertaining the height of clouds and the direction of the aerial currents up to about 1,200 metres.

The value of the data obtained by these casual explorations has led meteorologists to a more systematic study of the upper regions of the atmosphere.

Thus Dr. Hildebransen of Upsala has been devoting himself to a study of clouds, to ascertain their altitude, movements, and shapes at different seasons; and he has recently issued a circular to meteorologists in other countries, pointing out the importance of the inquiry, and inviting co-operation.

It has been recently discovered in France, by observations at the Montsouris Observatory, that *dust* of various kinds is at most seasons of the year floating in the atmosphere, consisting of spherules so minute as to be discernible only by the microscope or by chemical tests: and that which so floats is not always the same in the higher as in the lower regions of the air. The bearings of this new information on epidemics affecting both animal and vegetable life is awakening much attention among continental physicists.

These remarks refer to the information afforded by meteorological observations at high stations regarding the constitution of the atmosphere or the ingredients existing in it. But there is another use to which high-level stations can be and are applied, viz., to furnish early intimation of changes in the weather. It appears from the observations made at the high-level stations of the Scotch Meteorological Society, that changes of temperature take place in the upper regions from twenty-four to thirty-six hours sooner than in the same district at ordinary low levels. Lately Prof. Loomis has been comparing the observations made at high-level and low-level stations in America, and he finds a considerable difference, not only in the speed and direction of the wind, but even in the barometric pressure.

In these circumstances it is not surprising that scientific meteorologists in all countries should in addition to low-level stations have made strenuous efforts to obtain also stations at high levels, and that they have been to a large extent successful. Thus in France two meteorological stations have lately been formed on the Puy de Dôme and the Pic du Midi, at heights respectively of 4,809 and 9,439 feet above the sea. In Austria Dr. Hann, one of the ablest European meteorologists, with Government aid, established a station in Upper Carinthia at a height of 8,000 feet above the sea. There are three stations in Italy, at heights respectively of 7,087, 8,343, and 8,360 feet above the sea, and a fourth is about to be established on Mount Etna, at the Casa Inglese, at a height of 9,652 feet above the sea. In Switzerland there is a station at the Hospice of St. Bernard, at 8,130 feet above the sea. In the United States the meteorological station at Mount Washington is 6,600 feet, Mount Mitchell 6,691 feet, and at Pike's Peak (Colorado State), 14,216 feet above the sea; all of these stations were established by the Government, which also supplies instruments and pays the observers, who are soldiers.

Such being the state of matters in foreign countries as regards high-level stations, what is the case in Great Britain?

It is believed that in England the highest meteorological station is 1,372 feet above the sea; and that in Scotland the two highest are, respectively, 1,334 and 1,450 feet. It is matter of regret that in both England and Scotland there should not be stations at higher points, seeing that there are in both countries favourable positions for such. That regret has been repeatedly expressed by authorities to whose opinion some regard might have been expected to be paid. In February, 1877, the president of the English Meteorological Society, in his address to the Society, pointed out that "the most obvious way of gaining a clearer insight into the condition and movements of the gaseous envelope of the earth, is by the establishment of observatories on isolated mountain peaks. The value of this arrangement had been practically recognised abroad, and might well be imitated on some of our highest hills, such as Skiddaw in the north of England, or on the western seaboard of Ireland or Scotland, where their fitness as outposts, for giving early indications of storms from the west, would soon be appreciated."

The suggestion of high-level stations has been repeatedly made by the Scotch Meteorological Society. Thus, in July, 1875, the Chairman of that Society, after alluding to the value of the observations at the American high-level stations, observed that, so impressed was the Society's Council with the importance of high-level stations, that, if funds were forthcoming, "probably the very first thing which the Council would endeavour to do would be to establish stations on the two highest points of Scotland, viz., Ben Nevis on the west coast, and Ben Macdhui on the east coast, and, by means of intermediate stations on the sides of these mountains, obtain, as suggested by Mr. Stevenson, vertical meteorological sections of the atmosphere."

Again, at a general meeting of the Scotch Society in July, 1877, the Chairman, in his address, renewed his reference to the desirability of a station on, at all events, Ben Nevis, and mentioned that Lord Abinger, the proprietor, having consented to the erection of a hut for the purpose, "the Society's Council would readily establish the station if it only had the requisite funds." Testimony to the value of high stations has in like manner been given by Mr. Robert Scott, the Secretary of the Government Meteorological Department.

Mr. Scott was the first and principal witness examined before the recent Government Meteorological Commission, and his opinion on the point of high-level stations is shown by the following questions and answers :—

Q. Has the Meteorological Committee felt it desirable to have stations at some higher levels in Great Britain?
A. Certainly they have.
Q. Have they thought of any particular plan?
A. They have had no money for it.
Q. Have they thought of any place if they had money?
A. I may mention Settle, because there is a telegraph station there, a high-level station, about 1,000 feet above the sea, on the borders of Yorkshire and Lancashire. If ever we had 30*l.* a year to spare, we should like to have a station there. It is found in cold weather that warm weather sets in at the upper stations perhaps one or two days before it comes down.

When such are the opinions which have been expressed and brought before the country by authorities deserving of respect and attention, it is hoped that the establishment of high-level meteorological stations will be no longer delayed. It is true that none of the mountains in the United Kingdom are so high as in the other countries above enumerated where high-level stations have been and are about to be established. But on the other hand the British Isles occupy a position more important for meteorological purposes than almost any other European country, inasmuch as their westward position enables British meteorologists to obtain the earliest information regarding the great storms which sweep across the Atlantic, and of which warning as soon as possible should be given in Europe. Great Britain's duty, interest, and credit as a nation concur therefore in this matter; and it is hoped that measures will soon be taken to establish these high-level stations in the three divisions of the United Kingdom, unless, in fact, we are prepared to see the problems presented by the phenomena of the higher air worked out in other countries.

COAL

Coal: its History and Uses. By Professors Green, Miall, Thorpe, Rücker, and Marshall, of the Yorkshire College. Edited by Prof. Thorpe. (London: Macmillan and Co., 1878.)

I SHALL be much surprised if this composite little volume fails to attract the attention of many students of nature, but especially of such persons as are practically connected with coal, either as students, as proprietors, or as workers. The five professors of the Yorkshire College of Science who have combined to produce the volume are men thoroughly competent to deal with the portions of the subject which they have severally undertaken, and the result of their labours is a book at once picturesque and scientific.

Prof. Green commences the work by two chapters on the geology of coal. After dealing with the general phenomena of the deposition of sandstones, shales, and limestones, he proceeds to examine the special features of coal and the probable conditions attending its formation. In doing so he grapples with the peculiar questions connected with the well-known lamination of coal and the alternation of the brittle, lustrous, bituminous layers with those of the so-called mother-of-coal or mineral charcoal. This subject of course raises the question of the origin of these two elements—a question which still presents curious and unsolved difficulties. The mineral charcoal chiefly consists of cubical fragments of bark mingled with some vascular or woody elements of plant-stems. Prof. Green correctly points out that "the woody character of this mother of coal is palpable even to the unaided eye," further adding "that it is possible, in some cases, to say what the plant was of which they (*i.e.*, the fragments of charcoal) originally formed a part." I am afraid that this latter feat is not easily performed. The vascular tissues which we chiefly find in this mineral charcoal are not those seen in Lepidodendron and in Sigillaria. The absence of the latter structures is one of the facts not easily explained. The vessels forming the ligneous zones of these two representatives of the carboniferous flora are always of the barred or pendoscalariform type. The dominant element in the mineral charcoal consists of the various modifications of cellular tissue found in the bark of these, and many other unrecognisable, plants, but the vessels are mainly of the reticulated type. They closely resemble the vascular elements found in some Calamites, in most Asterophyllites and Sphenophylla, and in all the known Lyginodendra. Whether the barred vessels of the Lycopodeaceous plants did not form a part of the vegetable mass converted into coal, or whether, being there, they were bituminised more readily than others, is not easy to say. I am inclined to believe in the latter explanation; anyhow, I have as yet failed to find a solitary fragment of one of these *barred* vessels in the mineral charcoal. A fragment of American coal, sent to me many years ago by that distinguished microscopist, Dr. Bailey, of West Point, consisted wholly of numerous layers of similar reticulated vessels which had not undergone bituminous disintegration. The laminæ of mineral charcoal alternate with those of the more bituminised coal with great irregularity; in some specimens they are distributed in about equal quantities, in others thick bituminous layers separate the charcoal layers very widely. Whatever the agency may have been that brought about the result, this lamination shows that, in most instances, there have been irregularly recurring periods when innumerable, perfectly carbonised, but only partially disorganised, fragments of certain decaying stems were strewed over the surfaces of those portions of the vegetable mass which became converted into the more bituminous laminæ of the coal. In other cases I find no difficulty in seeing the charcoal intermingled with, and actually undergoing conversion into, the bituminous condition, whilst interposed layers of perfect macrospores have undergone no such conversion. The charcoal fragments are

precisely such as we see when an old post, or the stem of a dead tree, undergoing decay, is breaking up into small cubical or rectangular pieces, and which would float freely upon the surface of an overflowing current of water.

My present impression is that these charcoal layers have originated in the decay of aged stems, such as have produced the "pot-stones" and "pot-holes" common in our coal-mines. The wood and the greater portion of the bark would accumulate, in a fragmentary state, within the hollow cylinders of the less easily destroyed bast-layer of the bark of such stems, until some recurring flood converted the damp ground into a temporary shallow lake, filling all these cylinders with water, when all such fragments would speedily float out, leaving the empty cylinders to be occupied by the sands and clays by which they are almost always permanently filled. The displaced vegetable fragments would swim freely for a time, diffused widely over the surface of the waters, but, as the latter subsided, they would settle down upon the layers of more completely disorganised vegetable soil, which, being already damp and soddened, would be likely to remain undisturbed by the temporary and sluggish currents that would exist where a forest-covered swamp was thus overflowed.

Prof. Green appears to balance very fairly the respective measures in which reproductive spores and the merely vegetative portions of the Carboniferous plants have contributed their quota to the carbonaceous mass. At the same time, much more work remains to be done before our theories on this subject can be regarded as wholly satisfactory.

The chief point in Prof. Green's work which I should be disposed to criticise is his map representing the probable distribution of sea and land during the carboniferous age. We are so ignorant of the extent to which denudation has affected vast geographical areas, that all such attempts appear to be inevitably hypothetical. At the same time Prof. Green's reasonings on the subject are extremely plausible and ingenious.

The third chapter of the volume is devoted to the coal plants—especially to the two types of Calamites and Lepidodendron, with the near relative of the latter, the Sigillaria. Since this portion of the volume is avowedly based upon my own memoirs, criticism of any kind would be out of place here. I would only observe that Prof. Miall appears to have reproduced my notions, be they right or wrong, with great accuracy. I may, however, observe here that one of the points for which I have so long contended, in opposition to my French friends, viz., the close identity of Asterophyllites and Sphenophylla, has just received unanswerable confirmation. Herr von Stur *has obtained both these types of foliage upon one single plant*, the former being the ordinary vegetative foliage, and the latter the whorls of leaves belonging to the fruiting branches.

In the fourth chapter we find Prof. Miall upon his own ground. In it he deals with the animals of the coal-measures, to the history of many of which he has made such valuable contributions. The chapter affords a rich illustration of the progress which the animal palæontology of the coal-measures has made during the last half century. Within that period Dr. Hibbert first called attention to the large teeth and bones of the Burdie-house limestone. It was only in the third decade of the century that we began to find scanty fragments of similar objects in the Ardwick limestones of Manchester, and it was at a yet later period that the Wigan Cannel was found to be rich in remains of similar objects. How changed this portion of our knowledge now is the reader of Prof. Miall's interesting chapter will readily learn. Prof. Miall wisely explodes the baseless notion of an atmosphere ladened with a superabundance of carbonic acid—existing during the Carboniferous age—a notion which still reappears in some geological works, but which rests upon no probable foundation. On the other hand he very correctly shows the importance of studying the fishes of the Carboniferous rocks when attempting to classify these rocks by the light of their animal remains. He points out the rarity of the Carboniferous Ganoids in the mountain limestone contrasted with the comparative abundance of the Elasmobranchs—a statement which every geologist familiar with these rocks will readily confirm, and he shows also how difficult it is to determine between the several influences of marine and fresh-waters in producing the Carboniferous beds. At the same time, fully admitting the correctness of Prof. Miall's statement that the Ganoids become proportionately more numerous as we ascend from the limestone to the newer beds, the Elasmobranchs in the upper coals of Lancashire and Yorkshire are far more numerous than in any known estuarian or fresh-waters at the present day, and consequently they place a great difficulty in the way of our regarding the upper Carboniferous, sedimentary deposits as being absolutely due to fresh water. Prof. Miall admits that his conclusions on this point compel us to admit "the supposition that the Elasmobranchs were more largely fluviatile than in any other periods." On the other hand, seeing how largely the noble ganoids of the Permian, Liassic, and Cretaceous ages were undoubtedly marine, it may be as readily contended that the Carboniferous Ganoids were marine as that their Elasmobranch companions were fresh-water.

In the fifth and sixth chapters the chemistry of coal is dealt with. For this portion of the work I presume we are wholly indebted to Prof. Thorpe and his auxiliaries in his Leeds laboratory. This portion of the work is of high interest. Dr. Thorpe has made numerous new analyses, and amongst other matters he has endeavoured to throw further light upon the question first raised by Prof. Huxley, viz., the true influence of Lycopodeaceous spores in producing the bituminous portions of the coal. But though Prof. Thorpe's researches into this question are highly interesting, the problem is far from solved; hence I believe he proposes to carry out a further series of analyses, in the hope of throwing further light upon this obscure but important subject. Space does not admit of my referring to the numerous other chemical aspects of coal which Prof. Thorpe so carefully records.

The four remaining chapters deal respectively with coal as a source of warmth, a source of power, and with what is commonly called the coal question; in the *two* latter chapters, ground gone over by Prof. Jevons, some years ago, is again traversed by Prof. Marshall. To attempt to criticise these latter chapters would be presumptuous on my part, still I cannot quite accept Prof. Marshall's conclusions that what he calls "physical

waste" in contradistinction to "commercial waste," should be allowed to go on. If his argument is a sound one, a tenant is justified in only getting such coal as is *most* cheaply obtained, and in leaving buried in the bowels of the earth, unattainable by future generations, valuable material merely because the getting of it would involve more cost than would attend the raising of coal from thicker and more cheaply workable seams. When a tenant rents *all* the coals under a given acreage of ground his interest in the property is temporary and limited. It is therefore to his interest to raise only such coal as can be most cheaply raised in the shortest space of time. It is not necessary that the raising of the coal which he thus abandons should involve an actual loss to the tenant. It is enough for the argument that he would have to raise it at a diminished profit, which he will certainly not do if he is permitted to devote the same time and labour to such coal as will leave him more profit. Of course freedom of trade and labour suggest that a man should be allowed to pursue the course most profitable to himself, but seeing the vital importance of our coal supply to our existence as a manufacturing nation, it does appear to me that the coal-raiser should be prevented by the lord of the manor primarily—or, if he fails to do his duty, by the legislature—from thus wasting the chief instrument in the production of our national wealth—even if such interference involved some reduction of his profits, or of some addition to the price of coal.

I need not say more to show the interesting character of the contents of this unpretending volume. It is most creditable to its authors, and I shall be much surprised if its merits do not meet with a wide recognition.

W. C. WILLIAMSON

ASCENSION

Six Months in Ascension; an Unscientific Account of a Scientific Expedition. By Mrs. Gill. (London: John Murray, 1878.)

THOSE who know anything of Ascension will wonder how on earth any one could find sufficient material during even a six months' stay to write a volume of 300 pages upon it. We doubt if any one but a lady circumstanced as Mrs. Gill was could have made a readable story out of the barren materials to be found on this land-ship of an island, as it really is, to all intents and purposes; and she has managed to write a thoroughly interesting narrative. If every scientific worker were as fortunate as Mr. Gill in having so sympathetic a companion and coadjutor to tell all the circumstances of his work, the world might think a great deal more of those apparently dry figures and easily-expressed results, which seem to have no touch of nature about them. To those who see only the scientific side of the matter, it may seem a very simple and very pleasant thing to watch the stars night after night; let such read Mrs. Gill's book, and perhaps they will have more sympathy and perhaps a little pity for astronomers sent on scientific expeditions to remote islands.

Mr. Gill, as many of our readers will remember, spent the last six months of 1877 on the Island of Ascension for the purpose of observing Mars in opposition, an opportunity occurring then such as would not occur again during the century. Lord Lindsay lent his celebrated heliometer, the Astronomical Society granted 500*l.*, and the Admiralty did all in their power to make Mr. Gill's sojourn on Ascension as pleasant and successful as possible. In an introductory chapter Mr. Gill gives an exceedingly interesting sketch of the principal previous attempts which have been made to measure the distance of the sun, and the results that have been come to with regard to that hitherto rather inconstant "constant." The perusal of this chapter will not only be instructive to the general reader, but must greatly increase the interest of the subsequent narrative, showing as it does the important results that depended on the success of the expedition, the human side of which is so graphically described by Mrs. Gill. She herself gives a brief but very successful explanation, in popular language, of how the sun's distance is measured.

As outgoing vessels seldom touch at Ascension, the expedition, consisting of Mr. and Mrs. Gill, with their heavy baggage, were detained for some time at St. Helena, a detention which both seem to have enjoyed. Mr. Gill succeeded in finding what he, with considerable probability, surmises to have been the site of the observatory used by Halley, in 1677, to observe the transit of Mercury, and make his catalogue of southern stars. They of course saw all the lions of the island, and Mrs. Gill's descriptions are so clear and fresh that even those who have read much about St. Helena will find in them much to interest. Even when Ascension was reached, it was no easy matter to land through the great "rollers" which are so characteristic a feature of some Atlantic islands. We hear a good deal about these puzzling phenomena, which are so difficult to account for, but which Mrs. Gill is inclined to think are probably due to the cause suggested by Capt. Evans. That keen-sighted hydrographer thinks these rollers are probably the far-reaching result of the breaking-up of the continents of ice in the Antarctic regions, miles and miles of which break off, and, plunging into the sea, give rise to huge submarine waves, whose strength is not expended even when they reach Ascension and St. Helena. Mrs. Gill's description of life in Ascension, and especially of the life of the "expedition," is given with a good-natured and graphic pencil. At first the observatory was established on an admirable piece of ground at George Town (or "Garrison," as it is called on the island), on the West Coast, where a very nice cottage was allotted to the astronomer and his wife. Her housekeeping difficulties are amusing enough to read of, though at first awkward enough to one unaccustomed to garrison or rather naval life. Ascension is to all intents and purposes one of her Majesty's ships at anchor, and everything is conducted exactly as on shipboard. All food and drink are served out at fixed time as rations, and as supplies are at all times limited, there is ample room for economical management, and little room for luxury and extravagance. The new-comers soon became accustomed to the routine of the land-ship, and after a day or two got comfortably settled in their cottage, and had the heliometer and other instruments most satisfactorily located on their solid asphalte floor. But, unfortunately, this happy state of things was not without a cloud—a real genuine cloud—which threatened to frustrate the great object of the voyage and all the preparations. This

cloud persistently hung over the island and prevented anything like satisfactory observation, and had it not been for Mrs. Gill's pluck, failure might have been the result. It occurred both to her and her husband that the cloud was only local, and to prove the truth of this suggestion, Mrs. Gill undertook a journey along the coast, starting about midnight, over great cinders and deep rifts, to a point almost four miles south. The supposition proved true, and amid many difficulties the observatory was dismounted, and the instruments removed to the south-west of the island, to a small inlet christened Mars Bay, in memory of the expedition. But what a change from "Commodore's Cottage," as the "Garrison" residence was called. Ascension is an extinct volcano, and it is now little more than a huge mound of cinders and dust. On such floor did Mr. Gill pitch his tent, and on such a base had he to erect his delicate instruments. The discomforts attending his surroundings knocked him completely up, but with the help of Mrs. Gill and the doctor he was set on his feet again, and by the ministry and companionship of the former the encampment was made tolerable. Fortunately after all these hardships and trials and doubts as to weather, the observations at the critical time were completely successful, as were a long series of subsequent comparison observations. The captain of the island and his subordinate officers deserve the greatest credit for the assistance and support which they gave to the enthusiastic astronomer and his ever-helpful and cheerful wife. After the real work of the expedition was completed Mr. and Mrs. Gill made several excursions over the tiny island, and with the exception of an oasis on the summit of the "mountain," the island seems dreary in the extreme, and Mrs. Gill failed to find the neat square gardens and paved streets seen by Sir Wyville Thomson. Altogether, on a very unpromising subject, she has succeeded in writing a really interesting and instructive book, telling us much about the islet and its inhabitants, and still more about the circumstances under which an important piece of scientific work was done. We strongly recommend it to the perusal of our readers.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Schwendler's Testing Instructions for Telegraph Lines

In the absence of my friend Mr. Schwendler in India, perhaps I may be allowed to offer a few remarks on the notice of his book on line-testing which appeared in NATURE, vol. xix. p. 192.

It must be remembered that the book is primarily intended for the use of the officers of the Indian Telegraph Department, and that the conditions in that country differ very much from those in England. Here the overland lines are in such positions that any accident happening to them may be easily detected, but in India the lines run in many parts through countries with few inhabitants, and the distances between stations is sometimes very great. Formerly when a breakdown or fault occurred, the line-riders were sent out from the stations to find out what was the matter, and Mr. Schwendler gave a very amusing account of two of these natives riding out from the two ends of the line to find a fault, meeting in the middle, salaaming and asking one another if anything wrong had been seen. On receiving a negative reply they salaamed and rode back, but the line was none the better for it. It must be, to say the least, tedious to ride many miles over a rough country staring at a wire on an Indian sky on the chance of finding a dead snake across the wires or a bird's nest on an insulator. These difficulties suggested the very systematic line-testing now in vogue in India.

It is unfortunate that the reviewer has shown such a disrespect for mathematical formulæ. There is no doubt that the book swarms with them, but it is by means of these that Mr. Schwendler has discovered many of the facts stated in the book, some of which your contributor seems to doubt. I will, with your permission, instance two cases. He writes, "Indeed it is very doubtful whether his proof that the sensibility of the bridge method is greatest when the branch and the resistance are equal is true. At any rate in our practice we find that the more delicate the galvanometer of the bridge the more sensitive and the more accurate is our test."

In the last sentence it is not quite clear what the reviewer means by a "delicate" galvanometer. I do not know that Mr. Schwendler says that the galvanometer should not be delicate, but he does say that its resistance should bear a certain relation to the resistance measured. For a high resistance a galvanometer of high resistance should be used, and for a low resistance one of a proportionately low resistance. But the alteration of the resistance should not be made by using only a portion of the coils. This is fully explained on p. 22, where it is shown that the coils should be connected either consecutively or parallel in order to increase or diminish the resistance of the instrument. Thus all the convolutions of the galvanometer are used, but as the resistance is diminished in the second case a larger flow of electricity takes place and a greater deflection is produced. The results of these theoretical considerations are so readily tested by experiment that it is surprising that the author of the notice should have thrown doubt on their accuracy. I have therefore thought that it might be useful to make some measurements which have fully confirmed the theory. I will not trouble you with all the experimental numbers of about 160 measurements, which would be as "appalling" to the readers of NATURE as Mr. Schwendler's formulæ are to the writer of the review. A reflecting galvanometer with two coils was used. Connected consecutively their resistance was 5590 units and parallel 1405. The results would have been more striking if the coils had been of unequal resistance so that the parallel resistance would have been less, or if the two halves of the bobbins could have been connected parallel, which would have reduced the resistance to about 700 units. The following table will show the results obtained on measuring three resistances with varying branch resistances, and with the two arrangements of the galvanometer the deflections of which were noted when a certain alteration of the comparison coil was made:—

Resistance tested. Units.	No. of cells used.	Resistance of branches. Units.	Alteration of comparison coil. Units.	Deflection of spot of light with galvanometer coils: Consecutive.	Parallel.
4740	20	10	20	14	15
—	—	100	—	91	95
—	—	1000	—	192	190
760	20	10	4	23	39
—	—	100	—	140	234
—	—	1000	—	333	386
90	1	10	2	47	93
—	—	100	—	77	148
—	—	1000	—	77	121

These numbers show that the branches should approximate to the resistance measured and also that the galvanometer resistance should be smaller when a small resistance is measured. Calculation shows that the most advantageous resistances of the galvanometer in the three cases would be 2870, 880, and 95 respectively.

There is another point with regard to testing with Wheatstone's Bridge, which is not noticed in the review, but to which I may be allowed to direct attention;—that is, the position of the galvanometer. It is not indifferent in which diagonal of the bridge the battery and galvanometer are placed when the branches are unequal. In such a case the method is much more delicate when the galvanometer is placed in the diagonal joining the junction of the two largest resistances with the junction of the two smallest. As, I believe, we have in this laboratory the only Wheatstone's bridge yet constructed after Mr. Schwendler's design, by which the position of the galvanometer and battery can be altered by the shifting of four plugs, I have made a few tests which will show the advantage of this arrangement.

The diagonal joining the junction of the branches with the junction of the comparison coil and the resistance measured is called *m n*; the other diagonal being *p q*.

Resistance measured. Units.	No. of cells used.	Resistance of branches. Units.	Alteration of comparison coil. Units.	Deflection of galvanometer in diagonal *m n*	Deflection of galvanometer in diagonal *p q*
		$\frac{b}{a}$			
90	1	$\frac{100}{100}$	2	148	147
—	—	$\frac{100}{1000}$	20	53	122
—	—	$\frac{10}{1000}$	200	$7\frac{1}{2}$	$34\frac{1}{2}$

It will thus be seen that when the branch resistances are equal it is indifferent in which diagonals the galvanometer and battery are placed; but this is not the case when branch *a* is greater than branch *b*. It is hardly necessary to observe that in a practical test more than one cell would be used when the branches are unequal, in order to obtain much larger deflections, and more accurate measurements. HERBERT MCLEOD

Royal Indian Engineering College, Cooper's Hill, January 6

The Unseen Universe—Paradoxical Philosophy

THE principle of continuity forbids us to imagine that the collocation called the atom has existed as it is from all eternity. This the authors of the "Unseen Universe" have insisted upon, and I need not go further than their title-page to remind Mr. Hallowes that in like manner they do not contemplate a future eternal existence for the atom.

But this principle cannot tell us what was the exact nature of the thinkable antecedent of the present universe, nor can it tell us the exact nature of that state which will follow the disappearance of the present system. There are, however *strong scientific analogies* which lead us to believe that the thinkable antecedent of the present system was a spiritual unseen, which not only developed but which now sustains the present order.

Is it therefore necessary that I myself should in like manner help to sustain some inferior universe? I repudiate any such obligation. I am not fit for it.

Because a little boy has a father, is it logically essential that he should likewise have a son?

HERMANN STOFFKRAFT

Schloss Ehrenberg, Baden, January 11

Molecular Vibrations

IN NATURE, vol. xix. p. 158, col. 2, is the following:—

"It has been suggested that the same molecule may be capable of vibrating in different ways, and thus of yielding different spectra, *just as a bell may give out different notes when struck in different ways*." It is well to note that the bell as a whole gives but one sound, and the other sounds are not true harmonics, but come from parts of the bell, either before the whole is in vibration or from parts badly amalgamated, flaws in the metal, air-bubbles in pouring into the mould, lack of homogeneity, inequalities in the mould, &c.

The noises in a belfry are most discordant, whereas harmonics form a succession of consonances—octave, fifth, fourth, major and minor thirds, seventh and treble octave.

WM. CHAPPELL

The Electric Light

WHILE so many experiments are being made on lighting by the incandescence of infusible materials produced by electric currents, it is well to point out that Dr. Draper, as early as 1844, used a strip of platinum so heated to determine the facts that all solid substances become incandescent at 977° F., that light increases in refrangibility and intensity, and that the order of the colours emitted followed the true prismatic order as the temperature increases.

Dr. Draper says: "Among writers on optics it has been a desideratum to obtain an artificial light of standard brilliancy. The preceding experiments furnish an easy means of supplying that want, and give us what might be termed a 'unit lamp.' A surface of platinum of standard dimensions raised to a standard temperature by a voltaic current will always emit a constant light. A strip of that metal one inch long and $\frac{1}{20}$th of an inch wide, connected with a lever by which its expansion might be measured, would yield at 2,000° a light suitable for most purposes. Moreover, it would be very easy to form from it a photometer by screening portions of the shining surface. An ingenious artist would have very little difficulty, by taking advantage of the movements of the lever, in making a self-acting apparatus in which the platinum should be maintained at a uniform temperature, notwithstanding any change taking place in the voltaic current." (*Vide* Draper's "Scientific Memoirs," p. 45.)

Wimbledon, January 11 W. H. PREECE

Force and Energy [1]

III.

IN consequence of energy not being a directed quantity we come at once upon an important distinction between transference of energy and transference of momentum. There may be a large force exerted, *i.e.*, a large amount of momentum rapidly transferred, without there being any accompanying transference of energy. In the distance V on the two sides of a given section of the stressed material through which the two opposite streams are flowing, there is lodged a certain amount of motion which is the same in the one portion on the one side of the section as in that on the other side. The momentum and the energy lodged in each portion are simply different functions of one and the same motion. In unit time the whole of the motion in the portion on the one side of the section is transferred into the portion on the other side, and *vice versâ*. The resulting quantitative transference of the one function of the motion is double what would take place if only one, instead of two, opposite streams were flowing through the section, the reason being that this function is a directed quantity. The resulting quantitative flow of the other function of the motion is zero, because it is a function which has no direction. The rate of transference of momentum, or the force, is in this case eE, the sign being given by the sign of e. Suppose, now, one only of these streams of motion to be flowing past the section, the rate of transference of momentum being $\frac{1}{2}eE$, where e is the geometrical ratio of extension, or the strain. The rate of transference of energy remains to be calculated. The material may be either at rest or in motion. In fact whether it is to be considered at rest, or at what velocity it is to be considered moving, depends altogether upon the set of bodies relatively to which the motion is to be measured. Its relative velocity may also be either uniform or variable. The relative velocity of the centre of inertia of the material lying between two given sections will be uniform if the whole of the motion measured in any quantitative way flowing in through one of these sections is equal to that simultaneously flowing out at the other.

Suppose that before the force begins to act there is a uniform velocity, v_0, throughout a given length. As soon as there is a uniform force, $\frac{1}{2}eE$, throughout this whole length, the flow being only in one direction, one half the particles will have at any instant the velocity, v_0, while the other half has the velocity $(v_0 + v)$, where $v = e\sqrt{\frac{E}{\mu}}$.

$V = \sqrt{\frac{E}{\mu}}$ being the velocity of stream-flow; there is in the length V lodged an amount of momentum $(V\mu v_0 + \frac{1}{2}V\mu v)$ for unit section throughout that length. Of this amount $\frac{1}{2}V\mu v = \frac{1}{2}eE$ is transmitted forwards per unit of time. The mean velocity of the material is also $(v_0 + \frac{1}{2}v)$.

[1] Continued from p. 219

The amount of kinetic energy lodged in this length measured relatively to the same set of bodies as v_0 relates to, is

$$\left\{\tfrac{1}{2} \cdot \tfrac{1}{2} V\mu \cdot v_0^2 + \tfrac{1}{2} \cdot \tfrac{1}{2} V\mu \cdot (v_0 + v)^2\right\},$$

of which there is transmitted forwards per unit of time the amount

$$\tfrac{1}{2} \cdot \tfrac{1}{2} V\mu\left\{(v_0 + v)^2 - v_0^2\right\} = \tfrac{1}{2} V\mu v\left\{v_0 + \tfrac{1}{2}v\right\} = \tfrac{1}{2} eE\left\{v_0 + \tfrac{1}{2}v\right\}.$$

This is equal to the force multiplied by the mean velocity of the material. The truth of this last proposition is quite independent of which group of bodies the velocities are measured relatively to. The energy transferred is to be measured relatively to the same group as that to which the mean velocity of the material is measured. But the expression for the rate of transference of energy consists of two parts, only one of which varies with the choice of axes of velocity-measurement. Thus the acting-force $= \frac{1}{2} eE =$ rate of transference of momentum; the mean velocity of the material $= v_0 + \frac{1}{2} e \sqrt{\frac{E}{\mu}}$; and the rate of transference of energy $= \frac{1}{2} eE \left\{ v_0 + \frac{1}{2} e \sqrt{\frac{E}{\mu}} \right\} =$ rate of doing work. The sign of the last rate indicates simply in which direction energy is flowing. The sign depends on that of the mean velocity and on that of e. Here e is a linear strain, and must have the sign $+$ or $-$. If it is a twist it should have the sign $\sqrt{-1}$ or $-\sqrt{-1}$.

The constant part of the energy transferred—that part independent of the axis of reference—is $\frac{1}{4} e^2 E \sqrt{\frac{E}{\mu}}$. This and the amount of the acting force cannot be altered in any way by varying the choice of axes. This result at first sight seems somewhat contradictory to the notion that energy is a thing of infinitely greater objective reality than force is. The amount of the momentum of a body's visible motion and the amount of its energy can be made just as great or as small as we please, by simply imagining one or other group of bodies to be at rest. In this way its momentum may be made to vary at will from $-$ infinity to $+$ infinity, while its energy may be made to vary from zero to $+$ infinity. Etymologically the words "force," "momentum," and "energy" are mere names, but the first, force, has objective reality in the sense that it is related only to the fundamental units of mass, space, and time, and does not depend at all upon an arbitrary choice of axes; while the second and third, momentum and energy, are simply products of the imagination.

The first of these statements, viz., that respecting the physical reality of force in the sense above explained, may be objected to because of the appearance of velocities in the expression for it $\frac{1}{2} eE = \frac{1}{2} V\mu v$. But here the first velocity $- V$ is the length of material passed through by a wave of longitudinal momentum in unit time, and it is an experimental fact that this is quite independent of the velocity of the material measured relatively to no matter what set of axes. The second velocity v is double the mean velocity of the material after the force has begun to act, measured relatively to a set of axes, relatively to which the material was at rest before the force began to act. Thus, v may be looked upon as containing in itself the definition of the axes relatively to which it is to be measured, and thus its magnitude is not at all at the disposal of our imagination.

Similarly the rate of transference of energy measured relatively to a set of axes with respect to which the material was at rest before the energy began to be transferred, is absolute in the sense that we cannot arbitrarily alter its magnitude by an exercise of the imagination.

We have in the above supposed a single stream of motion flowing continuously onwards and through the material under consideration, so that that material neither gained nor lost on the whole momentum or energy. If the portion of material considered does not pass on the whole motion it receives, but retains either a part or the whole of it, its rate of gain of energy is to be found by applying the above equations to its one or two or more surfaces, or surface layers, through which transfers of energy are going on. If it is receiving energy only through one surface and losing it through no surface, its rate of gain of energy is $\frac{1}{2} e\mathrm{E}\left\{v_0 + \frac{1}{2} v\right\}$, where e is the strain at the receiving surface, and $(v_0 + \frac{1}{2} v)$ the velocity of that surface, measured in the proper directions. It is to be observed that new finite increments of velocity gradually spread over the whole material, so that each small part is accelerated by fits and starts, and the whole mass is accelerated by what might be called pulsations, or, in the case of the strains being shearing ones, in a sort of wriggling fashion. The surface particles have at any instant the velocity v_0, say. They instantaneously gain the velocity v and immediately afterwards lose it again, and experience this change a great many times for an interval during which their time-average velocity is $(v_0 + \frac{1}{2} v)$. After this interval they for another equally long interval alternate between the velocities $(v_0 + v)$ and $(v_0 + 2v)$ in such a way that their time average velocity is $(v_0 + 1\frac{1}{2} v)$.

The gain of energy in one unit of time is in magnitude evidently dependent on v_0: that is, on the axes of reference arbitrarily chosen. Thus, not only can we alter the magnitude of the energy resident in a body arbitrarily by choosing different sets of axes, but, by a simple exercise of the imagination, we can set the energy possessed by any portion of the universe increasing at any arbitrarily desired time-rate. The momentum may be imagined what we like, but we cannot exercise our imagination upon its rate of transference, or force; on the other hand, both the amount at any time and the rate of transference of energy we may make what we please. This last, however, does not at all invalidate the conservation of energy as a proposition concerning the energy measured relatively to a given set of axes; because, although the time-rate of gain of energy of one portion of the collection of bodies investigated may be increased by changing from one set of axes to another, still that change creates simultaneously a correspondingly increased rate of loss of energy in another part, namely, that other part from which the energy is being transferred to the former.

It is to be observed that this change arbitrarily accomplished in the magnitude of rate of exchange of energy is only possible if a force is acting. If no force is acting, e is zero, and the rate of exchange of energy is zero, whatever v_0 be.

This mathematical possibility of altering, by a change of motion-axes, the time-rate of gain of energy of any special portion of the system, seems to me to furnish the strongest conceivable argument in favour of there being existent no other kind of energy except that of motion, *i.e.*, kinetic energy, represented algebraically by the formula $\frac{1}{2}\mathrm{MV}^2$. If the conservation of energy is true in any sense which will include kinetic energy as part of the energy which is conserved, and if the rate of transference of energy from one part of the system to another can be altered by arbitrary changes of the velocities effected by choosing different axes, then there can be no energy that is not energy of relative velocity.

Comparing the kinds of reality to be ascribed to "force and to "energy," we see that while force has quantitative definiteness quite independent of the stand-point arbitrarily assumed by the physical imagination in viewing them, it lacks that kind of reality which some believe to be an attribute of those things only which are "conserved," because force comes into existence and goes out of it again. This kind of reality may be more or less aptly illustrated by supposing that the personality of a human being be not immortal but to come into existence either gradually or suddenly with the birth of the human being, and to go out of existence with its death. If this were the case, and if the results of the temporary existence of this human being were always to live in the subsequent history of human phenomena, then force would have very much the same sort of reality as the personality of a human being. On the other hand, the quantity of energy that exists depends on this standpoint of the imagination, but so long as this standpoint is unchanged there is no change in the amount of the energy. In other words it is "conserved." So long as the position from which it is viewed is not shifted energy can neither be created nor destroyed. To make for energy an illustration somewhat parallel to the above made for force, suppose that all mankind had agreed upon a certain unit of goodness, and that the Deity was a thing the amount of whose goodness, measured by this unit, was really dependent upon the characters of the philosophies believed in by different sets of men, or upon the characters of the men themselves, then the beneficence of the Deity would be constant so long as the philosophic stand-point from which he was considered remained the same, and would have no other kind of constancy. If this were the case then energy would have very much the same kind of reality as the Deity. Again, momentum is conserved in the same way as energy. Also, force being the rate of transference of momentum, the real existence of force implies also the real existence of motion, of which energy is

simply one of various possible algebraic function; that is, of which energy is one of various possible quantitative measures, and of which momentum is another such measure. But although the reality of force implies the reality of energy and of momentum, the absolute quantitative definitiveness of force does not imply any corresponding quantitative definiteness of energy or of momentum. Now physics is distinguished from metaphysics by being essentially quantitative. It appears, then, that force is a physical reality independent of relation to axes of reference, and that energy and momentum become physical realities only when they are referred to such axes, because when not so referred, they have no quantitative definiteness. They remain, however, when not referred to axes, what may be called non-quantitative realities, and probably many people would choose to call them on that account metaphysical realities.

In conclusion I may offer one remark not strictly bearing upon the subject of this letter, which is the proper PHYSICAL use of the words force and energy, but which was suggested during an explanation of the above definition of force to a friend. There are some minds so constituted that they cannot get on at all without continually referring to metaphysical ideas. This fact should make those whose minds are not so constituted unwilling to believe, as they are very apt to do, that metaphysics is only an unreal, improper, and injurious phantasy or disease of the brain. If there are two such real sciences as metaphysics and physics, in the first place it is clearly advantageous to avoid confusion of the two as far as possible, and we may hope to be able keep them separate from the top down to the base where they rest together, or one upon the other. If there are certain words which it is very convenient to use in both these sciences and with accuracy, it is clear that they must have different definitions, *i.e.*, different meanings in the two. But it would be unfortunate if there were no correspondence between the two meanings. If the two sciences are realities they must consist in two different methods of assimilating as part of our knowledge the same facts; and the statements of the one science ought to be capable of definite translation into the language of the other. And this ought to be held in view in arranging the nomenclature of the two. Now I think that the strictly physical definition of force I have given, viz., the time-rate of transference of momentum, has a true correspondence with the ordinarily accepted metaphysical idea of force as "the cause of the change of velocity in masses." Metaphysically the cause of the acceleration of momentum of the one body is the transference of momentum from the other body, and this transference is also the cause of the retardation of momentum of the other. In the physical definition quantitative accuracy is obtained by introducing the idea of the "time-rate." In a metaphysical definition quantitative accuracy is neither possible nor is it desired, the inherent difference between metaphysics and physics being that the latter is quantitative while the former is not so. The friend to whom I threw out this hint objected that I was here only going one step further back, and that the question became "what was the cause of the transference of momentum?" It was evidently he who had made the step backwards, and of course it was a metaphysical step, not objectionable in itself, but having no bearing on the matter in hand. The above question is no objection to the metaphysical statement or definition, that the cause of the acceleration of momentum is the transference of momentum. If metaphysics is fit to do anything at all it ought to be able to investigate the cause of a cause; but even if it were not able to follow the chain of causes beyond any certain point, that would not constitute any objection to the statements of causative sequence made in following along the chain to the possible limit. The metaphysical answer to the question, "What is the cause of transference of momentum?" would probably be different according to the circumstances of the transference, whether it were by impact or by gravitation, or otherwise. To show, however, that my physical definition of force has a true correspondence to the metaphysical idea, it is quite unnecessary to answer this question, it is unnecessary to go beyond the cause which is called "force" in metaphysics.

ROBERT H. SMITH

Absorption of Water by the Leaves of Plants

I FEEL sure that many of your practical readers will be pleased with the article in NATURE, vol. xix. p. 183, on the "Absorption of Water by the Leaves of Plants," as a correction of a fallacy long held by many physiological botanists in antagonism to the experience of plain observers of nature.

In reference to the concluding remark on the statements of Prof. Calderon, the following may perhaps be interesting.

Every botanist who visits my Sewage Farm is struck with the luxuriance not only of the cultivated crops, but with that of weeds found growing, out of reach of the hoe, on hedge-banks and places whence it is impossible for their roots to reach the fertilising stream, which readily accounts for the growth of the crops.

It seems clear, therefore, that plants can absorb nitrogenous organic matter which may be wafted over their leaves by winds from a sewage-irrigated field, and I welcomed Mr. Darwin's account of insectivorous plants as a confirmation of my theory; but, after all, no one has ever doubted the power of absorbing carbon through leaves since van Helmont's celebrated experiment with the willow, and it can hardly be unnatural to credit plant-life with the power of obtaining another element of nutrition by the same channel.

ALFRED S. JONES

Havod-y-wern Farm, Wrexham

The Formation of Mountains

I HAVE deferred replying to Mr. Fisher's letter (NATURE, vol. xix. p. 172) till I had an opportunity of looking at Maxwell's "Theory of Heat;" but, having done so, I am no wiser, for I do not find the point in dispute anywhere referred to. In the "English Cyclopædia," art. "Heat," I find, however, the following statement: "If we suppose the mass of the earth to have been at any remote period at a very high temperature, the effect of the radiation of its heat through the colder surrounding space would be, to cool first the superficial strata, and successively, *though in a less degree*, the internal strata." This slower cooling of the internal parts of a heated mass seems a necessary result of the "law of exchanges," to which the supposed "more rapid cooling of the interior of the globe than the crust" seems as decidedly opposed.

Mr. Fisher's illustration certainly shows how the centre *might* cool more rapidly than the outside, if heat were not subject to laws, and could set the law of exchanges at defiance. He says: "As the people disperse they move off the more quickly the further they get from the dense mass." This would be true for heat, and exactly corresponds to the quotation given above from the "English Cyclopædia;" but it is inconsistent with Mr. Fisher's statement a little further on, that the numbers in an outer belt "may continue about the same, while those in the central crowd become fewer and fewer." The two things are contradictory; and I still fail to see how the "more rapid cooling of the interior of the earth," limited as it must be to that superficial layer within which the effects of solar heat are confined, can be held to furnish a *vera causa* for the compression and contortion of deeply seated rocks and their upheaval into mountain chains.

ALFRED R. WALLACE

Musical Notes from Outflow of Water

EVERY one is familiar with the sounds produced by water running out through a pipe from the bottom of a vessel, when the water-level has got low. The other evening I witnessed a phenomenon of this order, which has, I think, certain interesting features. Desiring to empty my cistern, and the pipes being frozen, I rigged up a gutta-percha tube siphonwise, and brought the water through it. When the orifice of the tube in the cistern got partially uncovered by the descending water-level, a series of rhythmical vibrations was generated, giving a musical note. The plane of the orifice was about vertical; but notes may be had when it is at any inclination with the horizontal water-surface. The intensity of the notes depends, I believe, partly on the difference of level of the vessels; but I cannot furnish exact data as to this, or the way the pitch is affected by various influences (width of pipe, &c.). Would some one proffer an explanation of the "mechanism" or essential character of the phenomenon?

M.

Shakespeare's Colour-Names

MR. BREWIN's assertion that Shakespeare's "word was doubtless *keen*" (not *green*) in the passage ("so green, so quick, so fair an eye") in "Romeo and Juliet," iii. 5, may be put on a par with his "wonder that the correction was not made long

ago." That alteration was made by Sir Thomas Hanmer, and has been rejected by every subsequent editor, and rightly so. "Green" was a common epithet for the eyes, and examples occur in many of our early poets, from Chaucer to Milton. Dyce quotes from H. Weber (*à propos* of Cervantes), "Green eyes were considered as peculiarly beautiful." We have of Neptune, "Thy rare green eye," in "The Two Noble Kinsmen," v. 1, in a passage attributed by some to Shakespeare. That Shakespeare wrote *green* in "Romeo and Juliet" I think beyond reasonable doubt; and if he wrote *green* he certainly meant *green*, and not *blue:* for in "A Midsummer Night's Dream" green eyes are compared to leeks. In our day violet eyes have the precedence over green eyes, yet I think there is still a kind of fascination in the latter. I leave the eagles to the naturalists. *Ne sutor, &c.*

C. M. INGLEBY

Valentines, Ilford

OUR ASTRONOMICAL COLUMN

A VARIABLE STAR OBSERVED BY SCHEINER IN 1612.—In the last number of the "Vierteljahrsschrift der astronomischen Gesellschaft," Prof. Winnecke examines an observation made by Scheiner, of *Rosa Ursina* notoriety, which appears to involve for its explanation the variability of a star at a past time which of late years has exhibited no fluctuation in brightness. In Scheiner's second work, "De Maculis Solaribus," published at Augsburg in 1612, are several letters addressed to his patron, Welser, one of which, dated April 14, 1612, contains observations of Jupiter and his satellites from March 29 to April 8. (It will be remembered that Scheiner regarded the solar spots as in reality solar satellites, which explains the introduction of notices of the satellites of Jupiter in a work professedly relating to sun-spots.) On March 30 he remarked, in addition to the four known satellites of the planet, a fifth star in the same field of view, not observed on the preceding night. This star diminished to invisibility on April 9. Suspecting a slight proper motion, it was regarded by Scheiner as a *fifth satellite* of Jupiter. From figures showing the position of the star with respect to the planet on March 30 and April 7, it may be inferred that they were in conjunction in longitude on the latter day, with a difference of latitude of 10′, the star to the south. Some years since Prof. Winnecke had calculated the place of Jupiter from Bouvard's table for the date of observation, with the view to identify the star which so soon disappeared, but Leverrier's tables for this planet being now available, he engaged Herr Küstner, one of the students at Strasburg, to compute the position of Jupiter for April 7, 1612, at Paris midnight: the geocentric longitude was found to be 136° 13′ 4″·3, and the latitude + 1° 6′ 52″·7 (differing about 6′ from Bouvard's place); hence the position of Scheiner's star, referred to the epoch of the "Durchmusterung"—1855·0, will be in R.A. 9h. 29m. 21·2s., Decl. + 15° 52′·1, thus identifying the object with a star of 8·5m., which the "Durchmusterung" places in R.A. 9h. 29m. 21·4s., Decl. + 15° 53′·5. There are several observations of this star; it occurs in Lalande's zone, 1796, April 4 (No. 18886 of the reduced catalogue), as 8m.; Bessel observed it twice in 1825, estimating it, on February 24, 8m., and on March 12, 7·8m., and Struve using it as a reference-star for Biela's comet on October 26 in the following year, also rated it 7·8m. Again, it was observed by Preuss with the Dorpat meridian circle, in March, 1833, and noted of the same magnitude, so that during this period its brightness appears to have been constant, and Prof. Winnecke adds that repeated comparisons made by himself during the last seventeen years have not indicated any variation. The close agreement of place identifies the star satisfactorily, and he infers that we have here an instance of a star which, though apparently constant during the present century, was variable in Scheiner's time. Prof. Winnecke remarks upon the interest that would attach to a spectroscopic examination of this object by the possessors of powerful telescopes. Its position for 1880·0 is in R.A. 9h. 30m. 44s., N.P.D. 74° 12′·7. He considers that, notwithstanding Scheiner's inexpressible prolixity, the author of the *Rosa Ursina* does not deserve the severe reproach which he has received at the hands of the astronomical historian, but that he was thoroughly candid in communicating what he had seen, and much acquaintance with his writings has strengthened this opinion.

The unusual phenomenon to which we have adverted appears to have made a strong impression upon Scheiner. who transmitted his observation on the instant to Welser,

THE ZODIACAL LIGHT.—We have already alluded in this column to the very questionable accuracy of a statement so often made in popular astronomical works, that the evening zodiacal light is best seen in these latitudes in March, near the vernal equinox, the inclination of its axis to the horizon being then greater than earlier in the year. Notwithstanding this circumstance, it appears certain that of late years the finest views, or we would say the most conspicuous exhibitions of the zodiacal light have occurred between the middle of January and the middle of February. Many instances of bright displays of the phenomenon during this interval might be mentioned. Thus, on February 6, 1856, Secchi records that the light at Rome was brighter than he ever remembered to have seen it, and of great extent; it was yellowish towards the axis, and while the more conspicuous part of the Via Lactea, in Cygnus, was invisible in a hazy sky at a low altitude, the light was traceable to the horizon; it was slightly curved towards the north, and is described as presenting on the whole "un grande spettacolo;" on this evening, it is added, the rest of the sky was illuminated in an unusual manner. Again, it was in the middle of February, 1866, that Mr. Lassell, during his last residence at Malta, witnessed a remarkable display. He says as he went up to the Observatory the striking brightness of the zodiacal light riveted his attention as never before. It was at least twice as bright as the brightest part of the Milky Way, and fully twice as bright as he ever saw it before, and Mr. Lassell upon this occasion also remarked that its character was quite different to that of the Milky Way, a difference more easily recognised than described; generally it is of a much redder hue. In 1874, in the neighbourhood of London, the most conspicuous displays took place on the evenings of January 14 and 17, and February 18, and in 1875, on January 24, 25, and 30, on the first of these evenings the zodiacal light was surprisingly conspicuous, decidedly reddish, and much excelling any part of the Milky Way. Observations on the position of the apex during these favourable views of late years fully support the conclusion of Prof. Julius Schmidt in his treatise on the phenomenon, published in 1856, that the maximum eastern elongation of the apex falls about the middle of January. Towards the end of March, on the contrary, there is a minimum, according to the Athens astronomer, as regards elongation, breadth, and the inclination of the axis of the light on the north side of the ecliptic.

BIOLOGICAL NOTES

NEW ASIATIC FISHES.—In the *Annals of Natural History* for 1873[1] was given a translation of Prof. Kessler's description of the new sturgeon, *Scaphirhynchus fedtschenkoi*, recently discovered in the Syr Daria or Jaxartes, and a note by Dr. Günther, pointing out the interest attaching to the existence in Northern Asia of a second species of this curious form, hitherto only known from the single species, *S. cataphractus*, of the Mississippi. Recently, however, a second Asiatic species of *Sca-*

[1] "On a Remarkable Fish of the Family of Sturgeons," &c. (*Ann. Nat. Hist.*, ser. 4, vol. xii. p. 269).

phirhynchus has been discovered in the Amou Daria or Oxus by Modest Bogdanoff, and named after the well-known governor of Turkestan, *S. kaufmanni.* This new fish was first described and figured in a Russian work on the Natural History of Khiva, prepared under General Kaufmann's directions some time since, but not yet published—owing, we may well suppose, to General Kaufmann's time being too much taken up with other more important matters. Figures and descriptions of it are given in Prof. Kessler's great work upon the results of the Aralo-Caspian Expedition. The fourth part of this work, published in January, 1877, contains not only full details as to this species, but also of a third Asiatic species of this genus—*S. hermanni*, Severzoff, likewise from the Oxus, without caudal filaments, which, however, is only based upon young examples. As already remarked by Dr. Günther in the note above referred to, the presence in the great Asiatic, as well as in the North American rivers, of this and another peculiar form of the limited group of sturgeons[1] is one of the highest importance in zoological geography. There can be little doubt that species of the genus *Scaphirhynchus* will also be found to occur in the great Chinese rivers, the Yang-tzé-kiang and Ho-ang-ho.

RESPIRATION OF AMIA.—*Amia calva* is a fresh-water fish of the United States. It is abundant in the Mississippi and its tributaries and in the great lakes. It attains a length of about two feet. Mr. Burt G. Wilder has published (*Proceedings* of the American Association for the Advancement of Science, 1877) an account of a series of experiments, which seem very conclusively to show that Amia not only exhales but also inhales air, and that this respiration is carried on by means of its swim (air) bladder. This is so much subdivided, that it will be remembered that Cuvier and others compared it to the lung of some reptiles. Experiments seem to show that the aerial respiration was more active when the water in which the fish lay was imperfectly aerated. The average of twenty-three measurements of the amount exhaled was thirteen cubic centimetres. The exhaled air contained about three per cent. of carbonic acid, and when the fish was fasting it contained at least one per cent. Amia displays great powers of endurance of privation of water. On one occasion a specimen was kept out of water for an hour and five minutes without any apparent discomfort or injury. During most of the time the gill-covers were tightly closed, but there were regular movements of the jaw, hyoid apparatus, and sides of the mouth.

CHILIAN BUTTERFLIES.—We have received a monograph of the butterflies of Chili, by Edwyn C. Reed, printed at the national press at Santiago de Chile. It contains descriptions of some sixty-six species, several of which are described as new, and the monograph is accompanied by three plates. We hope that we may from time to time be able to announce further new contributions to the natural history of this district, so well known by the elaborate "Historia fisica y politica" of Gay.

INSECTS IN TERTIARY ROCKS.—Mr. S. H. Scudder has recently published an account of some very remarkable forms of insects from the tertiary rocks of Colorado and Wyoming. These descriptions form Article xxiv. of the forthcoming vol. iv. of the United States Geological and Geographical Survey. Perhaps the most generally interesting insect described is a fossil butterfly (*Prodryas persephone*), which was found so perfect as to allow of the description even of the scales of the body and wings. It is the first butterfly fossil found in America, and, as only some nine species are known from the well-worked tertiary strata of Europe, it is undoubtedly of great value and interest. It shows a marked divergence from living types. A beetle is described (*Parolamia rudis*) which is rather of an Old World than of a New World type. A fly (*Palembolus florigerus*) is interesting not only as representing a highly-specialised type hitherto unknown in America, but as showing how the semblance of an original vein may be formed in a wing out of mere fragments of distinct veins. Masses of eggs of a species of Corydalites are also described as the first insect eggs found in a fossil state.

ON THE RELATIONS OF RHABDOPLEURA.—Prof. Allman believes that the very anomalous characters of this curious polyzoon genus (*Rhabdopleura*) admit of being derived from the typical confirmation of a polyzoon by certain easily understood modifications. One of the most puzzling of those characters is the apparent absence of a tentacular sheath. He maintains that the endocyst is really represented by the contractile cord which seems to take the place of the funiculus in the fresh-water polyzoa, but with which it has nothing to do. In *Rhabdopleura* the endocyst has receded from the ectocyst, and in its posterior part of the approximation of its walls, and the consequent nearly complete obliteration of its cavity has become changed into the contractile cord. Anteriorly, it spreads over the alimentary canal of the polypide, to which it becomes closely adherent, and here represents the tentacular sheath. Still more posteriorily the endocyst undergoes even greater modification, for the contractile cord becomes chitinised and converted into the firm rod which is found running through the stem and branches of the older parts of the colony, and which still presents in its narrow lumen a trace of the original cavity of the endocyst. The shield-like appendage which is attached to the lophophore is one of the most remarkable features in the genus. G. O. Sars regards it as representing the epistome of the Phylactolæmatous polyzoa, but this view is entirely opposed by the history of its development. Prof. Allman, by tracing its development in connection with that of the polypide has arrived at the conclusion that it is formed as a primary bud from the modified endocyst, and that in its turn it gives origin to a bud of the second order, which becomes directly developed into the definite polypide. The primary or scutiform bud continues for some time to increase in size with the developing polypide which it considerably exceeds, but is at last surpassed by the latter. It never disappears, however, but ultimately remains in the condition of a subordinate appendage of the polypide to which it had given origin. We have thus in the life-history of *Rhabdopleura* an alteration of heteromorphic zooids. The first term, however, in the genetic series, the direct product of the sexual system is as yet wanting, no trace of this system having hitherto been discovered in *Rhabdopleura* (Linnean Society of December 19, 1878).

GEOGRAPHICAL NOTES

M. BRAZZA and Dr. Ballay, the two Ogowé explorers, have arrived in Paris. M. Brazza is now preparing a map showing his discoveries in West Africa. It shows that the Ogowé issues from a large chain of mountains, and is formed by a number of rivulets descending from the high regions. The explorers suppose that a large part of the water filling the bed of the Ogowé issues by subterranean infiltrations from the Congo Basin. MM. Brazza and Ballay are led to this conclusion by the belief that the Congo is to be found on the other side of the range of mountains mentioned. They were unable to make a direct verification of this assumption, on account of the hostility evinced by the native tribes, who are of the most warlike disposition. It was with the utmost difficulty that the French explorers escaped from the hands of these barbarians, whose war-cries, arms, and canoes present striking resemblances to the ferocious

[1] Of the Sturine genus *Polyodon*, or Shovel-nosed Sturgeons, one, *P. folium*, occurs in the Mississippi, and a second, *P. gladius*, in the Yang-tzé-kiang.

blacks fought by Stanley. The Central Section of the Paris Geographical Society has decided to give to MM. Brazza and Ballay the great gold medal for 1879. Some members proposed to give it to Nordenskjöld, but the prolongation of his voyage, owing to his detention in Behring's Straits, was considered sufficient reason to adjourn Nordenskjöld's claims to 1880.

MANY proposals have been made for a new initial meridian to be adopted by all nations, but no satisfactory solution has yet been reached. The present state of things is very confusing, with nearly as many different initial meridians as there are civilised countries. In *L'Exploration* M. de Beaumont proposes the adoption of a zero passing through Behring's Straits and down the Pacific, its antemeridian passing through the centre of Europe and Africa; but, indeed, any universally-adopted zero would be better than the present confusion.

THE capitalists of Liverpool and Manchester, finding so many of the old markets shut against their enterprise, propose making a railway 500 miles long, from Zanzibar to the south end of Victoria Nyanza, to develop the trade of Central Africa. In the speeches on the subject at Manchester great ignorance was shown of the geography and hydrography of the region in question, and if only a desire to develop the resources of Africa is at the bottom of the movement, it is quite unnecessary to spend a million of money on a railway. With the magnificent water-way explored by Stanley and other travellers, and with the help of either Indian or tamed African elephants, the resources of Central Africa could be quite adequately developed for many years to come.

IN the January number of Petermann's *Mittheilungen* Dr. Gerhard Rohlfs gives the results of his search, during his last journey in the Libyan Desert, for the supposed empty river-bed of the "Bihâr-Bilâ-mâ." He discusses the chief references to this supposed extinct river, and concludes from his researches that there is no warrant for placing its dried-up bed on our maps. The same number contains a fine map of the region about the sources of the Oxus, with short explanatory text by Dr. Behm, and a map of North Siberia, showing Nordenskjöld's track from the Yenessei to the Lena. Accompanying the latter are German translations of the letters of various members of the expedition.

A CONTRACT has been concluded by M. Sibiriakoff, of Irkutsk, in Siberia, with the firm of Kockum, whereby the latter are to build him a steamer of 350 tons burden, for the purpose of going to the assistance of the *Vega*. It is expected that the steamer will be ready soon enough to start, fully equipped with provisions, in time to reach Behring's Straits, by way of the Suez Canal, next August, in order to assist Prof. Nordenskjöld and his companions. The vessel will afterwards trade to the Lena, and, if possible, even to the Yenessei.

AN Italian traveller, Manzoni, made a journey of some importance in Yemen, Arabia, in 1877, the results of which appeared in the *Exploratore*. In June last Manzoni commenced a second journey from Aden northwards to Asir and eastwards to Hadramaut. After visiting several places of interest he arrived in Sana, where, according to last reports, we learn from Dr. Behm's summary, he was ill. This exploration is supported by the Italian *Cosmos*.

THE last number of the *Isvestia* of the Russian Geographical Society contains an important paper by M. Maieff, giving an account of his journey last summer to South Bokhara. M. Maieff describes the various *beckdoms* or subdivisions of Bokhara, their productions, trade, and people, their chief physical features and hydrography. He concludes by some important information on the various routes from Guzar to the Amu Daria and Afghanistan.

A LETTER received from M. Oshanin, from Turkestan, announces that he has just returned from his great journey to Karataghin. He has discovered a very fine glacier, which he has called by the name of the late Fedchenko. This is the third locality bearing the name of the traveller: M. Ujfalvy has called the Lake Kutban-kul "Lake Fedchenko," and M. Maieff has given the same name to one of the peaks of Ghissar.

THE GEOLOGICAL HISTORY OF THE COLORADO RIVER AND PLATEAUS[1]

FOR convenience of geological discussion, Prof. Powell has divided that belt of country which lies between Denver and the Pacific, and between the 34th and 43rd parallels, into provinces, each of which, so far as known, possesses structural and topographical features which distinguish it from the others. The easternmost he has named the Park Province. It is characterised by lofty mountain ranges, consisting of granitoid and metamorphic rocks pushed upward and protruded through sedimentary strata, the latter being turned upwards upon the flanks of the ranges and their edges truncated by erosion. The generalised transverse section, on the assumption that the sedimentaries, prior to uplifting, were continuous across the mountains, is that of a broad and extensive anticlinal, sometimes profoundly-faulted parallel to the trend, the sedimentary strata which may once have extended across being removed by erosion. The intervening valleys still retain the entire sedimentary series. This form of mountain structure, with its resulting topographical features, gradually passes, as we go westward, into another type, arising from the decreasing frequency of the greater displacements or differential vertical movements of the earth's surface; but most frequently the dislocation is a combined monoclinal and a fault, or series of faults, with all shades of relative emphasis. The small departure from horizontality amid great general displacement is a strong trait, and justifies the name which has been applied to it by all observers with one accord—the PLATEAU COUNTRY.

West of this province lies a third one—the Great Basin. It is characterised by short, jagged mountain ridges, separated by goodly intervals of barren plains. These ridges are usually produced by the up-lifting of the strata along one side of a fault. Sometimes the faults are multiple, that is, consist of a series of parallel faults, the intervening blocks being careened in the same manner and in the same direction. This repetitive faulting is of very common occurrence. Other modifications, and even different types of structure, are presented; but there is throughout the Great Basin a striking predominance of monoclinal ridges, in which one side of a range slopes with the dip of the strata, and the other side slopes across their upturned edges. The forms impressed upon these masses by erosion are rugged, bristling, and sierra-like, and their peculiarities are aggravated by the fact that before these mountains were brought forth the platform of the country from which they arose had been plicated and the plications planed down by erosion. The Basin is the oldest western land of extensive area. Its final emergence was not later than Jurassic, and may have a still older date.

Between the Plateau and Park Provinces there is no definite boundary. Gradually as we proceed westward from the Parks of Colorado the valleys widen out and expand into a medley of terraces, bounded by cliffs, which stretch their tortuous courses across the land in every direction, yet not without system. The boundary separating the Plateau Province from the Great Basin, on the contrary, is abrupt. In many parts of its extent it seems almost possible to hurl a stone from one province to the

[1] By Capt. C. E. Dutton, U.S. Army, Assistant-Geologist U.S. Survey o the Rocky Mountain Region, under Prof. J. W. Powell, in charge.

other. Still there is a border country where the plateaux take on a type of structure which suggests the Basin type, though never to be confounded with it. Powell has given it the name of Kaibab structure, and through it the Grand Cañon of the Colorado cuts transversely. This structure extends northward from the Grand Cañon more than 250 miles, reaching within 100 miles of the Uintas, or even nearer than that. Between the great faults tabular masses have been uplifted to the average altitude of 11,000 feet, with grand valleys between them.

To the eastward of these high plateaus is spread out a wonderful region. Standing upon the eastern verge of any one of these, where the altitude is nearly 11,500 feet, the eye ranges over a vast expanse of nearly level terraces bounded by cliffs of strange aspect (Fig. 1). They wind about in all directions, here throwing out a great promontory, there receding in a deep bay, but continuing on and on until they sink below the horizon, or swing behind some loftier mass, or fade out in the distant haze. Very wonderful, too, is the sculpture of these majestic walls.

FIG. 1.—Bird's-eye view of Cliffs of Erosion, showing the Shin-ar-Ump Cliffs, Vermilion Cliffs, and Gray Cliffs, in order from Right to Left.

Panels, pilasters, niches, alcoves, buttresses, needing not the slightest assistance from the imagination to point the resemblance—grotesque colossal forms neatly carved out of solid rock, endless repetitions of shapes, which pique the fancy to find analogies, are presented to us on every hand, and fill us with wonder as we pass. But of all the characters of this unparalleled scenery, that which appeals most strongly to the eye is the colouring. The gentle tints of an eastern landscape, the rich blue of distant mountains, the green of summer vegetation, the subdued tints of hill-side and meadow—all are wanting, and in their place we behold belts of fierce, staring red, yellow, and toned white, which are intensified rather than alleviated by alternating belts of dark iron-gray. The Plateau Country is the land of cañons. Gorges, ravines, cañadas, are found in every high country, but cañons belong to the region of plateaus. Like every other river the Colorado has many tributaries, and in former times

had many more than now; and every branch and every twig of a stream here runs in a cañon. The land is honey-combed with them. To cross it, except in certain specified ways, is a feat reserved exclusively to creatures endowed with wings. The region is a desert of the most formidable description. A few attenuated streams

FIG. 2.—Grand Cañon of the Colorado (6,200 feet deep).

meander through it, but usually in cañons of which the bottoms are somewhere between the earth's surface and centre. The springs will not average one to a thousand square miles. But in the High Plateaus, at levels above 7,500 feet, we find a moist climate, exuberant vegetation, and hundreds of sparkling streams.

During Cretaceous times, the Plateau Country was a marine area. After 4,000 feet of Cretaceous strata were

deposited, a large portion, and perhaps the whole of this region became, for a time, land, and the uplifting was attended by considerable dislocation and flexing of the strata. In numerous localities the Cretaceous strata are seen to be denuded, and the lowest Tertiary beds lie across the bevelled edges. This uplifting took place after the deposition of a group of beds which in part, at least, are the equivalents of those which King and Hayden have named the Laramie Group. I accept the verdict of Marsh, Meek, King, and Powell, that these beds belong to the local Cretaceous series, and reject the decision of Hayden, that they are Tertiary. Thus the close of the Cretaceous is marked by a physical break separating it from the local Tertiary series by widely distributed unconformities.

After an unknown interval of denudation immediately following the close of the Cretaceous period the region was again submerged, and then began the deposition of that remarkable series of Eocene beds which form such a striking feature in the stratigraphy of the peripheral parts of the Plateau Country. Around the southern flanks of the Uintas their aggregate thickness exceeds 4,000 feet, but southwestward the upper members at length disappear, and seventy miles north of the Grand Cañon only the lower portion of the local Eocene (the Bitter Creek of Powell or Vermilion Creek of King) remain; indeed, in the latter locality no later beds than the Bitter Creek were deposited. The evidence is now conclusive that the Bitter Creek series stretched more than a hundred miles across the Plateau Country, covering, doubtless, its entire extent, while the middle and later Eocene covered smaller areas to the northward. Only marginal remnants of these huge deposits now remain. The heart of them has been eroded and swept away. Just at the commencement of the Tertiary periods the Plateau Country was covered with brackish water, having perhaps an analogy to the Baltic or Euxine, but after the accumulation of a few hundred feet of deposits the region became a vast inland lake. Its northern shore was along the base of the Uintas, which had then apparently gained their first elevation. Its north-western shore, by a coincidence which can hardly be accidental, lay along the identical boundary which now sharply separates the Plateau Country from the Great Basin, and the latter was one of the mainlands which furnished the sediments of the lake. From the angle where the Uintas join the Wasatch it is possible to trace this shore line more than 300 miles south-westward into Arizona with certainty, and to point out its principal bays and headlands, and even to locate the sites of some of the ancient river channels through which the lower Eocene sediments were brought down. The eastern, south-eastern, and southern margins, and the remainder of the south-western margin, remain to be determined by future exploration. At length, after one-third to one-half of the lacustrine beds had been laid down, there began a series of events which has developed the physical features of the Plateau Country, and which has pursued an unbroken course to the present time, and which even yet may not have culminated. Then began that uplifting which has raised the Plateau Country more than 13,000 feet on an average. Then, too, began a marvellous erosion which has cut down the mean level about one-half that amount, leaving the present mean altitude nearly 6,500 feet. At the inception of this process the lake began to dry up, the south-western portion now drained by the Lower Colorado being the first to emerge. Gradually through the succeeding periods the lake contracted its area, withdrawing northward to the Uinta Mountains, where, at the close of the Eocene, it disappeared.

We are now in a position to trace the origin, growth, and history of the Colorado River, if not from the beginning, at least from an epoch near its beginning. Its creation was not the event of one epoch, but a gradual process extending through several periods. The lower course, extending from the mouth of the Virgin to the Pacific, is the oldest portion, and makes its appearance in geological history a little before, but very near, the middle Eocene. Whether it existed before this epoch is not beyond doubt, but probably it did. But earlier than the Tertiary periods it cannot go; for it is certain that up to the close of Cretaceous times the ocean flowed over its track. When the Plateau Country was first isolated from the ocean it became a brackish Euxine, and may be presumed to have had a Hellespont somewhere. It soon after became an inland lake and must have had a St. Lawrence to keep its waters fresh. There can be little doubt that in the middle Eocene the outlet was the lower course of the Colorado. Whether the lake prior to that had some other outlet which it abandoned for this one is an open question, with the probabilities (on general principles) in favour of the negative. But the question is of no great importance.

The growth of the Colorado may be illustrated by considering what might happen to the St. Lawrence if the whole region of the Canadian lakes were uplifted two thousand feet. In no great length of time Ontario would be drained by the St. Lawrence, lowering its channel, and that river would become one with the Niagara. The same process would be repeated at Erie, Huron, and Superior, the lakes vanishing and leaving only a great river with many branches. Such was the origin of the Colorado; first a Hellespont, then a St. Lawrence, then a common but rather large river heading in the interior of a continent. Its principal branch, the Green River, cuts through the Uinta mountains by the Flaming Gorge and Cañon of Lodore. A second lake, apparently coeval with the one we have just discussed, lay to the north of that range and poured its waters through these gateways into the southern lake. What other bodies of fresh water may have been connected with either of these it is impossible to say at present.

At the epoch when the desiccation was completed it is not probable that the cañons had any existence, for the indications are that the elevation of the country at the commencement of the Miocene period was not great. Conditions favourable to cañon cutting are highly exceptional, and there is no évidence that this exceptional combination of conditions existed at that time, while there is much evidence that it did not. That the conditions, however, were favourable to a rapid rate of erosion is highly probable. But the forms which it would produce might be more nearly analogous to those which may be observed in eastern Ohio and western Pennsylvania. That the climate was moist and sub-tropical is rendered probable by the vegetable remains found in the surrounding regions, and it is only rational to suppose that such a climate in a moderately elevated region would yield such results as may be seen in countries similarly conditioned. Whether the valleys were broad or narrow, abruptly walled or gently sloped, matters little. It is almost certain that they were not deep. The great cañons which we now see had not even been commenced, although they were foreshadowed, and the train of events which was to produce them at a later period had started into activity.

The history of the Colorado and its drainage system during Miocene time must be spoken of only in general terms. In truth during this great age there is no evidence of the occurrence of any critical event aside from the general process of uplifting and erosion which affected the region as a whole. The vast erosion of this region has swept away so much of its mass that most of the evidence as to the details has vanished with its rocks. But the more important features of the work, its general plan in outline, have left well-marked traces and these can be unravelled. It was a period of slow uplifting, reaching a great amount in the aggregate, and it was also

a period of stupendous erosion. The uplifting however was unequal. The comparatively even floor of the old lake was deformed by broad gentle swells rising a little higher than the general platform. In consequence of their greater altitude these upswellings at once became the objects of special attention from the denuding agents and were wasted more rapidly than lower regions around them. Here were formed centres or short axes from which erosion proceeded radially outwards, and the strata, rising very gently towards them from all directions, were bevelled off. As the erosion progressed so also did the uplifting of these local centres or axes, thus maintaining the maximum erosion at the same localities. It is a most significant fact that the brunt of erosion is directed against the edges of the strata and not against their surfaces, provided the stratification is but little disturbed. Usually such an uplift will have one diameter longer than another, and we may call the greater the major-axis. The strata dissolve away in all directions by the waste of their edges, and after the lapse of long periods the newest or uppermost strata will be found encircling the centre of erosion at a great distance—the next group below will encircle it a little nearer, and so on.

This has been the history of each of the sub-divisions of the Plateau Country. Upon the western and northern sides of the Colorado five of these centres are now easily discerned. By far the largest and probably the oldest is around the Grand Cañon. All these had their inception in Miocene time, though the one around the Grand Cañon may go back into the upper Eocene. The district known as the San Rafael Swell is by far the best suited for study.

If we stand upon the eastern verge of the Wasatch Plateau and look eastward we shall behold one of those sublime spectacles which fill even the calmest observer with awe and amazement. From an altitude of more than 11,000 feet the eye can sweep a semicircle with a radius of nearly seventy miles. It is not the wonder inspired by great mountains, for only two or three peaks of the Henry Mountains are well in view, and these with their noble alpine forms seem as strangely out of place as Westminster Abbey would be among the ruins of Thebes. Nor is it the broad expanse of cheerful plains stretching their mottled surfaces beyond the visible horizon. It is a picture of desolation and decay; of a land dead and rotten with dissolution apparent all over its face. It consists of a series of terraces all inclining upwards towards the east. We stand upon the lower Tertiary rocks and right beneath our feet is a precipice leaping down across the edges of the level strata upon a terrace 1,200 feet below. This cliff stretches away northward gradually swinging eastward, and finally southward, describing a rude semicircle around a centre about forty miles to the eastward. At the foot of this cliff is a terrace about six miles wide of upper Cretaceous beds inclining upwards towards the east very slightly, and at that distance it is cut off by a second great cliff plunging down 1,800 feet upon middle Cretaceous beds. This second cliff describes a smaller semicircle concentric with the first. From the foot of the second cliff the strata again rise through a width of about ten miles and are cut off again by a third series of cliffs as before. There are five of these concentric lines of cliffs. In the centre there is an elliptical area forty miles long and twelve to twenty wide, its major axis being north and south, which is as completely girt about by rocky walls as the valley of Rasselas, but such walls as Dr. Johnson never dreamed of. We have given it the name of the Red Amphitheatre. Yet, if we look back to Eocene time, we shall find that the whole stratigraphic series, up to the Eocene inclusive, covered this amphitheatre. Nearly 10,000 feet have now gone, and the floor is near, or quite, at the summit of the Carboniferous rocks. At present the Amphitheatre is drained by two streams which cut across it and find their way, one into the Green, the other into the Colorado, below the junction of the Grand.

Still more vast is the erosion which has taken place from the vicinity of the Grand Cañon. Here the Carboniferous strata form now the floor of the country, though a few patches of Trias still remain in the vicinity of the river. But the main body of the Triassic rocks stands now fifty miles north of the river, and beyond them, in a series of great terraces, rise the Jurassic, Cretaceous, and Tertiary formations—the latter capped with immense bodies of volcanic rock. The greater part of the erosion was accomplished in Miocene time.

It will be seen that these local uplifts are important in determining the subdivisions of the area and the distribution of the maxima and minima of degradation. We may see here a correspondence which is worthy of close attention. Those areas which have been uplifted most have been most denuded. I have asked myself a hundred times whether we might not turn this statement round, and say that those regions which have suffered the greatest amount of denudation have been elevated most, thereby assuming the removal of the strata as a cause and the uplifting as the effect; whether the removal of such a mighty load as ten thousand feet of strata from an area of ten thousand square miles may not have disturbed the earth's equilibrium of figure, and that the earth, behaving as a *quasi*-plastic body, has reasserted its equilibrium by making good a great part of the loss by drawing upon its whole mass beneath. Few geologists question that great masses of sedimentary deposits displace the earth beneath them and subside. Surely the inverse aspect of the problem is *à priori* equally palpable. That some such process as this has operated in the Plateau Country looks at least very plausible, and, if there could be found independent reasons for believing in its adequacy, the facts certainly bear it out. Yet its application is not without some difficulties, and the explanation is not quite complete. Granting the principle, it will be still difficult to explain how these local uplifts were inaugurated; and we can only refer them to the agency of that mysterious plutonic force which seems to have been always at work, and whose operations constitute the darkest and most momentous problem of dynamical geology. On the whole it seems to me that we are almost driven to appeal to this mysterious agency to at least inaugurate, and perhaps in part to perpetuate, the upward movement, but that we must also recognise the co-operation of that tendency which indubitably exists within the earth to maintain the statical equilibrium of its levels. The only question is, whether that tendency is merely potential or becomes partly kinetic; and this again turns upon the rigidity of the earth. But it is easy to believe that, where the masses involved are so vast as those which have been stripped from the San Rafael Swell, and from the Kaibabs around the Grand Cañon, the rigidity of the earth may become a vanishing quantity.

Let us turn now to a law which forms a most important link in the chain of discussion—a law without a thorough comprehension of which the structural geologist in the Plateau Country would see very little except Sphinxes, but one which, when he has fully saturated his mind with it, will enable him to translate many mysteries. This law may be called the persistence of rivers. It is a very simple one, but its uses are wonderful; indeed those who have found it so invaluable in the Plateau Country often wonder why so little use has been made of it elsewhere. If the study of this region should accomplish nothing more than drawing this principle from its modest retirement and installing it in its rightful place in the logic of geology it will still have accomplished a great result. But the law has its limits, which we cannot overstep with safety.

Of all the changing features of a continent the least changeful are its great rivers. Undoubtedly rivers have

perished and undoubtedly they have shifted parts of their courses somewhat; but on the whole their tenacity of life is wonderful, and the obstinacy with which they sometimes maintain their positions is in powerful contrast with the instability of other topographical features. This characteristic, however, fails at low levels. A river near its mouth may often change its course; but where the country is high enough to enable it once to fasten its grip it will hold it, despite all the changes to which the surface of a continent is ordinarily subject throughout the term of its secular existence. Its stability and persistence will depend usually upon its altitude, or what amounts to the same thing, upon the rapidity of its slope. When that is small we may look for signs of inconstancy. Other conditions might be formulated which could affect it or modify it; but on the whole the fact remains that rivers have a remarkable power of maintaining their positions. It would be difficult to point out an instance where a great river has ever existed under conditions more favourable to longevity and stability of position than those of the Colorado and its principal tributaries. Since the epoch when it commenced to flow it has been situated in a rising area. Its springs and rills have been among the mountains and its slope has throughout its career been continuously though slightly increasing. The relations of its tributaries have in this respect been the same, and indeed the river and its tributaries have been a system and not merely an aggregate, the latter dependent upon and perfectly responsive to the physical conditions of the former. And now we come to the point. The Colorado and its tributaries run to-day just where they ran in the Eocene period. Since that time mountains have risen across their tracks, whose present summits mark less than half their total uplifts; the river has cleft them down to their foundations. The Green River, passing the Pacific Railway, enters the Uintas by the Flaming Gorge, and after reaching the heart of this chain, turns eastward parallel to its axis for thirty miles, and then southward, cutting its way out by the splendid cañon of Lodore. Then following westward along the southern base of the range for five miles, a strange caprice seizes it. Not satisfied with the terrible gash it has inflicted upon this noble chain, it darts at it viciously once more, and entering it, cuts a great horse-shoe cañon more than 2,700 feet deep, and then emerging, goes on its way. Thenceforward, through a tortuous course of more than 300 miles down stream the strata slowly rise—the river almost constantly running against the gentle dip of the beds, cutting through one after another, until its channel is sunk deep in the carboniferous. Further down, near the head of the Marble Cañon, the Kaibab rose up to contest its passage, and a chasm more than 6,200 feet in depth bears witness to the result. It is needless to multiply instances. The entire province is a vast category of instances of drainage channels running counter to the structural slopes of the country. I am unable to recall a single tributary to the right bank of the Colorado which does not somewhere, and generally throughout the greater part of its course, run against the dips. The northern tributaries of the Grand Cañon have their entire courses thus related. If we were to take the sums of the lengths of the river and its right hand affluents, we should find that at least three-fourths of that total length lay where the streams run against the dips.

It is clear, then, that the structural deformations of the region—the faults, flexures, and swells, had nothing to do with determining the present distribution of the drainage. The rivers are where they are in spite of them. As these irregularities rose up, the streams turned neither to the right nor to the left, but cut their way through them in the same old places. The process may be illustrated by a feeble analogy with the saw mill. The river is the saw, the strata are the timber which is fed against it. The saw-log moves while the saw vibrates in its place. The river holds its position almost as rigidly, and the rising strata are dissevered by its ceaseless wear.

What, then, determined the situation of the present drainage channels? The answer is that they were determined by the configuration of the old Eocene lake-bottom at the time the lake was drained. Then surely the water-courses ran in conformity with the surface of the uppermost Tertiary stratum. Soon afterwards that surface began to be deformed by unequal displacement, but the rivers had fastened themselves to their places and refused to be diverted. This, then, is the key which unlocks for the geologist the vestibule of the Plateau Country. The rivers were born with the country itself, they are older than its cliffs and cañons, older than its great erosion—the oldest things in its Tertiary history; nay, they are its history, which we may yet read imperfectly in their cañon walls. The mountains and plateaus are of subsequent origin. They arose athwart the streams only to be cleft asunder to give passage to the waters. The rivers amid all changes have ever successfully maintained their right of way. Such are the uses of the limited theorem of the persistence of rivers.[1] I shall not attempt to suggest how far it may be applicable to other regions, but I am confident that any geologist visiting the Plateau Country will be quickly overwhelmed with the conviction that it is true there.

In this connection it remains to add something to indicate the magnitude of the work accomplished, and the real extent of the obstacles which the Colorado has accomplished in maintaining its existence. In the Colorado itself, the maximum work has been done at the Grand Cañon (Fig. 2). This chasm is 217 miles in length, to which should be added properly the Marble Cañon above, 69 miles long, since the two are continuous, and their separation merely nominal. The average depth of the Grand Cañon is a little more than 5,200 feet—almost exactly one mile. Its maximum depth through the Kaibab Plateau is nearly 6,300 feet, this depth being maintained approximately as the river runs for about fifty miles. Surely it might be thought that to cut such an abyss is work enough in the life of one river however ancient of days. But the summit of the Kaibab is Carboniferous limestone. When the river began to run in this part the whole local Mesozoic and lower Eocene series rested upon the site of this plateau, but have since been swept away together with a part of the Carboniferous rocks. The river has cut through the entire fossiliferous system of strata and now runs 2,000 feet deep in the archæan. The total thickness of the fossiliferous system here is, or rather was, very nearly 17,000 feet. Hence in its lifetime the river has cut through about 19,000 feet of strata. Through the remainder of the Grand Cañon the total cutting has been from 2,000 to 3,000 feet less. As we ascend the river the amount diminishes—not regularly but with local maxima—until we approach the southern base of the Uintas. The principal branch, the Green River, has cut its channel into the quartzites of this range even more deeply than the Colorado in the Kaibab. Yet strangely enough the instant the Green is clear of the mountains it enters a long stretch where the cutting has been practically nothing. The explanation of this contrast will become obvious to the geologist by a mere reference to the fact that where the cutting has been zero the locality has been always at the base level of erosion, and never above it. Only those parts which rise above the base level are cut down.

(*To be continued.*)

[1] Mr. Jukes employed the same principle in explaining some features in the lower courses of the rivers of Ireland. *Quart. Journ.* Geol. Soc. of London, xviii. (1862), 378, quoted in Jukes and Geikie's "Manual of Geology," Third Edition, p. 454. [But the idea may be found in Hutton's great work the "Theory of the Earth," and in Playfair's "Illustrations." See particularly pp. 102 and 350 *et seq.* of the latter work.—ED.]

THE BRITISH MUSEUM LIBRARY

WHAT sort of reference library can be provided in connection with the natural history collections when they are moved from the British Museum to South Kensington? is a subject now under consideration. It is stated on good authority that, so far as the building arrangements at Kensington go, no provision whatever has been made for library space, and that in the Act passed at the end of last session to enable the trustees to move the collections, a reference library seems to have been entirely overlooked. That Act has, however, been the subject of a resolution by the General Committee of the British Association, requesting the Council to take such steps in the matter as they might deem expedient; and although the resolution had principal reference to the administration of the collections, its force extends equally to such an important matter as a library, should the Council "deem it expedient" to include that subject.

Whatever may be the decision as to what part of the library can be transferred to Kensington, or what ought to be transferred, it is only the works relating to biological studies that will be essential there, and it is only these, therefore, that are likely to be the subject of inquiry. But it might, perhaps, lead to changes of great value to those who use the British Museum Library for the purposes of referring to the literature of science in its other branches as well, if the inquiry could be extended to include the question of the actual state of this literature, which is available for use at the Museum. Whether it should be expected that the national library should contain as complete a collection as possible of scientific publications, or whether those who wish to consult them ought to belong to several of the incorporated learned societies, and use their libraries, is a separate question. When this question is considered, if it has to be considered at all, it must not be forgotten that no one society has anything like a comprehensive collection of scientific works, each society aiming at completeness in its own subjects; that to belong to several societies is not within the means of every student; and that, as one of the advantages of these societies is that members may take books away, no one can be sure of finding on the shelves what they may wish to consult.

But quite apart from such a question as this it would be of great use, with a prospect of effecting changes, to know what is the actual state of the British Museum library as regards scientific literature.

Only those who have had occasion to work at the library can have any idea how incomplete it is in this department, or what a wearisome toil it is, in consequence of the system of cataloguing adopted, to find whether a work they wish to consult is or is not there. If the experiences of those who have had occasion to use the library for such purposes could be collected, the probability is that it would be found that from a third to a half of the works asked for were not obtainable there. This may seem at first sight a very surprising assertion to make, but there is good reason to believe it true. What the Museum does or does not contain can, however, be known only by an inquiry, especially directed to ascertain the facts. A reference to the catalogue, as at present arranged, is quite inadequate to give an answer. The officials themselves could not tell from it what they have and what they have not. For example: suppose a particular volume of the Reports of the United States Geological Survey of the Territories is wanted, a reference to the catalogue will not tell whether it has been received or not. The catalogue simply gives the information that the series is on a particular shelf. If a ticket for the whole series is filled up according to the requirements of the reading-room regulations with the press mark, the title, and Washington, 1873, &c., 4° added, then it will be found when the books are brought to the reader's seat, that only volumes two, six, nine, and ten of the whole series are there. This illustration applies to all publications which are issued in a series either by societies or by government departments. To ascertain, therefore, what is the incompleteness of series of which some numbers find a place in the catalogue, it would be requisite, if a reader undertook such an investigation, to write tickets for every series separately, to have all the numbers brought, and then to make note of the gaps. Such a work is rather the duty of the officials than of readers, but, as already stated, it would require a special inquiry, whether made by readers or by officials, to ascertain what is really the state of the British Museum library as to the literature of science.

It must be borne in mind that an important part, perhaps the most important part, of the literature to which a worker in science wants to refer, is that which is in the series of the different societies and government departments, and it is just in this that the British Museum is weakest, and in which it might be supposed a remedy might be most easily found. To fill up gaps of old standard works out of print is not very easy. Chances of sales of libraries must be carefully looked out for to effect this, but the current literature of societies and of departments is more easily secured.

An inquiry into the state of the scientific literature at the Museum, and the facilities for its use, might be advantageously directed under three distinct heads, each of which has an important bearing on meeting the requirements of those who wish to consult the collection:—

1. As to the incompleteness of series.
2. As to the length of time that elapses between the publication of a number and its being obtainable at the Museum.
3. As to the method of cataloguing.

As regards (1) incompleteness of series, there is no reason to believe that it is confined to publications referring to any particular branches of science more than others. For example, to take a few cases at random, there are only three volumes of the reports of the state of the Brussels Observatory; there is only one part of the long series of reports on the health of the City of London; there are three volumes wanting of the Report of the Commissioners on the Sanitary Condition of the Labouring Population of Great Britain; the publications of the Geological Survey are very incomplete; there are none of the maps of the Water Supply Commission nor of the Coal Commission; and so on. To attempt to give a list of what is known to be wanting would not be of much use for the reason stated above, that nothing short of a full inquiry into the matter could make known what is the real state of affairs. When a question is asked as to why certain volumes are missing, there is always one reply given—the publications of societies, home and foreign, are presented, and cannot be demanded, and as to the publications of Government departments, the Museum has no claim. If they happen to be sent to the Museum they are received, but if not, it would seem that under the existing system there is no help for it.

As regards (2) the length of time before a volume that is sent can be had for reference, it may be safely put at from one to two years. If a question is asked, how it is that such delays occur, a very general answer is that some societies are very irregular in sending their publications, but when such cases as this occur—that at the Museum a reader cannot now have a volume of the *Bulletin* of the Brussels Academy later than 1876, while at another public museum, the Patent Office Library in Southampton Buildings, he can have it up to June in this year—it seems to point rather to some feature in the administration of the Museum as the cause. Many cases of this kind might be quoted if it were required to establish the fact. It is, no doubt, a wise arrangement that novels and magazines that can be seen at any circulating

library should not be available for use at the Museum till a year after publication. But the case is very different with the class of scientific publications now referred to. Of the foreign and colonial publications not many copies of each issue reach this country, and in some cases they can be seen only by the courtesy of an officer of a society that has received a copy. Then, again, not only the amount of interest taken in any particular communication, but sometimes its value, is changed in twelve months. It has been already said that perhaps the question may be raised whether the British Museum is the place to expect to see recent scientific publications, but it would be well if its present state were in any case known.

Then (3) as to the method of cataloguing. The use of the catalogue is of course to enable a reader to find the press mark of the books he wants with the least possible delay. There may be differences of opinion as to the extent to which a catalogue should help a reader, but the facts as regards the British Museum are these. Scientific publications which are not books, magazines, or newspapers, are for the most part grouped under "Academies." The majority of those which do not fall under this head are to be found under the titles of the government departments by which they are issued. In order not to waste time over the catalogue the reader must know certain particulars about the work he wants. If it is issued by a British government department he must know whether it has or not been presented to Parliament. For example, the pathological researches of Dr. Sanderson and Dr. Klein were addressed, through the Local Government Board, to the Lords of the Privy Council; the geological work of the Survey is through the Science and Art Department of the Committee of Council on Education, also under the Privy Council. The pathological researches are, however, presented to Parliament, and the volume containing any particular part of them must be, therefore, looked for under "Parliamentary Papers," while the geological work is not presented to Parliament, and must therefore be looked for under "Great Britain and Ireland—Geological Surveys." In the former case it is requisite to know beforehand in what year the papers were included; in the latter case the memoir to a map may be obtained in this way, but no clue is given as to how to obtain the map itself. (If the press mark for the map is searched for in the map catalogue, cross-references lead to "World—miscellaneous —*see* geographical and geological"). The difficulty of knowing whether a work has or has not been presented to Parliament is sometimes great. For example, some of Mr. Simon's Cholera Reports are included under the Registrar-General's returns and are therefore to be looked for among "Parliamentary Papers;" while the celebrated 1848 Report, which seems somehow not to have been presented, has to be found in the general catalogue under the name Simon, John. This is, of course, quite consistent with the method adopted. As it is with the British so with the foreign publications of departments, it is requisite to know to what department a report is sent. An entomologist may be surprised that to get at some of the United States' publications giving monographs on certain groups, he has to get his press-mark from the catalogue under United States—Department of the Interior—Geological Surveys of the Territories—yet such is the case. And this, too, is quite consistent with the method of cataloguing adopted.

If the work to be consulted is issued by a learned society it will probably be found entered under "Academies." In order to find it in the catalogue the exact title must be known. For example, it is no use to look for a Society of Arts' publication under "Society of Arts." it is requisite to go in the catalogue from "of" to "for" as the full title is "Society for the Promotion," &c. It is also essential to know whether a society has the prefix kaiserliche or kaiserliche-königliche, or königliche, or Imperiale, or Royal, or British, or the title of any nationality or town. It is also requisite to know where the work is published, as the grouping is according to the plan, Academies at so and so. That a reader should first have all this information about a work he wants to consult may be very reasonable, for perhaps the collection at the Museum is too extensive to admit of printing, as the Patent Office library does, a compact and convenient "list of the scientific and other periodicals and transactions of learned societies in the free library."

But it is after a reader has found in the catalogue the title of the society that his real trouble begins. It might reasonably be supposed that the first entry under the name of the society would be the memoirs, transactions, or journal, as the case may be, of the society. That is not the British Museum plan. First are given the press marks of charter, laws, bye-laws, notices of annual meetings, lists of members, and such like things, and page after page has to be turned over to get to the publications of the society. If there are two sets of publications, such as quarto transactions and an octavo journal, these are generally separated by some pages of other references. To take a very familiar case, the memoirs of the French Académie are of course frequently referred to. After the reader has found the right volume of the catalogue containing "Academies at Paris," and has found Académie des Sciences, he will have to look on one page for vols. i. to xi., then, eight pages further on, for vols. xii. to xxiv., and then, further on again, xxv. onwards. It is difficult to imagine what principle is supposed to be followed, or what is gained to a reader by such a plan. If it should happen that the reader does not know that one series of the memoirs contains the communications of members and another series the communications of "Savans Étrangers," he will still have more trouble in obtaining what he wants. Or take an English case. Suppose a reader wishes to refer to an account of a paper communicated to the Ashmolean Society. He will find, under that heading, entries of an account of the Society, old notices of meetings to be held (handbills), rules, &c., but no intimation of whether the Society issues any transactions.

In short, with all the societies, the entries of the regular publications are so mixed up with rules, list of members, bye-laws, &c., that it takes some time, after the right volume and right page have been found, to turn out their press mark. Again, it is not always easy for a reader to know what is classed as an academy and what is not. An account of a communication given before the Royal Institution in Albemarle Street must be sought in the journal catalogued under academies, while one given before the London Institution in Finsbury Circus, though equally a chartered society, must be sought under "London." Or again, how should the records of observations be catalogued? under periodical publications? under academies, or in the general catalogue. The practice differs in different cases.

Were it not for the kind and ready assistance given in cases of need by the reading-room superintendent and his assistants, a reader would be often quite unable to see what he needs.

THE "GRAHAM" LECTURE AND MEDAL

SOME time ago the Chemical Section of the Philosophical Society of Glasgow had under consideration the propriety of raising a fund for the encouragement of original research. The movement soon began to assume practical shape, and in course of time the fund was found to have reached to nearly 300*l.*, the subscribers being chiefly well-known chemical manufacturers and merchants in Glasgow and the west of Scotland. For a time there was

some difference of opinion as to whether the money subscribed should be invested for the purpose of endowing a lectureship or exclusively for the awarding of medals for original research. It was eventually agreed, however, that two-thirds of the fund should be appropriated for lecture purposes and one-third for medal purposes, and it was likewise determined that the medal should bear the name of the "Graham" Medal; and that one triennial lecture should also be designated the "Graham" Lecture, both lectureship and medal being instituted in commemoration of the eminent services of a former citizen of Glasgow and member of the Philosophical Society, the late Thomas Graham, Master of the Mint, so distinguished for his researches in chemistry and physics.

As the scheme is now in such a complete state that it may be announced to the scientific world, we mention a few facts of interest in regard to it.

Through their president, Mr. James Mactear, of St. Rollox Chemical Works, the Council of the Chemical Section have been successful in obtaining from Her Majesty's Mint a valuable die of Prof. Graham, and the authorities of the Mint have agreed to strike the medal free of charge, the Trust Fund supplying the necessary gold for the purpose. The Council intend to award the medal at not less intervals than three years, in order that time may be allowed for papers to be brought forward of sufficient merit to justify them in making an award. It may be remarked that the medals, of not less value than 10*l.*, is to be awarded for the best original investigation in chemical physics or in pure or applied chemistry, which may be communicated to the Philosophical Society of Glasgow, or the Chemical Section thereof, during the three sessions preceding the award.[1] The Council of the Section will make the award, or it may be made by an equivalent body of local chemists of repute, with power on their part to remit the function to the Professor of Chemistry in University College, London, or to the Professor of Chemistry in the University of Edinburgh. Papers in competition for the "Graham" medal, may, we believe, be offered from any part of the United Kingdom; in other words, authors need not necessarily be members of the Philosophical Society of Glasgow, or of its Chemical Section. Dr. William Ramsay, University Laboratory, Glasgow, the present Secretary of the Section, will be glad to answer all inquiries in regard to the matter.

For the purpose of inaugurating the "Graham" lecture scheme in the most fitting manner possible, the Council of the Section have induced Mr. W. Chandler Roberts, F.R.S., the successor to Prof. Graham, as Chemist to the Mint, and for a long time his chief assistant in carrying out his later investigations, to deliver the first lecture, which is announced for Wednesday, the 22nd inst. Of course it is to be delivered in Glasgow. The subject is to be "Molecular Mobility, or some Forms of Invisible Motion," with special reference, doubtless, to the valuable physico-chemical researches instituted by Graham. It is the intention of the lecturer to exhibit and even to use a good deal of the apparatus employed by Graham, and now the property of Mr. Roberts.

"The fitness of things" in connection with the delivery of the inaugural "Graham" lecture is still further shown in the choice and consent of Mr. James Young, F.R.S., of Kelly, to preside on the occasion. There may be many readers of NATURE to whom it is not known that the gentleman just named was, when a very young man, a student in the evening popular classes conducted by Graham in the Mechanics' Institution and Anderson's College, Glasgow, whom he afterwards faithfully served as lecture assistant, first in Glasgow, and then in the laboratory of St. Thomas's Hospital, London. That he benefited by the scientific teachings of his great master is abundantly evident in the fact that he is himself the founder of one of our greatest chemical industries, namely, the manufacture of paraffin and paraffin oils—in a sense, the creation of the last quarter of a century, but already big with scientific and practical results. His devout respect for Graham's memory has since become almost a passion, and it is but proper that he should "assist" at this further effort to commemorate the great scientific triumphs of his teacher, master, and friend.

JOHN MAYER

[1] This is surely a mistake; why any limit?—ED. NATURE.

NOTES

AT the anniversary meeting of the Imperial Academy of Sciences of St. Petersburg, on December 29, 1878, it was announced that Mr. Hind, F.R.S., superintendent of the *Nautical Almanac*, had been elected a Corresponding Member of the Academy. Besides Mr. Hind there are in the list of Corresponding Members of this great Academy the names of Airy, Darwin, De la Rue, Frankland, Hooker, Huxley, Miller, Owen, Sabine, and Sylvester.

WE trust the subject discussed in our first article this week will meet with the attention it deserves in the proper quarter. It is clear that, by almost every civilised government but our own, the vast importance of meteorological observatories at high altitudes is recognised, and the universal value of weather forecasts is now taken as a matter of course. France has her Pic du Midi and Puy de Dôme, America her magnificently-appointed Pike's Peak, and, as our article shows, other countries in Europe have each one or more of these all-important lofty observatories; but, as usual, we are half-a-century behind. How valuable meteorological observations would be to the nation, on one or more of our loftiest Scotch mountains, any meteorologist can tell, and may be seen clearly enough from the article. We earnestly hope the question will not be allowed to subside, but will be persistently urged in the proper quarter as a matter of national importance. It would not take the price of a new gun to found such an institution as is wanted.

THE telegraph wires of Pic du Midi Observatory have been broken again for a number of days. Some anxiety was felt at Toulouse for the safety of General Nansouty, and a rumour spread that the house had been crushed by an avalanche descending from the rock at the foot of which it has been built for protection against the wind. M. Albert Tissandier was sent to reconnoitre with three mountain guides. On January 9, in the morning, he started from Bagnères. On the 10th, in the evening, he arrived at the observatory, where he found that the rumours spread in the plains were unfounded; General Nansouty was taking his readings. On the following morning M. Tissandier took some drawings, and on the 12th he returned to Bagnères. The telegraphic communications with Puy de Dôme were interrupted on the 11th, at the very time when they were restored with Pic du Midi.

A SUCCESSFUL experiment was made the other night at Liverpool Street station in electric lighting, the particular form used being that known as the "Farmer-Wallace." Several platforms were successfully lighted up, and only one or two minor and easily remediable drawbacks characterised the display. By means of a small electro-magnet on the top of the frame, carrying a clutch, the carbons are kept constantly adjusted, without interference. The gas companies have at last determined to show what they can really do in the way of illumination, and give themselves fair play in any comparison with electric lighting. The Phœnix Company, on Saturday night, lit up a part of Waterloo Bridge Road with gas of increased power, on an increased number of lamps, with special burners, in specially-arranged cages. The result was quite sufficient to

cause discontent with the present inadequate illuminating power of gas in street-lamps. The expense is of course greater, but we doubt if it would be so much as the cost of any satisfactory system of electric lighting; and perhaps, rather than run the risk of being relegated to the category of "lights of other days," the companies may, by a little pressure, be made to see the advisability of supplying better gas at even a cheaper rate than the present.

A NUMBER of friends and colleagues of the late Karl von Littrow, director of the Vienna Observatory, have had a medal struck in his memory, which may be obtained at a moderate cost from the publishing firm of Gerold and Co., Vienna. The face of the medal bears a portrait of von Littrow, and the reverse a view of the new Vienna Observatory.

M. DELESSE has been nominated a member of the Paris Academy of Sciences, in the section of Mineralogy, in place of the late Prof. Delafosse.

M. EDMOND BECQUEREL, who has been appointed vice-president of the Academy of Sciences for 1879, will act as president of the Academy in 1880, according to the constant custom. The president for 1879 is M. Daubrée, the celebrated geologist, who was nominated vice-president last year. M. Daubrée has been nominated president of the Central Section of the Geographical Society of Paris for 1879; M. Delesse, the newly-elected member of the Institute, one of the vice-presidents.

OUR readers will regret to hear that Prof. Clifford's health is still extremely delicate; he sailed for Madeira a few days ago, accompanied by Mrs. Clifford, in the hope that the genial climate would lead to improvement. We trust this hope will be realised.

THE Managers of the Royal Institution of Great Britain have decided that the next Actonian Prize shall be awarded to an essay illustrative of the wisdom and beneficence of the Almighty; the subject being "The Structure and Functions of the Retina in all Classes of Animals, viewed in relation with the Theory of Evolution." The prize is 100 guineas, and will be awarded or withheld as the managers shall think proper. Competitors for the prize are requested to send their essays (with or without their names being affixed) to the Royal Institution, addressed to the Secretary, on or before October 1, 1879. The adjudication will be made by the managers in 1879.

THE widow of Faraday died last Monday week.

IT seems to be acknowledged that the readings taken by the electrical instruments kept at Montsouris Observatory are not sufficient to give an accurate idea of the changes in the tension of the air. During the present period, when almost every fall of snow was observed during the night, the readings of the Montsouris Observatory gave no sign of negative tension. We are in a position to state that a self-registering apparatus would have been kept in operation from the beginning of last year, if it had not been required to send it to the Champ de Mars Exhibition for the instruction of visitors. M. Marie Davy is now engaged in putting into operation this apparatus. It should be stated that in 1873 a scientific delegate having been sent to England by M. Jules Simon, then Minister of Public Instruction, suggested that the self-registering instruments to be established in the French observatories, should be constructed according to the pattern adopted at Kew Observatory, so that comparisons should not be rendered impossible. The remarkable conclusions recorded by Mr. Whipple last week are an indication of the soundness of these suggestions.

THE winter has been so severe in France that the whole of the land on January 11 was transformed into a solid mass of ice, communication by rail, and even intelligence by wire becoming very difficult in the elevated parts of the country. The most extraordinary fall of snow recorded in the period was close to Montargis, where it accumulated to a height of 2 metres on a long narrow band of several kilometres long. In the meanwhile the largest rivers of the land overflowed, owing to the great rains which had been prevailing. The Seine reached an altitude of 6·21 metres at Pont Royal, the Loire was higher than on any year on record at Nantes, where the inundation was a public calamity; the increase of the Garonne and Rhône was only stopped by the freezing of the high lands.

AMONG the papers to be read at the forthcoming meetings of the Society of Arts are the following:—February 26, "Indian Pottery at the Paris Exhibition," by George Birdwood, M.D., C.S.I.; March 5, "The Social Necessity for Popular and Practical Teaching of Sanitary Science," by Joseph J. Pope, M.R.C.S.; March 12, "The Compensation of Time-keepers," by Edward Rigg, M.A.; March 19, "Economical Gardens for Londoners," by W. Mattieu Williams, F.C.S.; March 26, "The Treatment of Iron to Prevent Corrosion" (a second communication), by Prof. Barff, M.A. In the Chemical Section—January 30, "Gas Illumination," by Dr. William Wallace, F.R.S.E. In the Indian Section—January 21, "Quest and Early European Settlement of India," by George Birdwood, M.D., C.S.I.; February 21, "The Trade of Central Asia," by Trelawney Saunders. In the African Section—February 4, "The Opening of the District to the North of Lake Nyassa, with Notes of a Recent Expedition through that country," by H. B. Cotterell; March 18, "Some Remarks upon an Old Map of Africa contained in Janson's Atlas, published at Paris in 1612," by R. Ward; April 1, "The Contact of Civilisation and Barbarism in Africa, Past and Present," by Edward Hutchinson. Cantor Lectures—First Course, on "Mathematical Instruments," by Mr. W. Mattieu Williams. The Second Course will be by Dr. W. H. Corfield, M.A., on "Household Sanitary Arrangements;" it will consist of six lectures, to commence on February 17. The Third Course will be by Mr. W. H. Preece, on "Recent Advances in Telegraphy." A course of two lectures will be given by Dr. B. W. Richardson, M.A., LL.D., F.R.S., on "Some Further Researches in Putrefactive Changes," in continuation and completion of his course of Cantor Lectures given last session.

A NEW application of the electric light has just been made by some German River Steamboat Companies. Experiments made on the steamers plying on the Weser and Elbe Rivers having proved perfectly successful these steamers will henceforth be illuminated by electricity.

THE captain of the steamboat *Chillon*, the Geneva correspondent of the *Times* writes, which was caught in the storm on the morning of January 2, describes in a letter to the local papers a scene which is not witnessed once in a generation. On Lake Leman, between Rivaz and St. Gindolph, the two winds the *föhn* and the *bise*, met twisting the water up into a column nearly 40 feet high and 10 yards in circumference. It was a veritable waterspout, and, after retaining its position for several minutes, took the form of a vapoury cloud and melted away. The meeting of the *föhn* and the *bise* is more common on the Lake of Lucerne than that of Geneva; but wherever it happens it is terribly destructive, sweeping down the tallest forest trees and wrecking every craft smaller than a steamer.

A SLIGHT shock of earthquake was felt on Friday last at 3 A.M. at Cologne and Eschweiler. From Buir two shocks are reported—the one at 3.15 and the other at 7.43. The *Neue Freie Presse* announces a considerable shock of earthquake from Unterdranburg, which occurred on the 11th inst., at 10.8 A.M. The Central Observatory at Vienna reports a powerful shock at 10h. 18m. 15s. on the same day observed at Klagenfurt. It

lasted thirteen seconds, and was followed by three slighter shocks. The direction was from south-east to north-west. A report from Eisenkappel gives the same time. The shock observed at Cologne, it will be remarked, occurred on the 10th inst., a day sooner.

INVALIDS will be glad to learn that, amidst the severe weather, a comparatively mild climate exists in a part of their own country. A bouquet arrived from Glengariff on Saturday, the 11th instant, comprising wallflower, primrose, primula, stocks, chrysanthemum, scarlet geranium, arbutus-berries, and a rose-bud, all picked from an exposed garden. In Madeira—but five days' journey—ripe bananas and custard-apples are hanging on the trees. Many of the gayest flowers are in full bloom.

IT is proposed at the next meeting of Russian naturalists to unite all Russian scientific societies into one large association, with zoological, botanical, and physiological sections which would have branches at all the Universities.

THE admirable work by Mr. William H. Edwards on the "Butterflies of North America" has been continued by the publication of the seventh part of the second series, which, like its predecessors, is illustrated by five quarto plates of interesting species, drawn by Miss Mary Peart, of Philadelphia. It is especially interesting from the number of observations made by the author, and his correspondence upon dimorphism and polymorphism of a number of the lepidoptera.

WE cannot help expressing our regret at the almost total destruction of the Birmingham Central Reference Library, with its irreplaceable special collections. We have had frequent occasion to speak of the reports of the Birmingham system of libraries, one of the best anywhere. The building which has suffered disaster was close by the Midland Institute, which fortunately has escaped.

DURING the past summer discoveries of a very interesting series of fossil forests were made by William H. Holmes, of the Hayden (U.S.) Geological Survey. The fact of the occurrence of abundant fossil wood, and in some places of fossil trunks *in situ*, had been noted by former visitors to the Yellowstone Park, but nothing had been learned of the manner in which the forests had been preserved, neither had their great extent been suspected. It is found that an extensive series of forests have been buried in the sedimentary formations of the volcanic tertiary, especially in the region drained by the East Fork of the Yellowstone. From the bottom to the top of the highest cliffs rows of upright trunks may be seen, weathered out and ranged along the ledges like the columns of a temple. Throughout a long period of subsidence a constant alternation of land and sea seems to have been kept up by the irregular deposition of fragmentary volcanic products, so that numberless forests grew and sank, one after another, beneath the sea. Fully 4,000 feet of the tree-bearing strata were formed before the final upward movement began. These same strata now cap some of the highest ranges of the Rocky Mountains, and cover an area of upward of 10,000 square miles. The silicified trunks are in many cases from twenty to thirty feet high, and fairly rival the giant trees of California in dimensions.

IN the *Colonies and India* we find a note respecting the employment of sheep as beasts of burden. In Eastern Turkistan and Thibet, for instance, borax is borne on the backs of sheep over the mountains to Leh, Kangra, and Rampur on the Sutlej. Borax is found at Rudok, in Changthan, of such excellent quality that only 25 per cent. is lost in the process of refining. The Rudok borax is carried on sheep to Rampur, which travel at the rate of two miles a day; but, notwithstanding the superior quality and the demand for it in Europe, the expenses attending its transport seriously hamper the trade, which, but for the sheep, would hardly exist at all.

THE proportions of some principal constituents of sea-water have recently been determined by Herr Jacobsen, from forty-six samples of water taken from the most different regions and at different depths, during the expedition of the *Gazelle*. With regard to carbonate of lime, he obtains an average of 0·269 parts of it in 10,000 parts of sea-water; the minimum was 0·220 parts, and the maximum 0·312 parts. Such differences he attributes mainly to experimental errors, and draws the simple conclusion (not favourable to some interesting biological and geological speculations) that the proportion of carbonate of lime in sea-water varies but slightly. The influence of extensive separation of the carbonate of lime by organisms and that of extensive local replenishing of the water with the carbonate are speedily equalised by ocean currents and obliterated for analysis. One region of the ocean does not afford better life conditions for lime-secreting animals than another (by containing more carbonate of lime), and among the causes from which most of such animals are found on coasts and at comparatively small depths is not to be reckoned that adduced by J. Davy, that in the open sea the carbonate almost wholly disappears. Nor is there ground for Forchhammer's supposition that those animals must be capable of changing the sulphate of lime into the carbonate. The author found but little variations also in the proportions of chlorine and sulphuric acid (the chlorine was somewhat the more constant). The observations in general point to a rapid mixture of the sea-water of different regions by currents both horizontal and vertical.

A RECENT number of *La Nature* gives the following statistics of education in Germany and France:—Of the 86,177 conscripts enrolled in 1877 in the German army 78,622 had received primary instruction in the German tongue, 5,415 in other tongues, and 2,140 or 2·483 per cent. could neither read nor write. The district of Posen furnished the largest contingent of this last category, 11·20 per cent.; then follow Prussia, Silesia, Pomerania, Westphalia, Hanover, Brandenburg, Sleswig-Holstein, the Rhine provinces, Hesse Nassau, and lastly Hohenzollern, of which all the conscripts had received primary instruction. According to the census of 1876 there are, in France, 4,502,894 children of six to thirteen years of age. The number of primary schools is about 71,547, of which 9,352 are absolutely gratuitous. It is reckoned that there are 624,743 children who do not attend any school.

THE new railway bridge across the Lim fjord in the Danish province of Jütland was opened for traffic on December 15.

HERR SCHAPER, an eminent Berlin sculptor, has just finished the model for a bust of the late Prof. Braun, for many years director of the Berlin Botanical Gardens. The bust will be executed in bronze and will be erected in the gardens upon a granite pedestal of 2 metres height.

WE have received parts 1 and 2 of vol. ii. of the *Transactions* of the Watford Natural History Society, containing several papers which show that the Society continues to do good work among the animals and plants of Hertfordshire.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. Wm. Trent; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. Carroll; a Common Marmoset (*Hapale jacchus*) from South-East Brazil, presented by Mrs. Currey; a Triangular Spotted Dove (*Columba guineæ*) from South Africa, presented by Col. F. C. Hassard, C.B.; a Great Eagle Owl (*Bubo maximus*), European, deposited; a Bar-winged Rail (*Rallina pœciloptera*) from the Fiji Islands, purchased.

INDO-OCEANIC RACES

Two papers of considerable interest on the peoples of the Pacific and Indian Islands were read at the last meeting of the Anthropological Institute. The first of these papers, by the Rev. S. J. Whitmee, so long resident in Samoa, was for the purpose of proposing a revised nomenclature of what he calls the Inter-Oceanic races of men. There is much confusion, it is admitted, in the use of geographical and ethnographical names in the Pacific. Polynesia is employed by some for all the intertropical islands eastward of New Guinea. By others it is used for those islands which are east of Fiji, while Melanesia is employed for the southern islands from Fiji westward, and Micronesia for the northern island. Mr. Whitmee proposes that Polynesia should be uniformly employed in the wider signification, and that the different portions be indicated by east, west, and north-west, just as we indicate the parts of a continent.

The term *Inter-Oceanic Races* is used for the people found in Madagascar, Australasia, the Indian Archipelago, Formosa, and Polynesia. In this region there are two classes of people, who may be superficially described as *dark* and *brown*.

The dark people comprise three very distinct races: 1. The Australians, who may bear the name *Australs;* 2. The people found in the Andaman Islands, the interior of the Malacca peninsula, and some portions of the Indian Archipelago, who already have a good name, viz., *Negritos;* 3. The woolly-haired people of Western New Guinea, the Aru, and other islands in the Indian Archipelago, and Western Polynesia. Two names have been used for these—Papuan and Melanesian, and Mr. Whitmee proposes to keep *Papuan* and drop Melanesian. Where these Papuans are somewhat mixed with brown Polynesian blood, they may be conveniently known as sub-Papuan.

The people known as Alfurese in the Indian Archipelago Mr. Whitmee does not regard as a separate people. As used by the Malays, Alfuro appears simply to mean non-Mahommedan and non-Christian—pagan wild men, whether brown or black. Hence Alfuro cannot be used as an ethnic appellation.

The brown people found, from Madagascar, through the Indian Archipelago, in Formosa, in north-west and eastern Polynesia and in New Zealand, Mr. Whitmee regards as having sprung from one stock which had its home in the Indian Archipelago or the Malacca peninsula. For this family he wishes to retain Baron von Humboldt's name, *Malayo-Polynesian;* not because it is the best possible name, but because it is in use and well understood.

There are five branches of this family: 1. Mr. Whitmee believes the first branch which broke off from the parent stock was that which went across the Pacific to Eastern Polynesia and New Zealand. These people probably retain more of the primitive condition of the parent stock than the others, owing to their isolation. But it is almost certain they have to some extent deteriorated from that condition. This race, which does not now possess a satisfactory collective name, he proposes to call *Sawaióri:* this word being compounded from the following representative names, *Sa*-moa, Ha*wai-i*, and Ma-*ori*, following the precedent of the Horsoks of North Thibet, whose name is from *Hor*-pa and *Sok*-pa. 2. A much later migration went westward to Madagascar, and these people bear the appropriate name *Malagasy.* Probably an approximate date of this migration may be fixed by the presence of a few Sanscrit words in the Malagasy language. 3. Mr. Whitmee is unable to express any opinion as to when the *Formosan* migration took place. 4. The latest exodus from the Indian Archipelago was doubtless that which went to north-west Polynesia (Melanesia). For these people he proposes the name *Tárapon*, from Tárawa, in the Gilbert group (used by Mr. Horatio Hall for the language of that archipelago) and Pónape, a representative atoll of the Caroline group. 5. For that branch of the family found still in the Indian Archipelago he proposes to use the generic name *Malayan.* He believes all these people may be included under this term, and that the differences which exist between them may be accounted for by the isolation of some, while others have had a greater mixture of foreign blood, and have been more in contact with external culture and other influences which have changed them since the family has been broken up.

At the eastern end of New Guinea there are mixed people, who may be called *sub-Sawaióri*, or *sub-Malayan*, as their affinities with one or other of these divisions may hereafter prove to be.

The following table shows in compact form the divisions proposed by Mr. Whitmee:—

Inter-Oceanic Races of Men.	Dark Races— ? Negrito-Polynesians	Austral	Australia.
		Negrito	Andaman Is. Samang, &c.
		Papuan	Aru Is. Western New Guinea. Western Polynesia.
	Brown Stock— Malayo-Polynesians	Sawaióri	Samoa. Hawaii. New Zealand, &c., &c.
		Malagasy.	Madagascar.
		Formosan.	Formosa.
		Malayan	Malays of Sumatra, &c. Javanese, &c., &c.
		Tárapon	Caroline Is. Marshall Is. Gilbert Is.

A lively discussion followed, in which Mr. Wallace, Prof. Flower, and Mr. A. H. Keane took part; the two former, while approving of some of Mr. Whitmee's proposed changes, preferred, on the whole, to utilise existing terms. Mr. Keane, in the main, supported Mr. Whitmee's conclusions; indeed, Mr. Whitmee acknowledged his indebtedness to Mr. Keane for several important suggestions contained in his paper.

The second paper, by the Rev. W. G. Lawes, recently returned from a three years' residence at Port Moresby, New Guinea, was an extremely interesting series of ethnological notes on the Motu, Koitapu, and Koiari tribes of New Guinea.

It is extremely important that all statements about New Guinea should be specific as to locality. It is even more important with reference to the people than to the country, the diversities of race and tribe are so numerous. Twenty-five different dialects and languages are spoken, to the writer's knowledge, in the 300 miles of coast extending from Yule Island to China Straits. Port Moresby is the centre of the Motu district, and is in lat. 9° 30′ S. and long. 147° 10′ E. The Motu were fully described by Dr. W. Y. Turner in a paper published in the *Journal* of the Institute, May, 1878. So far as the Motu is concerned Mr. Lawes' paper was simply supplementary.

Great importance is attached among the Motu to the tattooing of the women as a means of enhancing beauty. No importance seems to be attached by them to the pattern. The men are sometimes slightly tattooed, but with them it is a decoration of honour, and shows that the wearer has killed some one.

The taboo system of Polynesia is practised on many occasions and for many purposes.

The spirits of the departed go away to ocean space (their *hades*), and ultimately find their way to the place which is associated in the native mind with plenty and animal enjoyment.

The legend of the Motu respecting the origin of fire is that smoke being seen out to sea, the animals assembled and volunteered to fetch it. The snake, bandicoot, bird, and kangaroo, all started, but came back without it. The dog then went, and succeeded.

The Koitapu are now for the most part to be found living at one end of the Motu villages although preserving their distinctness and separateness. They are also to be found in little groups of a few houses a little way inland, on a hill overlooking the sea all through the Motu district. The typical Koitapu man is slightly darker in colour than the Motu, and the hair is frizzy, not woolly. The principal differences between the Koitapu and Motu are the following:—

Language.—This is essentially different from the Motu and coast tribes. In a vocabulary of 250 words there are only 12 words which have any affinity for coastal or Malayo-Polynesian dialects.

Food and Cooking.—Their bill of fare is more extensive than the Motu, and the mode of cooking different.

Ornaments.—Those different to the Motu are the breastplate and feather head-dress.

Weapons and Manufactures.—The weapons are stone clubs and spears; the bow and arrow is confined to coast tribes. A netted bag and peculiar kind of matting are made by Koitapu, but the knowledge of pottery is confined to coast tribes.

The Koitapu excel in hunting, but the coast tribes are fishermen. The Motu are the conquerors and superior race, but have a superstitious fear of the Koitapu and inland tribes. The

Koitapu are supposed to have power to bewitch and cause disease, also to prevent rain from falling.

The Motu take presents to Koitapu in case of disease, and the women sometimes suck the seat of pain in the same manner as described in Sir J. Lubbock's "Origin of Civilisation," pp. 27, 28. There are many indications that the Koitapu are now but a small remnant of what was once a numerous and powerful race.

The Koiari are closely allied to the Koitapu, and inhabit the mountains at the back of the Motu and Koitapu district. They consist of a number of scattered tribes. They are physically inferior to the Motu and Koitapu, but more numerous. They are small in stature, dark in colour, and dirty in person. Their hands and feet are remarkably small. Their villages are built on the ridge of a hill. Tree houses are common, almost every village having one at a considerable height. Their language is similar to Koitapu. They cultivate the soil carefully, and are great hunters. The women are more degraded than among the Koitapu or Motu, and polygamy is more common.

The dead are laid out for some weeks in the house, and then exposed to sun and smoke until perfectly dry. When the bones fall apart they are collected and tied up in a bundle and hung up in the deserted house or in a tree close by.

The mode of salutation among the Koiari is peculiar. They salute their friends by chucking them under the chin.

They are great chewers of the betel-nut, and are very eager to obtain salt. They barter their produce occasionally with the Motu at Port Moresby for fish, cocoa-nuts, salt, and pottery.

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 11, 1878.—In a valuable paper on the passage of the galvanic current through iron, Herr Auerbach describes experiments with reference to the effect of longitudinal magnetisation of iron bars or wires on their resistance, and to the extra currents at closing and opening of the circuit, explained by a transverse or circular magnetisation. Circularly magnetic iron conducts a current worse, the stronger the circular magnetisation. The resistance of longitudinally magnetised iron may be less or greater than that of unmagnetic; in the former case the resistance-function has nowhere a minimum or maximum; the resistance rises steadily from the state of saturated longitudinal, to that of saturated circular, magnetism; and this is realised in hard steel. In the other case the resistance-function has a minimum for the unmagnetic state. Herr Auerbach explains the effects observed on the hypothesis of rotatable molecular magnets, and indicates the bearing of his views on them on the fundamental laws of galvanism, and the galvanic constants of iron.—In a third series of experimental magnetic researches, Herr Fromme deals with two modes of magnetising a rod with a spiral conveying a galvanic current. It may be inserted in the spiral after the circuit has been completed, and withdrawn while the current is still flowing; or it may be inserted before the circuit is closed, and withdrawn after it is opened. He now obtains a distinct difference, unperceived before, between the effects, and the causes of the phenomena are thought to be not of secondary nature (or very little so), but deducible from the essence of magnetism. The results of experiment are found to agree better with the Neumann-Kirchhoff theory, when the latter of the two above methods is abandoned.—Herr Ritter communicates a first paper of researches on the height of the atmosphere, and the constitution of gaseous cosmical substances. On the two hypotheses of an indifferent state of equilibrium in the atmosphere, and of the oxygen and nitrogen retaining approximately, in all changes of condition, the properties of a so-called perfect gas, he arrives theoretically at a height of 40 km. for the atmosphere, whereas Schiaparelli's observations make it more than 200 km. He removes this discrepancy by supposing that, in the rise of the air-masses, not only aqueous vapour, but oxygen and nitrogen, pass into the state of aggregation of a snow-cloud.—Dr. Kolacek studies mathematically the influence of capillary surface-pressure on the velocity of propagation of water-waves.

SOCIETIES AND ACADEMIES

London

Royal Society, January 9.—"Note on the Inequalities of the Diurnal Range of the Declination Magnet as recorded at the Kew Observatory," by Balfour Stewart, F.R.S., Professor of Natural Philosophy in Owens College, Manchester, and William Dodgson.

We are at present engaged in searching for the natural inequalities of the above range, more especially for any of which the period is between 24 and 25 days. We find strong evidence of an inequality of considerable magnitude of which the period is 24·00 days, very nearly. We have also found preliminary evidence of the existence of two considerable inequalities having periods not very far from 24·65 and 24·80 days. These two appear to come together in about 11 years, but we cannot yet give the exact time of this.

We have not found a trace of any inequality with a period of 24·25 days.

"Some Experiments on Metallic Reflexion," by Sir John Conroy, Bart., M.A. Communicated by Prof. G. G. Stokes, Sec. R.S.

He finds that when light is reflected from a polished surface of gold or copper in contact with various media, the angle of principal incidence diminishes, and the principal azimuth increases with the increase of the refractive index of the medium in contact with the metallic surface; and further, the diminution in the value of the principal incidence appears to be nearly in proportion to the increase of the refractive index of the surrounding medium.

He states that the values of these angles for gold with red light are:—

	Principal Incidence.	Principal Azimuth.
	° ′	° ′
In air	76 0	35 27
In water	72 46	36 23
In carbon bisulphide	70 03	36 48

Assuming that the angle of principal incidence for a metal is the same as the angle of polarisation of a transparent substance, that is the angle whose tangent is equal to the refractive index, the value of that angle in air, as deduced from the measurements made in water and carbon bisulphide by multiplying the tangent of the principal incidence in those media by their refractive indices is 76·53 and 77·22 instead of 76.

"Researches on the Absorption of the Ultra-Violet Rays of the Spectrum by Organic Substances," by W. N. Hartley, F.Inst. Chem., F.R.S.E., F.C.S., Demonstrator of Chemistry, King's College, London, and A. K. Huntington, F.Inst. Chem., A.R.Sc. Mines, F.C.S. Communicated by Prof. G. G. Stokes, Sec.R.S.

The following were the conclusions reached:—

1. The normal alcohols of the series $C_nH_{2n+1}OH$ are remarkable for transparency to the ultra-violet rays of the spectrum, pure methylic alcohol being as nearly so as water.

2. The normal fatty acids exhibit a greater absorption of the more refrangible rays of the ultra-violet spectrum than the normal alcohols containing the same number of carbon-atoms.

3. There is an increased absorption of the more refrangible rays corresponding to each increment of CH_2 in the molecule of the alcohols and acids.

4. Like the alcohols and acids, the ethereal salts derived from them are highly transparent to the ultra-violet rays, and do not exhibit absorption-bands.

In order to ascertain whether isomeric bodies exhibited similar or identical absorption-spectra, a series of benzene derivatives was examined. From the great absorptive power of this class of substances it was found necessary to use very dilute solutions, even though the cells holding the liquids were not more than 0·75 inch in thickness. Curves were plotted by taking the proportions of substances in solution as ordinates, and the position of absorption-bands as abscissæ, and these curves are highly characteristic features of very many compounds. About twenty diagrams have thus been made.

The following is a summary of the chief points of interest appertaining to benzene and its derivatives:—

1. Benzene, and the hydrocarbons, the phenols, acids, and amines derived therefrom, are remarkable firstly, for their powerful absorption of the ultra-violet rays; secondly, for the absorption-bands made visible by dissolving them in water or alcohol, and diluting; and thirdly, for the extraordinary intensity of these absorption-bands, that is to say, their power of resisting dilution.

2. Isomeric bodies, containing the benzene nucleus, exhibit widely different spectra, inasmuch as their absorption-bands vary in position and in intensity.

3. The photographic absorption spectra can be employed as a means of identifying organic substances, and as a most delicate test of their purity. The curves obtained by co-ordinating the extent of dilution with the position of the rays of the spectrum absorbed by the solution form a strongly-marked and often a highly characteristic feature of many organic compounds.

There is a curious feature in connection with the position of the absorption bands; at the less refrangible end they either begin at line 12 Cd or line 17 Cd, and those which begin at 12 end a little beyond 17.

No napthalene or anthracene derivatives have yet been examined, and very few substances of unknown constitution—hence most interesting results may be anticipated from a continuation of this research, and this contribution must be accepted rather as a bare commencement of the subject than its conclusion.

Mathematical Society, January 9.—C. W. Merrifield, F.R.S., president, in the chair.—Dr. J. Hopkinson, F.R.S., was admitted a Member.—The following communications were made to the Society: On a theorem in elliptical functions, by Prof. Cayley, F.R.S.—On a new modular equation, by Prof. H. J. S. Smith, F.R.S.—On coefficients of conduction and capacity of two electrified spheres, by Prof. Greenhill.—On certain systems of partial differential equations of the first order with several dependent variables, by Prof. Lloyd-Tanner.

EDINBURGH

Royal Society, December 16, 1878.—Prof. Kelland, president, in the chair.—The first paper was on the action of light on the iris, by Mr. William Ackroyd. In the paper Mr. Ackroyd suggested certain methods for determining whether the amount of light admitted to the eye had an influence on the pupil or not. Certain of the suggested methods had reference to light emanating from a bright point held close to the eye, while another dealt with rays of light emanating from a bright point at a distance.—The next paper was by Mr. John Aitken, on a new variety of ocular spectrum.—Mr. Alex. Macfarlane, D.Sc., M.A., then read the first half of a paper on the principles of the logical algebra. In it he entered into a minute examination of the principles of the logical calculus, as laid down by Prof. Boole in his treatise on the "Laws of Thought," and advanced a new theory of the operation of the mind, founded upon the analysis of language and the nature of mathematical reasoning.

MANCHESTER

Literary and Philosophical Society, December 10, 1878.—J. P. Joule, D.C.L., LL.D., F.R.S., president, in the chair. On the combinations of aurin with mineral acids, by R. S. Dale, B.A., and C. Schorlemmer, F.R.S.—On the estimation of small excesses of weight by the balance from the time of vibration and the angular deflection of the beam, by J. H. Poynting, B.A., B.Sc.

PARIS

Academy of Sciences, January 6.—M. Fizeau in the chair.—M. Edm. Becquerel was elected Vice-President for 1879.—M. Fizeau gave information regarding the publications of the Academy, and the changes among members and correspondents during the year. The deceased members are Becquerel, Regnault, Delafosse, Bernard, Belgrand, and Bienaymé. Deceased correspondents, Didion, Secchi, Mayer, Malaguti, Leymerie, De Vibraye, De Valdrôme, Shumann, Rokitanski, and Lebert.—The following papers were read:—Reply to M. Pasteur, by M. Berthelot.—On a gigantic isopod from a great depth in the sea, by M. Milne-Edwards. This creature, called *Bathynomus giganteus*, was brought up from a depth of 955 fathoms, on the north-east of the bank of Yucatan, by the American expedition in the *Blake*, which started in December, 1877, under A. Agassiz. It measures 0·23m. in length and 0·10m. in breadth, and is specially distinguished by its respiratory apparatus. This has the form of plumes or tufts from branching stems under the false abdominal feet, which serve as a kind of opercular system. The whole system, including a hollow part in the false abdominal feet, may be injected with coloured liquid. Doubtless the arrangement is specially adapted for the great depth at which the animal respires. The eyes are very well-developed (which would hardly have been expected in a very dark medium). They are each formed of nearly 4,000 square facettes, and, instead of being above the head, as in all errant Cymothoadians, they are lodged under the frontal border, at each side of the base of the antennæ. M. Milne-Edwards places the animal in a new family, which he designates *Branchiferous Cymothoadians*.—On the parallelism of axes of rotation, by M. Sire. The tendency to this is illustrated by a simple apparatus.—On an economical method of bathing adopted in the 69th Regiment of Infantry, by Dr. Haro. Each man takes his place in a tub of warm water, and receives a pulverulent douche of warm water, at the same time rubbing himself with black soap and a brush; then comes a second douche of warm water, then washing with cold water. 80 to 100 men are thus washed daily at a cost of 1 fr. 20 per *séance*, or 0·012 fr. per man.—On the existence of the intra-Mercurial planet indicated by Leverrier, by M. von Oppolzer. He finds (by calculation) the existence of such a planet very probable, but thinks it cannot be identical with any of the two objects observed by Mr. Watson.—Double nebulæ in motion, by M. Flammarion. He suggests that such may be the origin of systems of double stars. Having compared the observations made on 5,000 catalogued nebulæ, he indicates those which show a certain motion, and the nature of it.—On the formation of organic ultramarines, by M. de Forcrand. He obtains such products by heating ultramarine of silver with chlorides or iodides of different alcoholic radicles.—On the separation of ethylamines, by MM. Duvillier and Buisine.—On a new group of silicified stems of the coal epoch, by M. Renault. Completing, in some sort, the observations of Prof. Williamson, he finds among the fossils of Autun a series of types parallel (as regards the growth of the ligneous axis) to the Sigillarineæ, but related, on the other hand, to stems of Cordaites by certain details of structure. This new group he designates *Poroxyleæ*, from the nature of their wood; they present the three types of stem found in Sigillarineæ.—On the disease of the chestnuts, by M. De Seynes. The parasite mycelium forms a superficial and a deep network, which destroy the cellular layers of the root, the richest in protoplasm; the liberian and ligneous fibres are not attacked.—On dental grafting, by M. David. He distinguishes the graft by *restitution* and the graft by *borrowing*. The former consists in reimplantation after extraction, and is resorted to to rectify direction, to treat caries and periostitis easily, or to facilitate operations on some other tooth in the mouth. In twenty-two cases, only one proved unsuccessful. The graft by borrowing consists in substituting a sound tooth (which has had to be extracted from the same or another mouth), for a bad one. Or a sound root from a lower animal may be inserted for a bad one, as base for a pivoted artificial tooth.—On animal grafting in its applications to the therapeutics of certain lesions of the dental apparatus, by M. Magitot. He furnishes some data regarding graft by restitution in the case of chronic periostitis of the top of the root of teeth, &c. His success amounts to about 92 per cent. (sixty-two operations, fifty-seven cures).—M. Delesse was elected member in mineralogy, in room of the late M. Delafosse.

CONTENTS

THURSDAY, JANUARY 23, 1879

GAS VERSUS ELECTRICITY

THE gas companies are at last awakening to the peculiarity of their position, and gas-shareholders are recovering their confidence in the stability of their property. It is interesting to observe how steadily the shares in all the great gas companies have during the last few weeks been rising, and unless any untoward event occurs there is no reason why in a short time they should not recover the position they so singularly lost in August of last year. Looking dispassionately upon the events that have occurred, it is difficult to understand how such a panic and scare could have arisen. Nothing of any sort or kind has been discovered either in the laws of electricity or in their application to electric lighting to account for it. We know no more of the electric light now than we did in 1862, when as great a display was made in our Exhibition of that year as was made in the French Exhibition of last year. There is no doubt, however, that the enterprise of our neighbours on the other side of the Channel in lighting up so brilliantly one of their grand new streets produced a sensation that will not easily be forgotten. Englishmen never like to be beaten. We are accustomed to be startled by inventions from the other side of the Atlantic, but we are not accustomed to be beaten either in commercial enterprise or in inventive skill by our neighbours on this side of the Atlantic. Hence, all of those, whose name is legion, who visited Paris last year came back with exaggerated ideas of the effect of the electric light in the Avenue de l'Opéra, and spread through England a profound opinion of the value of electricity as a means of illumination.

It seems to be forgotten that only three years ago a competitive trial of gas and electricity was made in the clock tower of the Houses of Parliament. Each of these lights were tried for several months, the electric light being a Serrin lamp lit by a Gramme machine; and that, after a very careful examination, gas was successful, was adopted, and is now used by the Office of Works.

Again, it seems to be forgotten that the Elder Brethren of the Trinity House have been experimenting upon this question ever since 1857, and that the results of their experiments have only led to the adoption of the electric light in three of their lighthouses. If the electric light had had the wonderful advantage over gas or oil that its projectors profess for it, surely the governors of such an institution as the Trinity House would have fitted up all the lighthouses upon our coasts with this wonderful light.

The recent experiments, however, have shown both the strength and weakness of the position of the gas companies. Their strength consists in their being in possession of the ground; their weakness consists in their producing only a poor light—and a very poor light—when compared with electricity. But is there any reason why this weakness should continue? Is there any reason why gas should remain such an indifferent light? There is none but that of expense, and expense will not deter people from having a better light if they can only get it. The Phœnix Company has taken the question in hand, and has shown in the Waterloo Road what can be done with gas when the question of expense is not considered. Indeed, it would almost seem, from the experiments that have been made, that the quantity of light to be produced by gas is only a question of the quantity of gas consumed in a given space. There are now burning in the Waterloo Road two brilliant gas lamps, giving a light of 500 candles, and this is greater, in point of fact, than the intensity of the light developed by any one of the electric lights that are now on trial in the thoroughfares of London. There is, however, a defect in gas light which remains to be eradicated, and that is the colour of the light. The one great advantage which the electric light has over gas is that the electric light, owing to its very high temperature, produces rays of every degree of refrangibility, and therefore, as an illuminating power it is equal to that of the sun. But gas light, owing to the lowness of its temperature, is deficient in blue rays, and is therefore not so effective in discriminating colours as the electric light.

A very marked advance towards perfection in this direction in gas lighting has been made in the albo-carbon process, by which the gas burnt is enriched with the vapour of naphthaline—a refuse of gas manufacture. This process is being introduced by Mr. Livesey, and, to judge by the experiments that have been shown, it is very promising indeed. The intensity of the light of a gas burner is improved at least five times, and in some experiments witnessed by the writer the improvement was as much as twenty times.

The tentative trials that are being made with the electric light in London cannot be said to be very successful. That at Billingsgate was certainly a fiasco, that on the Embankment is very brilliant, but we have yet to learn its cost, and there is no doubt whatever that the efficiency of the light is very much less than that usually ascribed to the electric light. The trial on the Holborn Viaduct is not a success. The experiment seems to be conducted by some one who is not experienced in the working of electric circuits, for occasionally all the lamps are found extinguished, on other occasions only a portion of them are burning, and frequently they are very dull. It is quite difficult even at the distance of the Post Office to distinguish the gas from the electric lamp. The same effect is observed on crossing Blackfriars Bridge and looking towards the Houses of Parliament when there is the slightest mist in the air, and it is quite evident that the electric light has no more—if as much—penetrative power than gas.

A most complete and careful inquiry into the working of the electric light has been made by Mr. Louis Schwendler for the East Indian Railway Company, and his results are extremely interesting. He has recommended the introduction of the light into certain railway stations where no gas exists, and the system he proposes to use is the Siemens dynamo-machine and one Serrin lamp, and thereby save that waste which the multiplication of the light unquestionably produces. He proposes to distribute this single light by diffusion on a plan originally suggested by the Duke of Sutherland. His investigation has been conducted in a thoroughly scientific spirit, and when his report is published it will be a very valuable addition to our knowledge of the theory of the electric light. It has

been shown by the writer that the full effect of the current can only be obtained by one lamp on a short circuit, and that when adding to the lamps by inserting more of them on the same circuit, or on a circuit so that the current is subdivided, the light emitted by each lamp is diminished in the one case by the square, and in the other case by the cube of the number of lamps so inserted. Dr. Siemens maintains also the concentration of the power on one light, but other experimenters are endeavouring to partially multiply the light. For instance, M. Rapieff, in the *Times* office, very successfully distributes six lights about the office, and Ladd and Co., with the Wallace form of machine, also distribute six lights over the Liverpool Street Station. Although there is undoubtedly a loss of power in this distribution of the lamps, there may be an advantage in such distribution in cases like printing offices and railway stations. A successful experiment has been made by the British Electric Company in lighting up some of the stations of the Metropolitan Railway Company, and the India Rubber and Gutta Percha Company have been successful in lighting up the London Bridge station of the London Brighton and South Coast Railway Company. In all these cases we have scarcely emerged from the sphere of experiment. The electric light has not yet been permanently introduced on any large scale. Many are trying it, many are captivated by the brilliancy of the light, and many in their eagerness to keep up with the spirit of the age, are introducing it, as, for instance, the London Stereoscopic Company, and the Messrs. Nichols, the clothiers in Regent Street, where, however, the light does not appear to give very great satisfaction through its fluctuation.

We were led to expect very much from the experiments of Mr. Werdermann, but his attempt to subdivide the light seems to have subsided, for we have heard nothing of it for some time past. Again, we have heard no more of M. Arnaud's discovery, and the accounts that reach us from America of the doings of the Sawyer-Mann light, and of the supposed discoveries of Mr. Edison, are unworthy of attention.

The present state of the electric light question may therefore be said to be a tentative one, and the gas companies are with much enterprise now giving their retort courteous by showing that they are in a position—if people choose to pay for it—to give quite as powerful a light as the electric light; and, let us hope, before long that it will be quite as perfect. There can be no doubt that the use of electricity for the production of light is a very wasteful as well as a costly process, for the energy that is generated in the machine is not all consumed in the lamp, but is proportionately distributed over the whole circuit. It is therefore not utilised only in the place where it is wanted, as in the case of gas. If we are using a certain amount of energy in an electric lamp to light a street, we are wasting as much if not more energy in the street in maintaining the current to produce that light.

There are three points which all electric lights for general purposes should be required to attain. The first is a brilliancy far exceeding that of any known lamp; the second is a durability greater than that which would be required for night operations in England; and the third is absolute steadiness, to enable work to be conducted without affecting the eyes. There is no electric light that has yet been introduced which supplies us with these desiderata.

W. H. PREECE

THE "NOVUM ORGANUM"

Bacon's Novum Organum. Edited, with Introduction, Notes, &c., by Thomas Fowler, M.A., Professor of Logic in the University of Oxford. (Clarendon Press, 1878.)

THE writings of Lord Bacon, and especially the "Novum Organum," possess a fourfold interest. They have a direct bearing upon the history of philosophy, literature, logic, and physical science; and whatever estimate we may form of their influence upon each of these branches of knowledge, we think that few will fail to admit that Bacon threw a bridge over that vast and deep gulf which separates the ancient from the modern modes of thought, and directly opened a way to our present philosophy and science. Those who would make him the Founder of a sect, the Inventor of induction, or the Father of experimental philosophy, know nothing of his writings. Many had written against Aristotle before his time, many had advocated the collection of positive facts, and the application of a just induction, but they had offered on their part no system which could replace that of Aristotle. When the Scholastics began to abandon their leader, some took refuge in the meagre philosophies of Ramus, of Telesius, of Aconcio, of Nizolius, of Campanella, and of minor men. But when Bacon gave to the world a vast and definite system, and for the first time pointed out the fallacies of the old methods, and suggested new means of interrogating Nature, the scattered refugees from Scholasticism were glad to unite their forces under his banner.

We must bear in mind at the outset that Prof. Fowler approaches the editorship of the "Novum Organum," from the logical and philosophical, rather than from the scientific side. It is improbable that any one man could combine the very exhaustive knowledge of logic, literature, philosophy, and science, necessary for the complete and thorough editing of the work. The main object on the part of our author has been to show that the "Novum Organum" marks an epoch in the history of logic. At the same time he has by no means neglected the other aspects of the work. He has added copious notes, which from every point of view are admirable. It is only here and there that one detects that some of the notes relating to the scientific matters so largely discussed in the second book, were not written by a man of science. Playfair and Whewell are quoted among the older authorities, while Prof. H. G. S. Smith, Mr. Kitchin, and Prof. Clifton, have lent a willing hand in the elucidation of the more knotty points. The most recent ideas on scientific subjects are introduced: such as the kinetic theory of heat, and the conservation of energy. The liquefaction of oxygen and hydrogen is noticed, although much of the work must have been in type when these discoveries were made. Altogether we have no fault to find with the treatment of the work from a scientific point of view.

There have been wide differences of opinion concerning Bacon's influence on the rise and progress of physical science. While Voltaire and the Encyclopedists on the

one side call Bacon "the father of experimental philosophy," Sir D. Brewster asserts that he had no influence whatsoever on the development of our modern experimental method. As to the most recent attacks—those of Liebig and Tchihatchef—they are based on such a very shallow acquaintance with Bacon's works, and are couched in such a pitiful and contemptible spirit, that they are quite unworthy of notice. The true estimate of Bacon's influence on modern science is no doubt to be found between the extremes of the Encyclopedists on the one hand, and of Brewster on the other. Bacon certainly was not the father of experimental philosophy, but most surely he had much to do with our modern scientific method.

Prof. Fowler discusses the nature of Bacon's influence on the progress of science, under nine separate headings. (1) "He called men, as with a voice of a herald, to lay themselves alongside of nature, to study her ways, and imitate her processes. . . . In one word he popularised science." (2) "He insisted, both by example and precept, on the importance of experiment as well as observation." (3) He thus recalled men to the study of facts; and (4) in order to do this it was necessary to free them from the subjection to authority, to which they had so long submitted. "Nor can I doubt," says our author, "that his utterances on this subject had far more influence in producing the intellectual revolution which followed than the utterances of any one of his predecessors, or perhaps than those of all taken together." (5) "The emancipation of reason from the bewitching enchantments of imagination," which he effected (6) by asserting the claims of "a logic of induction which shall do for the premisses, what the old logic, the logic of deduction, does for the conclusions." (7) "The manner in which he insisted on the subordination of scientific inquiries to practical aims, the furtherance of man's estate, and the increase of his command over the comforts and conveniences of life." (8) The "hopefulness" of Bacon, as regards the future of the human race; and finally (9) "the marvellous language in which Bacon often clothes his thoughts."

Taken in connection with all this, the charges which have been brought against Bacon, as a man of science, appear very trivial. It is urged against him that he did not accept the Copernican theory, and that it was fully accepted more than fifty years before the "Novum Organum" was written; but we must remember that the system was by no means firmly established before the discovery of the satellites of Jupiter in 1609. Prof. Fowler remarks that "it is possible to draw up a long list of eminent men, astronomers and others, anterior to, or contemporary with, Bacon, who adopted and taught the Copernican theory; but we believe there were only ten Copernicans in the world, when the "Novum Organum" began to be written. Moreover, we must remember that the anti-Copernicans could boast the great name of Tycho Brahé, while Riccioli, five-and-twenty years after Bacon's death, pretended in his "Almagestum Novum" to refute fifty-seven arguments in favour of the theory. It has also been urged that Bacon did not fully recognise the value of the discoveries of Galileo. Liebig boldly tells us that he was ignorant of the discoveries of Jupiter's satellites, of the ring of Saturn, of mountains in the moon, of the law of the motion of planets, and of the spots of the sun, while in the 39th Aphorism of Book 2 of the "Novum Organum," we read "Secundi generis sunt illa altera perspicilla quæ memorabili conatu adinvenit Galilæus; quorum ope, tanquam perscaphas aut naviculas aperiri et exerceri possint propiora cum cælestibus commercia. Hinc enim constat, galaxiam esse nodum sine coacerrationum stellarum parvarum, plane numeratarum et distinctarum; de qua re apud antiquos tantum suspicio fuit. Hinc demonstrare videtur, quod spatia orbium (quos vocant) planetarum non sint plane vacua aliis stellis, sed quod cœlum incipiat stellescere antequam ad cœlum ipsum stellarum ventum sit; licet stellis minoribus quam ut sine perspicillis istis conspici possint. Hinc choreas illas stellarum parvarum circa planetam Jovis (unde conjici possit esse in motibus stellarum plura centra) intueri licet. Hinc inæqualitates luminosi et opaci in luna distinctius cernuntur et locantur; adeo ut fieri possit quædam seleno-graphia. Hinc maculæ in sole, et id genus: omnia certe inventa nobilia, quatenus fides hujusmodi demonstrationibus tuto adhiberi possit."

If we compare Bacon's writings solely as regards their scientific aspect with those of the greater number of his contemporaries, we find a decided balance in favour of the former; at the same time it must be admitted that men like Gilbert and Galileo were far in advance of our philosopher, both as experimentalists and as discoverers Among Bacon's experimental achievements we may mention, however, the experiment which simultaneously proved the slight compressibility of water, and the porosity of the densest solids, usually alluded to as "the celebrated experiment of the Florentine academicians." Bacon made use of a sphere of lead filled with water, while the Florentines employed a sphere of silver, but this was the only difference. Bacon's experiment was tried more than thirty years before the establishment of the Accademia del Cimento, and was published ("Nov. Org.," lib. ii. aph. 45) nearly fifty years before Megalotti, the secretary of the Academy, made it known in the "Saggi di Esperienze." Mr. Ellis speaks of this as "perhaps the most remarkable of Bacon's experiments."

We may also mention that Bacon endeavoured (we believe for the first time) to determine the relationship between the volume of a vapour and that of the liquid producing it ("Nov. Org.," lib. 2, aph. 40; also the tractate, "Phenomena Universi"). Furthermore, he determined the specific gravity of seventy-three substances, taking gold as the standard. It is true that the method was clumsy, but the table was, at least, far more extensive than that of any previous writer.

In the "Historia Soni et Auditus" Bacon suggests the method for determining the velocity of sound which was employed with so much success by the French nearly two centuries later; and in the same treatise he compares "visibles and audibles" with great acuteness. Again, in the second book of the "Novum Organum," the inquiry into the nature of heat often displays, not only great observational powers, but an elegant application of logical inference.

All this, and much more, Prof. Fowler has pointed out in his exhaustive notes. His work has been, to a great extent, a labour of love; he has bestowed upon it an infinite amount of care and pains, and he has been unwearied in his endeavours to sift everything to the

bottom, and in giving an opinion to act as a just judge; moreover, he has brought to bear upon every part of it his own logical habit of mind. It will be welcomed as a valuable addition to Baconian literature, and to the history alike of philosophy, literature, logic, and science.

G. F. RODWELL

THE AMERICAN CYCLOPÆDIA

The American Cyclopædia: a Popular Dictionary of General Knowledge. Edited by George Ripley and Charles A. Dana. 17 vols. (New York and London: Appleton and Co., 1873-1878.)

IT was not to be expected that so eminently practical a nation as the United States would be long behind the stereotyped peoples of Europe in so indispensable an article as an encyclopædia. It is indeed many years since such a work was published in the States, and that so recently completed by the enterprising firm of Appleton is really a new edition of what some of our readers may remember as "The New American Cyclopædia." On the very surface the present issue is a vast improvement on the old, with its black funereal covers and unpleasant type. Indeed, the present edition may be regarded as really a new work, brought up to date in all departments. Ten years had elapsed between the completion of the old edition and the commencement of the new, and between 1863 and 1873, advances of vast importance had been made in nearly all departments of science. That Messrs. Appleton made competent provision to take account of these advances is evident from the list of men whose services they were able to obtain in bringing out the new edition. Besides the editors-in-chief, Messrs. Ripley and Dana, and four "associate editors," there was a large staff or "revisers," and a "corps" of contributors containing most of the well-known scientific workers of the States. The organisation of the work of the new edition appears to have been excellent, and from a description of the extensive premises devoted to the staff, it seems to have been a British Museum in miniature, with greatly improved arrangements.

The "American Cyclopædia" can scarcely be compared with any existing Cyclopædia in this country. It is not on so extensive a scale as the "Britannica," but is considerably larger than "Chambers'." It is indeed a kind of compromise between these two well-known works of reference; the information is not so conglomerated into huge articles as in the former, nor is it quite so subdivided as the latter—a feature which renders the latter so satisfactory from a purely "reference" standpoint. The "American" has, however, on the whole, stronger affinities with "Chambers'" than with any other; for while there are longish articles on some of the leading departments, still as a rule the great subjects are broken up into their subdivisions. Thus the article "NATURAL PHILOSOPHY" is little more than a reference to the various departments included under the wide term; under "CHEMISTRY" some of the main principles and data of the science are given, with copious references to subordinate heads. Some of these latter, in the two great divisions of physical science, are treated at considerable length, as AFFINITY, ATOMIC THEORY, HEAT, LIGHT, MAGNETISM, and so on, the last-mentioned having been written by the late Joseph Henry. GEOLOGY is a moderate-sized article by Sterry Hunt, and BOTANY is rather short, with, however, a good bibliography appended; the author's name is not given. Prof. Cleveland Abbe contributes a model article on METEOROLOGY, and many kindred subjects are written by the same able hand. One feature which the "American" has in common with "Chambers'" is the giving biographies of living men, a feature the advisability of which we do not care to discuss. Happily the "American" confines itself mainly to a statement of facts in the life and work of living men; eminence in any direction is sufficient to gain admission to these pages, and all sorts of names will be found therein, from "Boss" Tweed to Charles Darwin.

The geography in this new edition is specially well done, one of the largest and best articles in the work being that on the United States. Japan is well done by Prof. Griffis of Tokio, the language being by Dr. Hepburn, of Tokio, and the literature by Mr. Satow, our Secretary of Legation there. We are glad to find that in most cases where it is desirable, satisfactory bibliographies are appended to the articles. Perhaps one of the most distinctive features of the Cyclopædia is the copious index, occupying the whole of the seventeenth volume, which has been prepared for the whole work. This, indeed, doubles the value of the Cyclopædia as a book of reference. Although, as we have said, the great subjects are, as a rule, subdivided into their leading branches, still, throughout the greater number of articles are incidental references containing scraps of valuable information which can find no place of their own. In this way much useful knowledge would be buried but for a good index, and the index prepared for the "American" by Dr. Conant, is one of the most thorough and best planned we have seen. It covers 800 pages, is simple in its method, easily consulted, and admirably adapted not only to bring out all that is in the work, but to enable any one who might desire it, to follow out any subject to completeness. The bold clear type in which the index is printed adds greatly to its usefulness, and, altogether, it is a feature which those who are in the habit of consulting cyclopædias in earnest will know how to value.

The maps and illustrations in the "American" are, on the whole, faithful and good, and ample in quantity, and the type and paper are excellent. In short, in all the features distinctive of a cyclopædia the "American" will hold its own with any in the Old World. It would no doubt be possible to pick faults in plan and criticise some of the particular articles, but this we are not disposed to do where the work as a whole is so eminently satisfactory. The only objection we feel inclined to make is to the price. The volumes are almost the same size as those of "Chambers'," but each is more than double in price, and not very much less than the price of a volume of the "Britannica." This may have been rendered necessary by the great expenses of preparation, but we doubt if at such a price it would command any great sale here. We are surprised to find that the work is sold, not through the regular "trade," but by what is known here as the "canvassing" system. We should have thought that so high-class a work would not have had to depend on any such system for sale. Of course the articles are mainly

written from the American point of view; but to an English reader this adds little that seems peculiar, and were it not for the price of the work, it might very well be put into the English market. Altogether it is highly creditable to publishers and editors, as well as to American enterprise.

OUR BOOK SHELF

The Fairy-Land of Science. By Arabella B. Buckley. (London: Stanford, 1878.)

THE modest preface which Miss Buckley has prefixed to her attractive-looking volume almost disarms criticism, her desire being, she states, simply to awaken a love of nature and of science, while giving pleasure to young people. In this aim Miss Buckley will, we have no doubt, fully succeed.

The substance of this volume was given as a series of lectures to children last spring, at St. John's Wood, and it is at the request of friends who were then present, that the lectures have been printed. We could wish that there were some one in every town equally gifted in rendering science attractive to young people and thus inciting them to a farther and deeper study of natural knowledge.

It would be easy to find fault with some things in this book if we simply regarded it from the narrow standpoint of the scientific critic, without taking into consideration the aim of the author; but as a reading-book to inspire children with a love for nature, which is all the author claims for it, we do not know of a more interesting nor useful gateway to science. The really admirable illustrations with which the book abounds and the pleasant, light manner in which the author carries her readers along from one subject to another will make the "Fairy-Land of Science" a welcome and useful addition to juvenile literature.

In the opening lecture Miss Buckley introduces us to her fairies, showing how things far more wonderful than those related in fairy-tales are daily happening around us, and also how this fairy-land of science may be entered by any one with eyes and with a wish to use these eyes.

In concluding the series of lectures, after showing how it is but the outskirts of this fairy domain which has been touched, the results of a study of science are thus summed up:—

"Pleasant and happy thoughts may thus be conjured up at any time, wherever we find ourselves, by simply calling upon nature's fairies and asking them to speak to us. Is it not strange, then, that people should pass them by so often without a thought, and be content to grow up ignorant of all the wonderful powers ever active in the world around them?

"Neither is it pleasure alone which we gain by a study of nature. We cannot examine even a tiny sunbeam, and picture the minute waves of which it is composed, travelling incessantly from the sun, without being filled with wonder and awe at the marvellous activity and power displayed in the infinitely small as well as in the infinitely great things of the universe. We cannot become familiar with the facts of gravitation, cohesion, or crystallisation without realising that the laws of nature are fixed, orderly, and constant, and will repay us with failure or success according as we act ignorantly or wisely; and thus we shall begin to be afraid of leading careless, useless, and idle lives. We cannot watch the working of the fairy 'life' in the primrose or the bee, without learning that living beings as well as inanimate things are governed by these same laws of nature; nor can we contemplate the mutual adaptation of bees and flowers without acknowledging that it teaches the truth that those succeed best in life who, whether consciously or unconsciously, do their best for others."

This extract will be sufficient to show the happy way in which Miss Buckley addresses her young hearers and readers. At the same time the author would, in our opinion, have done better had she not attempted to travel over so wide a range of subjects as is embraced in her lectures, for we skip from chemistry to physics, then to meteorology, physical geography, and geology, thence to the life of a primrose, afterwards to coal, then to bees, and finally to the fertilisation of plants. This discursiveness leads to occasional looseness of statement, as, for example, employing the terms positive and negative to express the poles of a magnet; it also causes a slurring over difficulties, as in the attempt to explain the measurement of the wave-lengths of light which, with the subject of diffraction, had better have been omitted in a child's book like the one before us.

More durable and equally interesting information might have been given by selecting some one branch of science, examining carefully a few simple phenomena, and regarding them under various aspects; Faraday's juvenile lectures at the Royal Institution—his lectures on a candle, for instance—are the best illustrations of what we mean.

In the study of nature there are very many statements which a child must take simply on the assertion of his or her teacher, with the explanation that their verification is only possible when the child has grown older and wiser; regions are thus opened up beyond its present powers, and the first lesson in education has been learnt—the consciousness of ignorance. We have no doubt, however, that this lesson Miss Buckley would wish to convey as much as we ourselves.

New Commercial Plants, with Directions for their Growth and Utilisation. By Thos. Christy, F.L.S. (London: Christy and Co., 1878.)

THIS is the second of what is evidently intended to form a series of pamphlets on plants either of entirely new economic interest or those whose uses have been extended or developed or are capable of being developed. It is a matter of notoriety that numerous products of the vegetable kingdom require only to be more generally or better known to become more largely used. New products which reach our markets often fall entirely through, simply for the want of a proper appreciation of their value or of some one to take them up and properly test them. This task Mr. Christy seems to have set himself to do, for in his preface he asks for information upon new drugs or plants, such as notes bearing upon their properties and uses, and what is a very valuable point indeed, he appeals to residents in tropical countries for flowers, leaves, and fruits of any useful plant, all of which can be sent any distance in perfect condition in jars or bottles filled with salt and water. This advice is well worthy of consideration by those in distant lands who have opportunities for sending home such specimens, for it often happens that much time, trouble, and expense are thrown away by sending home specimens in such a manner that they rot on the voyage.

As an illustration of what is a "new commercial" product so far as this country is concerned, but which has been known and used in India for a long time, we may mention the Chaulmugra (*Gynocardia odorata*), a full description of which, accompanied by a figure, is given by Mr. Christy. It is not a little remarkable the rapidity with which the oil from the seed of this tree has become adopted by the medical profession in this country for consumptive and cutaneous diseases. Amongst the other plants treated of in the pamphlet under review are *Urostigma vogelii*, Miq., a new source of india-rubber from West Africa, the Mahwa tree (*Bassia latifolia*, Roxb.), a native of the East Indies, the flowers of which are produced very abundantly and yield a large quantity of spirit.

A glance through the pamphlet will give an idea of what kind of products different parts of the world are yet capable of supplying.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

American Weather

I INCLOSE you a cutting from the *Manitoba Weekly Free Press* of December 14, 1878, containing a record of what I cannot but think is a phenomenon unsurpassed in the annals of meteorology. For a month to exceed its average temperature by the amount of twenty-five degrees is scarcely credible even in such a continental climate as that of Manitoba. An editorial paragraph from the same paper which I also inclose will show that the fact has not been overlooked by the Manitobans, and that their attention has also been drawn to the occurrence of the reverse characteristics in the weather over here. Surely the moral of all this is universal synoptic weather charts. The whole thing at present is worked on far too small a scale. The daily papers contain a weather chart which comprises scarcely a quarter of Europe, and of what goes on outside the limits of this we are practically ignorant, unless we hunt up reports when the atmospheric conditions they refer to are long past. Even granting the impossibility of drawing the daily isobars over the North Atlantic, except hypothetically, would it not probably have thrown much light on the proximate causes of, and probable duration of, our recent cold weather here, had we been able to secure a daily synoptic chart of the isobars over America, as well as those over our own islands and the countries immediately adjacent? Surely the valuable results which would follow such an extension of our present system would quite compensate for the extra outlay incurred.

E. D. ARCHIBALD

January 11

"*Weather Record for November*

"The following is Mr. Stewart's monthly record of the weather:—

"The highest reading of the barometer in the month was 29·650 at 7 A.M. on the 7th; the lowest reading was 28·643 on the 26th, showing a monthly range of 1·007 inches. The mean barometrical pressure for the month was 29·1377 inches. The highest temperature in the month was 53·3 on the 17th; the lowest temperature was 10·3 on the 30th; the warmest day was the 17th, the mean temperature being 44·10; the coldest day was the 29th, the mean temperature being 18·25. The mean temperature of the month was 30·75, being 25·73 *higher than the average of the month for the past seven years.* The mean monthly pressure of aqueous vapour was 0·148, and the mean humidity of the month was 83. The mean amount of sky clouded was 0·45. The highest wind in the month occurred at 8 A.M., on the 14th, the force being at the rate of 24 miles per hour. The most windy day in the month was the 14th, the average daily force being 15·92 miles per hour; the least windy day was the 7th, the average daily force being 2·42 miles per hour; the mean monthly velocity was 7·89 miles per hour. The prevailing direction of the wind was south. The total amount of rain that fell during the month was 0·070 inches; total amount of snow, 1·45. Total precipitation of rain and melted snow, 0·220 inches. The Red River opened again on the 18th. On the same day the steamboat *Lady Ellen* arrived from Lake Winnipeg; on the 23rd the steamer *Cheyenne* arrived from Pembina. The Red River was finally frozen over on the 27th. Two auroras and two lunar coronas were seen in the month."

The following is the editorial comment referred to:—

"The peculiar freaks of the weather during the last year or wo have defied the most ingenious efforts of the weather prophets to foreshadow its complexion with any degree of truthfulness. It is a comparatively easy task to depict the general characteristics of a season under ordinary circumstances, when the seasons for a number of years have shown no marked deviation from their usual regularity, but the abnormal nature of the weather of late has set the prophets completely at sea. The predictions of those wise-acres who, a month or two ago, told us the present season was to be excessively severe, and cited the musk-rats, the beavers, and the cornshucks, to support the prognosis, have not been verified up to the present, and without attempting the prophecy business ourselves, we would remark that the indications are against any unusual severity this season. *November has been mild to a marked degree*, and indeed the whole fall, which has just passed into winter, has been exceptionally pleasant. There has been severe weather both in Europe and Asia, and heavy frosts have fallen in England, Austria and Italy—and it has been remarked that when the winters in the Old World are very cold, they are very moderate in America."

The Microphone

IN a recent letter (NATURE, vol. xix. p. 221) Dr. Bleekrode mentions the fact that a microphone through which a strong current is sent emits an audible sound; the electro-dynamical action of the current on its movable part is considered the origin of it.

The experiment is a very interesting one, and is nearly related to the facts I published in NATURE, vol. xviii. p. 642. But I cannot agree with Dr. Bleekrode in the interpretation. It is my opinion that no electro-dynamical action is in play, but only a dilatation at the points of contact.

In a circuit were placed a battery, a tangent-galvanometer, and two pieces of carbon, which supported a third one. A sound was heard and sparks were seen. The galvanometer showed that the intensity of the current increased, the deflection increasing from five to ten degrees. This proves the influence of the clouds formed at the points of contact.

The pieces of carbon were then inclosed in very flat sheets of platinum, and the experiment repeated. No sound was heard; the deflection of the galvanometer rose to 28°. When a rough sheet of platinum was taken the intensity of the current fell again, sparks were seen, and a sound was heard.

Dr. Bleekrode believes that, the coefficient of dilatation of carbon being small, the sound cannot be caused by dilatation at the points of contact. But the temperature of those points is very high, a great part of the heat generated in the circuit being produced here.

I cannot see that his experiment is a true demonstration of the repulsive action between the subsequent parts of a current. In my opinion the experiments of von Ettingshausen (*Sitzungsberichte der Wiener Akademie*, lxxvii. p. 109) are considerably more convincing. Von Ettingshausen found that, with a current which was somewhat stronger than the one I made use of, the influence of the earth-magnetism was almost as great as that of the electro-dynamical action. Moreover, this action depends upon the relative position of the movable part and the other parts of the circuit. Now I have not been able to detect the slightest variations in the sound by changing the position of the movable piece of carbon in relation to the direction of the dipping needle, or in relation to the other parts of the circuit.

I therefore hold to the explanation of the acting of the microphone as a receiver, which I believe I was the first to propose. In my opinion it depends upon the varying dilatation at the points of contact by the varying intensity of the current.

Breda, Holland, January 13

V. A. JULIUS

The Formation of Mountains

THE quotation given by Mr. Wallace from the English Cyclopædia affords a sufficient basis to prove "the more rapid [present] cooling of the interior of the globe than of the crust." I will add a passage from Sir W. Thomson's "Secular Cooling of the Earth," of a like tendency: "I think it cannot be denied that a large mass of melted rock, exposed freely to our air and sky, will, after it once becomes crusted over, present in a few hours or a few days, or at most a few weeks, a surface so cool that it can be walked over with impunity. Hence, after 10,000 years, or, indeed, I may say a single year, its condition will be sensibly the same as if the actual lowering of temperature experienced by the surface had been produced in an instant, and maintained constant ever after." [1]

[1] *Trans.* R.S. Edin., 1862; also Thomson and Tait's "Nat. Phil.," App. D.

This constant temperature of the surface having been once established, the internal parts would be hotter than the crust, and their heat must then necessarily, by the law of conduction, pass from the hotter to the cooler region, and so into and through the crust, and be radiated away from the surface into space, the kind of action which I illustrated in my former letter by the dispersion of a crowd. Thus the interior would tend to fall to the already established temperature of the surface, and thenceforth tend to cool more rapidly than the "crust." For the nearer a stratum lies to the surface, the less cooling will be requisite to bring it down to the temperature of the surface. To take the extreme case; after the lapse of an infinite time the whole globe would eventually become of the temperature which the surface assumed at that already far-distant epoch, and has maintained ever since.

When the superficial strata had early assumed their nearly permanent temperature, they will concomitantly have attained a corresponding permanent volume, which will afterwards have proved too large for the cooling interior, so that they must, in subsiding, have become wrinkled. To this extent, then, I think Mr. Wallace's objections are untenable. Here, however, enters the question, so difficult to answer in nearly all geological problems, of "How much?" For my part, I think I have proved that the mere cooling, though a *vera causa*, would not be of itself a sufficient cause to account for the inequalities existing now, at what must be, judging by the enormous store of heat still within the earth, a comparatively early stage of the cooling.[1] O. FISHER

Harlton, Cambridge, January 18

Leibnitz's Mathematics

IN NATURE, vol. xix. p. 196, I see there is a letter respecting the claims of Newton and Leibnitz to the discovery of the differential calculus. In view of any future discussion of this matter it seems to me that the following extract from a letter of Leibnitz to James Bernoulli is worth the consideration of the advocates of both claimants:—

"Ego qui semper hoc habui eximium, ut essem mortalium docillimus, sæpeque luce ex unius magni viri verbis pauculis hausta innumera mea meditata nondum matura delevi; statim arripere monita summi mathematici."—Ex epistola Leibnitii ad Jac. Bernoullium, April, 1703, data.

The sense of this passage may, I think, be fairly rendered into English as follows:—

"I [am one] who ever regarded this as most important, that I should be most apt of mortals to receive instruction, and frequently light having been drawn from a very few words of a great man, my countless meditations not yet ripened I have blotted out forthwith to seize upon the hints of the most eminent mathematician." JAMES BOTTOMLEY

Lower Broughton, near Manchester, January 13

I HOLD myself prepared to make good my own assertions, and to respond to Mr. A. B. Nelson's call as soon as I know whether Prof. Tait has abandoned his position, or, if not, what he has to say in justification of his proceeding in denying Leibnitz to be a mathematician and affirming him to be a thief.

I am sure the editor will allow me to reply to his postscript. It is certainly not to be presumed, as a matter of course, that when Prof. Tait "lets pass such a challenge he has given up his point." But I do insist upon it that this "hard-worked scientist" had no right to pass it by after having provoked it. He put himself in the wrong, and I left him there.

But as to this being a question of merely "antiquarian interest," I take leave to deny it. I revere the name and intellect of Leibnitz, and I, for one, have a human interest in clearing that name from a foul slander. Nor should we pass by the main issue to discuss the collateral question which the editor raises in respect of Gregory's series. C. M. INGLEBY

Valentines, Ilford

German Degrees

IT having come to the certain knowledge of the Faculty of Philosophy in the University of Erlangen that a fraudulent trade is carried on in England under a pretence of procuring doctor diplomas of the said Faculty, I consider it in the interest of the public hereby to make it known that promotions *in absentia* are not conferred in that faculty, and that no one in England, or elsewhere, is, or has ever been, authorised to confer or negotiate for the conferring of such diplomas.

E. LOMMEL,
Dean of the Faculty of Philosophy,
University of Erlangen, Bavaria

Erlangen, January 14

[1] Cambridge *Phil. Trans.*, vol. xii. part 2.

Feeding a Python

THE following details of a recent attempt to feed a python now at the Raffles Museum, Singapore, may be of interest as upsetting previous ideas as to the certainty of that reptile's attack:—

The python in question is a fine specimen caught on the island, for the sake of the reward given by the police in such cases, and measures about 22 feet in length. It has been in my charge for about two and a half months, during which time it has not been fed. About ten days since it commenced casting its skin, and, as is usual after that proceeding, was unusually lively, snapping at a stick put into the cage, and in one or two instances narrowly missing the attendant's hand. The reptile, I should mention, escaped from its cage just before casting, but having taken refuge beneath some odds and ends of timber near the museum, was recaptured without difficulty, and was then placed in a cage about 5 feet square every way.

A pariah dog having been obtained, it was introduced, muzzled, into the cage, the muzzle being then slipped. While entering, the snake struck twice at the dog's hind-quarters, but without seizing it. The dog crept into a corner and sat down. Two or three more blows were then made by the snake, but, as before, without gripping, and the dog was then seen to have been struck by the teeth on the fore-quarters, the punctures slightly bleeding. *For nine successive times* the snake struck at the dog with the same ill-success, and as it was then growing dark, the shutter of the cage was closed. Early next morning the snake was found coiled round the dog, which it had killed and commenced to swallow; but a Malay attendant having touched the python with a rod, it untwined itself and retreated to a corner of the cage, refusing to again touch its prey.

I may be misinformed, but have always understood that snakes of the python or boa tribe seldom renew their attacks if the first fails; and I shall be glad if you can direct me to any published experience on the subject. The python in question is a male.

Singapore, November 25, 1878 N. B. D.

Shakespeare's Colour Names

I FEAR it would be somewhat rash to convict Shakespeare of colour-blindness, or even vagueness in the use of colour-names, solely on the evidence of the Nurse in "Romeo and Juliet"—a lady who is the Mrs. Malaprop of the play, and whose extraordinary faculty for the confusion of terms may perhaps have contributed somewhat to the "merriness" with which she credited her husband. It is possible that the Nurse—in the passage quoted by Mr. J. J. Murphy (NATURE, vol. xix. p. 197)—meant to convey the idea of a *hazel* eye, which would not be far removed in colour from that of an eagle, but also often has a slight tendency to a greenish hue. The nurse, not being particular as to the precision of her descriptions in general, refers to it as green.

It is likely besides that Shakespeare deliberately intended the incongruity, just as in the "Midsummer Night's Dream" he makes the bumpkin who acts Thisbe in that piece of "very tragical mirth," *Pyramus and Thisbe*, lament

"Those lily brows,
This cherry nose,
These yellow cowslip cheeks,
. . . . .
His eyes were green as leeks."

This passage indeed shows that Shakespeare knew perfectly well the chromatic meaning of green.

A very cursory glance through Shakespeare will show innumerable lines where colours are referred to in their true and exact sense.

Here are a few passages selected with special reference to the colours green and blue.

Prospero's description of the witch Sycorax:—

"This blue-eyed hag."—*Tempest*, i. sc. 2.

(The ideal Scandinavian witch.)

"... white and azure, laced
With blue of heaven's own tint."
Cymbeline, ii. sc. 2.

"Whose ranks of blue veins."—*Lucrece.*

"Those blue-veined violets."—*Venus and Adonis.*

"Where fires thou find'st unraked, and hearths unswept
There pinch the maids as blue as bilberry."
Merry Wives of Windsor, v. sc. 5.

"And *Hony soit qui mal y pense* write
In emerald tufts, flowers purple, blue, and white,
Like sapphire, pearl, and rich embroidery."—*Ibid.*, v. sc. 5.

Here there is no confusion. The comparisons are exact and beautiful. Again we have—

"When wheat is green, when hawthorn buds appear."
Midsummer Night's Dream, i. sc. 1.

The season indicated shows there was no confusion between green and *brown.*

We must not forget the well-known song—

"When daisies pied and violets blue,
And lady-smocks all silver white,
And cuckoo-buds of yellow hue
Do paint the meadows with delight."
Love's Labour Lost, v. sc. 2.

And to conclude our comparisons of green and blue—

"... I will rob Tellus of her weeds
To strew thy green with flowers: the yellows, blues,
The purple violets, and marigolds,
Shall as a chaplet hang upon thy grave."
Pericles, iv. sc. 1.

Returning to the colour of eyes. Shakespeare not only knew a blue eye, but could discriminate, and appreciate the beauty of a grey eye—a shade which often does duty for blue. The lovely rivals Julia and Sylvia are so endowed—

"Her eyes are grey as glass—and so are mine."
Two Gentlemen of Verona, iv. sc. 4.

"... Thisbe, a grey eye or so."
Romeo and Juliet, ii. sc. 4.

I think the above quotations afford good proof of the poet's correctness of colouring with regard to green and blue. It is true that he occasionally uses a small degree of licence with purple and blue, in the case of violets; but clearly not from unconsciousness of the difference. I cannot remember any instance where he confuses green with blue except purposely and humorously.

In the use of other colours Shakespeare is in most instances I am acquainted with equally true to nature. To give examples would occupy too much space; but if there are exceptions I have no doubt that your correspondents—now that the matter is broached—will be able to furnish them.

Sligo, January 10 EDWARD T. HARDMAN

Intellect in Brutes

THE following incident may interest some of the readers of NATURE, as affording evidence of the possession and exercise of reasoning power by a brute. During the present frost the window-sills of my drawing-room are supplied with bread for the benefit of the birds, who, finding food there, are constantly fluttering about the windows. One day a large water-rat was seen on the window-sill, helping himself to the bread. In order to reach the window he had to climb to a height of about thirteen feet: this he did by the help of a shrub trained against the wall. Neither instinct nor experience will easily account for his conduct: since he never found food there before. If neither experience nor instinct, what save reason led him? His action seems to have been the result of no small observation and reasoning. He seems to have said to himself—I observe the birds are thronging that window all day; they would not be there for nought; it may be they find there something to eat: if so, perhaps I too might find there something which I should like. I shall try.

Bardsea EDWARD GEOGHEGAN

OUR ASTRONOMICAL COLUMN

OLBERS' COMET OF 1815.—On March 6, 1815, Olbers discovered a small comet at Bremen, in about 49° right ascension, and 32° north declination, or between Perseus and Musca; it had an ill-defined nucleus and was not visible without telescopic aid. The first parabolic elements were calculated by Olbers himself, and he was followed by Bessel, Gauss, Triesnecker and others in the determination of similar orbits. Ephemerides founded upon them showed that the comet would be observable for a considerable period, and as the result proved observers were not negligent of this circumstance. Gauss, writing to Bode on April 24, alludes to the long visibility of the comet, and the probability that elliptical elements would be found, but this remark apparently was merely intended to imply that the grasp which a long course of observation would afford upon the orbit, might lead to an ellipse, not that Gauss had remarked any sensible deviation from parabolic motion; indeed he mentions that he had not then reduced his April observations. The first detection of the inadequacy of the parabola to represent accurately the comet's course, is due to Bessel: he had calculated parabolic elements from observations on March 11, April 11, and May 20, which, while agreeing well with the positions employed, gave the right ascensions sensibly too small from March 11 to April 11, and between April 11 and May 20, as decidedly too great, even to as much as 4′, and on May 26, the calculation was again many minutes in defect; these differences naturally induced Bessel to relinquish the parabolic hypothesis, and after some disappointment from the failure of the first method he employed, he communicated to Olbers on June 23 the elements of an elliptical orbit, in which the period of revolution was a little over 73 years. At the end of June Gauss deduced an ellipse with a period of 77 years, and soon afterwards Nicolai, then assistant to von Lindenau at Gotha, added a further confirmation of the elliptical character of the orbit, assigning a revolution of 72½ years. On July 22, being in possession of observations to the middle of the month, Bessel improved upon his first calculation, and now found an ellipse with a period of 73·8968 years, which was made the foundation for his subsequent investigations, of which we have presently to speak. Thus was the periodicity of the comet established, and Bessel, after remarking upon the importance of the addition to the system (at that time Halley's comet was the only one that could be considered certainly periodical) he proposed that it should bear the name of its discoverer—Olbers.

Besides a long series of observations taken by Olbers himself, the comet was observed by Gauss at Göttingen, Bessel at Königsberg, Triesnecker at Vienna, Struve at Dorpat, Oriani at Milan, Lindenau at Gotha, Maskelyne at Greenwich, and Bouvard at Paris. Its distance from the earth continued pretty nearly constant (about 1·45) during the greater portion of the time it was visible, and at no period was it a conspicuous object; its nucleus was pretty bright at the beginning of May, and it then had a tail about 1° in length.

On the disappearance of the comet Bessel collected the observations which extended to August 25, the last having been made by Gauss at Göttingen; indeed, he was the only observer after July 25. He then commenced the work which is incorporated in his great memoir upon this comet, published in "Abhandlungen der königlichen Akademie der Wissenschaften in Berlin, 1812–13," a volume which was not published until 1816. He formed ten normal positions, in which all the observations appear to be brought to bear, excepting those at Greenwich and Paris, which were doubtless unknown to him. He corrects these normals for the effect of perturbations from the action of Venus, the Earth, Mars, Jupiter, and Saturn, during the comet's visibility, and by a fine series of observations of the sun at Königsberg between March 8 and August 29, 1815, he applies corrections to the sun's places obtained from Carlini's first tables. Equations of condition were then formed and solved on the method of least squares, and thus the following definitive elements of the comet's orbit in 1815 were obtained:—

Perihelion Passage, April 25·99867, M.T. at Paris.

Longitude of the perihelion	149° 1′ 55″·9	1815·0
,, ,, ascending node	83 28 33·6	1815·0
Inclination of the orbit to ecliptic... ...	44 29 54·6	1815·0
Excentricity	0·93121968	
Semi-axis major	17·63383	
Logarithm of perihelion distance	0·0838109	
Period of revolution	74·04913 years.	

Motion—direct.

These elements represent the normals upon which they are founded very closely, considering that observations of comets in 1815 did not pretend to the degree of precision which is now sought to be attained, and, moreover, were subject in the reductions to errors in the places of the comparison stars.

But Bessel's labours did not stop here. With a special interest in the comet of 1815, not, it may be presumed, alone due to its exceptional character, but in no small degree to the circumstance of its having been detected by his most intimate and revered friend, Olbers, Bessel undertook, and in the year of its appearance accomplished, the laborious task of computing the perturbations of the planets Jupiter, Saturn, and Uranus upon the motion of the comet during the present revolution, and so determining the epoch of the next perihelion passage. The principal details of this work are comprised in the memoir to which we have already referred. The masses of Jupiter and Uranus were Laplace's, while the mass of Saturn was taken from Bouvard's tables. The whole period is divided into three sections, the first extending from August 4, 1815, to July 30, 1833; the second from the latter date, with new values of the semi-axis and excentricity to July 21, 1869, and the second from July 21, 1869, to the next perihelion passage. The action of each of the three planets tends to accelerate the comet's return, that of Jupiter by upwards of two years; the final result indicating an acceleration of 824·51 days, with reference to the period belonging to Bessel's definitive ellipse for 1815; it was thus found that the duration of the actual revolution would extend to 26222·4 days, and consequently the next perihelion passage is fixed to February 9·4, 1887. This conclusion will be affected not only by the imperfect values of the planetary masses which were available when Bessel undertook the investigation, but in a greater degree by the uncertainty which still remained as to the precise length of the revolution at the last appearance; this Bessel found to extend to ± 0·27657 of a year, or 101 days.

With such an amount of probable error attaching to Bessel's result it must soon be a matter for the consideration of the astronomer, whether a nearer approximation may not be yet attained. We have much more accurate values of the masses of Jupiter, Saturn, and Uranus than Bessel possessed, and are able to take into account the influence of Neptune, though this is not likely to be very material. Fortunately, in several series of observations, the observed differences of right ascension and declination between the comet and the comparison stars are preserved to us, and thus we can reduce the observations anew, with much improved positions of many of the stars and with modern elements of reduction. The series of observations thus available include the long one of Olbers (*Berliner astronomisches Jahrbuch*, 1818), and those of Greenwich, Paris, and Dorpat. It is a work which, together with the recalculation of the perturbations to the next perihelion passage, may perhaps be made the subject of a prize by one or other of our scientific academies; on the last return of Halley's comet, the first approximation to the epoch of arrival at perihelion was due to action of this kind on the part of the Academy of Turin, and though a much higher degree of interest attached to the reappearance of that famous body, we do not despair to see Olbers' comet deemed worthy of a new and more refined calculation.

If these cometary bodies wandering into the confines of the solar system from the stellar spaces are fixed therein by the action of one or other of the planets, it will have been owing to a very close approach to the planet Mars that Olbers' comet presented itself in 1815, moving in an ellipse of moderate dimensions. The nearest approach of the two orbits in that year was 0·07 in 86°·4 heliocentric longitude, but this distance must have varied in successive revolutions through the perturbations of the other planets, and at some past time there may have been an intersection of the orbits and a close encounter of the two bodies.

METEOROLOGICAL NOTES

BEFORE the commencement of the summer rains this year Mr. Eliot, the officiating meteorological reporter to the Government of India was called upon for a report on the prospects of the season. His reply, to which we have already referred in the "Notes," consisted of a short *résumé* of the most important characteristics of the southwest monsoons of recent years, from which the following conclusions were deduced:—"1. The persistent excessive pressure over Northern India at the present time (June, 1878), tends to diminish the baric gradient between Southern Asia and the Mid-Indian Ocean, and if this is not compensated by increased pressure over the sea area to the South of India, the monsoon current will be below its average strength. 2. There appear to be no strongly-marked abnormal variations of pressure over Northern India. It is therefore probable that the rainfall will be much more equally distributed than last year. 3. Comparing the present year with 1865, it is probable that the heavy rainfall during the cold weather, and more especially in May, will slightly retard the advent of the monsoon in Upper India. 4. The probable effect of the low pressure along the Bombay coast cannot be determined except by comparison with last year. It appears to promise fairly abundant rain over that portion of the country." These conclusions have now been subjected to the test of experience and are found to have been verified in almost every particular. The southerly current from the Indian Ocean has been decidedly below its normal strength; the rains set in from a fortnight to a month after the usual time; every district in the country has received a moderate supply of rain, though the average rainfall for the whole country has been less than usual, and over the Bombay Presidency, from Belgarum to Kurrachee, the rainfall has been in excess of the average for previous years. The only peculiarity of the monsoon of 1878, that was not predicted, was the frequent recurrence of heavy falls of rain over a few small and well-defined areas; but this would seem to be the character of the rainfall of every year in which the monsoon current is of less than the usual strength. The percentage of verifications reached by Mr. Eliot has thus been as great as that attained by the American observers, and the predictions in his case were made months, not days or hours, in advance. The same meteorologist has recently made a discovery which promises to be of the greatest possible value in connection with the system of storm-warnings to the ports round the Bay of Bengal. It is that a cyclonic vortex, when generated in the middle of the Bay, always travels towards that part of the coast where the wind velocity for the time being is least in comparison with the average velocity for the same place and time of year. This law has been verified by almost all the cyclonic disturbances that have occurred in the Bay since a chain of meteorological observatories was established round it, and it lends a great deal of support to the theory that a cyclonic vortex is developed through the accumulation, concentration, and condensation of aqueous vapour over a region of comparative calm. All that appears now wanted to

render cyclone prognostications for the Bay of Bengal almost absolutely certain is a submarine cable to the Andaman and Nicobar Islands, by which the meteorological stations on these islands, near the place of origin of all the great cyclones of the Bay, would be brought into telegraphic communication with the rest of the empire.

IN his "Tenth Contribution to Meteorology," which appears in the *American Journal of Science and Arts* for the present month, Prof. Loomis gives the results of an examination he has made as to the course of seventy-seven storms after leaving the eastern coast of the United States, these storms having occurred from March, 1874, to November, 1875. Of these seventy-seven storms he was able to follow thirty-six of them entirely across the Atlantic Ocean, eight of them, however, becoming merged in other storms before reaching Europe. The annual average of storms which are found to cross the Atlantic from the United States to Europe is eighteen, and nearly all of these storms pursued a course north of east, passing in their eastward course considerably to the north of Scotland; indeed, in only four of the storms did the centre pass as far south as the north of England. Prof. Loomis concludes that, when a storm with a centre depression at least below 29·5 inches leaves the coast of the United States, the probability that it will pass over any part of England is only one in nine; that it will occasion a gale anywhere near the English coast, one in six; and that it will give rise to a fresh breeze, one in two. A characteristic feature of these storms is the slow rate of their onward progress in crossing the ocean, as compared with their rate over the United States—a feature of the utmost possible importance in attempting to predict the time of their descent on the shores of Europe of those American storms which cross the Atlantic. About half of the whole number of the storms originated in the neighbourhood of the Rocky Mountains, five in or near Texas, and four were distinctly traced to the Pacific coast. Of six West India cyclones which occurred in the same time only two could be traced across the Atlantic, and even one of these became blended with another storm. The rest of the paper is taken up with a discussion of the fluctuations of the barometer on Mount Washington, 6,285 feet, and Pike's Peak, 13,960 feet, as compared with what takes place on the level ground at the base of these mountains. As regards Mount Washington, the valuable result is arrived at that the diurnal maxima and minima of the barometer occur more than three hours later at the summit than at the base, showing an average retardation of one hour for each 900 feet of elevation. In the case of Pike's Peak, the rate of retardation is one hour for an elevation of 1,380 feet. It is evident from these figures that the law of the rate of retardation is yet to be sought, one of the most important factors, in all probability, being the absence or presence of high plateaux and their extent near the high station, to which must be added the latitude of the place. Observations of the wind at these high levels show, just as at places near sea-level, a circulation about a low centre, the movement of the wind being approximately at right angles to the direction of the low centre; and further, that at the height of Mount Washington, the low centre of storms sometimes lags behind the low centre at the surface of the earth as much as 200 miles. This last result is so vital in the theory of storms as to demand a much more extended examination, the most special care being taken that the retardation of the time of occurrence of the diurnal barometric minima be allowed for in the discussion.

IT is with extreme satisfaction we learn that at a recent meeting of the Council of the Scientific Association of France, M. Mascart, Director of the Meteorological Department, submitted a proposal from the Departmental Commission of Vaucluse, for the establishment of an observatory on the top of Ventoux, situated to the north-east of Carpentras, and rising above all the surrounding summits to a height of 6,300 feet above the sea. This observatory in the south of France, along with the observatories of Puy de Dôme in the north, and of Pic du Midi in the south-west, may be regarded as furnishing France with an enviable system of elevated observatories for meteorological observations such as no other country possesses, thus putting French physicists in possession of the essential data whence the more difficult meteorological problems may be attacked, and the systems of weather-warnings for navigation and agriculture more rapidly developed and improved. It is estimated that 150,000 francs will be required to establish the station, of which sum there are already subscribed by M. R. Bischoffsheim 10,000 francs, by the Commune of Bédoin, situated at the foot of Mt. Ventoux, 10,000 francs, the Council of the Scientific Association 500; and as the Meteorological Commission of Vaucluse has opened a subscription-list, the General Council of the Department has promised to aid in forming the roadway up the mountain, and a subsidy is looked for from the Minister of Public Instruction, the establishment of this important observatory will doubtless soon become an accomplished fact.

GEOGRAPHICAL NOTES

WITH reference to the reports that Prof. Nordenskjöld's vessel had got shut in by the ice near East Cape, in Behring Strait, the Committee for Promoting Russian Trade and Industry have resolved to apply to the Governor-General of Eastern Siberia, requesting him to assist in instituting a search for Prof. Nordenskjöld, and in obtaining more certain information as to the situation of the expedition. Mr. W. H. Dall, the well known U.S. Alaska explorer, has written a letter to an acquaintance in Stockholm, mentioning the previously-reported statement of whalers, from which it is supposed that the *Vega*, has been stopped by ice east of Cape East. Should this be the case, Mr. Dall entertains no fears for the fate of the expedition. If these suppositions be correct, he says, "the breaking up of the ice next July will leave open water for the *Vega* to proceed to Behring Strait. Vessels pass to westward of East Cape every year. There is a creek there. (The letter here gives a sketch map describing a bay, with a small island in the middle of it, and an anchorage inside.) A river with fresh water runs into the bay, and on the coast is a native village. This is not marked in the ordinary maps and charts, and it is just here that the vessel, according to the repots of the natives, must be lying. She can safely winter there. There is a large village, inhabited by Tchuktchees, who would be able to supply fresh meat. This place is situate not more than 200 English miles from the white men's trading station at Plover Bay. If the *Vega* is lying there, the success of the operation is practically achieved, because, as I said, the bay is open every year, and does not get closed by ice until October. Vessels sail there, and carry on trade every summer."

THE last number of the *Isvestia* of the Russian Geographical Society contains an interesting paper by M. Grigorieff, on the temperature and density of water in the Arctic Ocean, along the coast of Russian Lapland, and in the White Sea, being the result of observations carefully made on board the schooner *Samoyede*, by means of good instruments. As to the Arctic Ocean, M. Grigorieff confirms the existence of a warm branch of the Gulf Stream which flows along the coast as far as Gavrilovskiye Islands, and thence turns due east to the Kanin Peninsula and Kolgueff Island, and further, to the Moller Bay on Novaya Zemlya. Beneath this warm current there is a cold one flowing in an opposite direction at some depth. When it meets with a rising bottom, and especially with the deep bank of less than 100

fathoms under 71° N. lat., this denser and cold current is compelled to change its direction, and makes its way between the Gulf Stream and the shore; hence the low temperatures and great density of water at the Lapland coast, in the space between Svyatoy Nos and the Seven Islands. The density of the eastern (North Cape) branch of the Gulf Stream (1·025 to 1·026, figures which correspond to a percentage of salt of from 3·28 to 3·41), seems to be smaller than that of the Spitzbergen branch, where Nordenskjöld has found a percentage of salt as high as 3·625. As to the White Sea, M. Grigorieff denies the entrance of a branch of the Gulf Stream into that sea, as was supposed some years ago by Prof. Middendorff; the Gulf Stream does not penetrate further than the Gulf of Mezen, and the warm temperatures observed by Middendorff are due to purely local causes. On the contrary, a cold polar current enters the White Sea along the Tersky coast, whilst the current which flows out of the sea into the ocean, follows the Winter and Kanin Coasts. The water of the White Sea on the whole has a very low temperature, especially in the deeper parts; on depths more than 100 fathoms the temperature is always below 32° Fahr., and this, because of the great loss of heat during the long winter. Altogether, the observations having been made and computed very carefully, and published *in extenso* in the *Isvestia*, are a real acquisition to science.

TWO new expeditions to Central Asia are planned in Russia for the next spring. The first, by Col. Prjvalsky, to Hlassa in Thibet, and thence to Afghanistan; and the other, by M. Blumenfeld, a German *savant* who has studied in Russia, for botanical and geological explorations; M. Blumenfeld will follow nearly the same route as that proposed by M. Prjvalsky.

UNDER the title of "D'Orenbourg à Samarkand" Madame de Ujfalvy has commenced in the *Tour du Monde* an illustrated account of her travels in Ferghanah and Western Siberia. Leroux, of Paris, has just brought out the first volume of M. de Ujfalvy's account of the results obtained during his mission. These results are mainly ethnological, and contain many observations and careful and detailed measurements of a large number of individuals representing the various races of that part of Central Asia visited by the traveller and his wife.

DURING the year 1878 the following accounts of Russian exploration were published in Russia: that of Col. Prjvalsky to Lob-Nor, now translated into English; of M. Wojeikoff in India and Japan; rather literary than scientific is that of M. Minayeff on his journey to India, which contains very interesting observations on Buddhism; of M. Ogorodnikoff to Persia, giving among other data an account of the trade-routes to Persia and Afghanistan; and of M. Skalkofsky to Eastern Asia and California.

AS Sir H. Rawlinson has announced his intention of delivering an address at the next meeting of the Geographical Society, on the road to Merv from the Caspian, it will not be without interest to note some particulars respecting the earlier part of the route, as far as the Tekké fort of Kizil Arvad, from an account lately furnished to the *Moscow Gazette* by a writer who appears to have been attached to General Llamakin's staff. The party were obliged to strike eastwards from the Chikishliar littoral by a road which has never yet been described, but which is the most practicable route to the Attrek, the bank of that river, from its mouth at Hassan-Kuli Bay almost to Balt Adji, being bordered by inapproachable morasses. The ground traversed was at first covered with shells, but soon presented the appearance of a salt marsh petrified by the sun; then, after a stretch of sand, firmer soil was met with. No water was found until the wells of Karadji-Batyr were reached. About twelve versts from the wells the party arrived at the gates, as it were, of an enormous wall, which bore a greater resemblance to an artificial structure than to a natural conformation of the soil. Three versts further on the valley of the Attrek appeared in sight, with the river itself winding between high and verdant banks. Here is Bayat-Adji, a name which is also applied to the whole of the surrounding country. From this spot the party proceeded up the Attrek to Chat or Chad, following an excellent road. About ten versts before reaching Chat the road turned to the left, leaving the Attrek at a point where there are large *auls*, or settlements of the Atabai tribe. At length Chat was reached, and it is described as the most repulsive place along the whole Attrek, although from a strategical point of view the most important, because it is here that the River Sumbar (which Capt. Napier calls the Sunt) flows into the Attrek, and the delta might be made an impregnable position. Fifty versts above Chat two enormous rocks rise out of the Attrek, forming a sharp delimitation of the geological structure of the country. This place is called Su-Sium; after this point the road is impassable for camels, and 10 versts further on is difficult even for horses; 100 versts beyond Chat the course of the Attrek can only be followed on foot, and it would take three months to make the road practicable. In consequence of the difficulties mentioned, the party was obliged to abandon the course of the Attrek at Su-Sium, and to strike a new road. After making the necessary surveys they turned to the left at a place called Alun-Yak, and proceeded over the high Sugundag chain. The ascent and descent of the Sugundag extends over a distance of 16 versts, the descent terminating at the small River Chandyr, which falls into the Sumbar. Twenty-five versts from Chat the party crossed the Sumbar, and marching between that river and the Chandyr, reached an elevated mountain called Bek-Tépé, belonging to the spurs of the Kurindag. Leaving the Sumbar they proceeded through the waterless defiles of the Ters Akon, and through the Morgo defile (belonging to the Kaplandag range), and reached the ruins of Hadjan-Kala, near the Tekké fort of Kizil Arvad. The road through the defiles presents many difficulties, and only two horses can proceed along it abreast, but it is thought that a good road could be made without much trouble or expense.

THE Society for Promoting Christian Knowledge publishes a very excellent small wall-map of Africa, by Stanford, containing all the most recent discoveries and useful both for teaching and general purposes.

ON January 25 the Geographical Society of Paris will hold a public reception in the large hall of the Sorbonne, in honour of MM. de Brazza and Ballay, the two French Ogowé explorers. The great medal for 1879 will be delivered on this occasion by Admiral La Roncière le Nourry, the president of the Society.

NO. 78 of the *Zeitschrift* of the Berlin Geographical Society contains a careful geographical and statistical study on the Brazilian province of Rio Grande do Sul, by M. Bescharn. Botanical students will be interested in Dr. Klunzinger's elaborate paper on "The Vegetation of the Arabian Desert near Koseir." This number contains a carefully arranged, most complete, and valuable bibliography of geographical literature and cartography for the year from November, 1877, to November, 1878.

NO. 3 of *Globus* of this year contains a fine illustration of the wonderful reclining statuary figure of Chac-Mool, unearthed in Yucatan some time ago by M. le Plongeon. The same number contains the sixth contribution of Herr Zehme to a *résumé* of recent exploration in Arabia.

THE GEOLOGICAL HISTORY OF THE COLORADO RIVER AND PLATEAUS[1]

II.

IN the Pliocene period the climate of the region gradually experienced a great change. Miocene times were characterised by a moist and ordinary sub-tropical climate; the Pliocene by developing an arid one, like that which now prevails there. Let us look at the causes which make this climate what it is. In whatever rectilinear direction we may undertake to pass from the Pliocene Country to the ocean we shall be compelled to cross some of the loftiest barriers of the Continent. It is hemmed in by range after range of high mountains. The winds laden with moisture are wrung dry long before they reach the plateaux in the heart of the province. The prevailing wind throughout the year is from the westward, and must cross the Sierra Nevada. Sweeping across the great basin it blows over many ranges, and at last strikes the Wasatch and the chain of high Plateaux which form the

Fig. 3.—Grand Cañon, from the Middle Terrace.

western wall of the Plateau Province. Here it is suddenly projected upward more than a mile and flings down moderately copious rains. Descending into the Cliff and Cañon Country, its humidity is so much exhausted that it can yield but the scantiest pittance of snow and showers. Thus the country is a desert. Now the strange forms impressed upon this land—its cliffs and cañons, with their myriads of wonderful shapes and their astounding architecture—are due, as we shall presently find, in great part to the aridity. The aridity is due to the great barriers which surround it, and above all to that great barrier of high plateaux which lies upon its western verge. Here, then, we may look for another key which may unlock another door within the vestibule. The search will not be fruitless.

The district of the high plateaux has been during the last four years a field of special study by myself, and has

[1] By Capt. C. E. Dutton, U.S. Army, Assistant-Geologist U.S. Survey of the Rocky Mountain Region, under Prof. J. W. Powell, in charge. Continued from p. 252.

been investigated as thoroughly as circumstances and my very limited qualifications would admit. Its original attraction consisted in the enormous displays of volcanic energy there in former times, to which I cannot here venture to allude any further. The structure of the district is also otherwise very interesting, and has been worked out with much care and patience, and in great minuteness of detail. It will be possible at present to give nothing more than a categorical statement of certain results. To master the evidence would require the handling of a large amount of detail, and unwarrantably protract discussion.

The structure of these plateaux is identical with what Prof. Powell has described as Kaibab structure, being in fact a northward continuation of the same belt which he has described and delineated in his well-known section of the Grand Cañon which cuts across this series of displacements at a right angle. The faults which have blocked out the plateaux and intervening valleys are of prodigious length, and the amounts of dislocation are very great—greater in the high plateaux than in the Kaibabs. The age of these displacements is an important landmark in the history of the country, and that age can be fixed with confidence as late Pliocene, and continuing into the Quarternary, and probably down to the present time.

With this fact in our possession as a datum we come now to the history of the cañons. The Grand Cañon first makes its appearance in the epoch of the faults. It suddenly bursts into view as a less than half-formed thing, with walls ranging from 2,000 to 2,700 feet high, late in Pliocene times. But it presents itself under somewhat unexpected circumstances, for it had been in the condition in which we first find it for a considerable period. The work of vertical erosion had long been suspended, the channels had ceased to grow deeper, and the energies of the river had for an unknown period been employed in another kind of occupation to which rivers have been frequently known to betake themselves under certain common conditions. It was widening its cañon and making a flood plain in which to meander. This any river will inevitably do when it has sunk its channel to the limiting depth which local circumstances prescribe for it. When that limit is reached it will attack its own banks whether they be walls of rock or nothing but gravel and loess, and will thereafter meander or squirm from side to side. There are numerous places along the Upper Colorado and its tributaries where this is abundantly exemplified. From local causes the fall of the river has for a space been diminished, the flow has been sluggish, sediment has been deposited, the river has ceased to erode its bottom, it has attacked its walls, and the cañon has been widened.

If now the reader will look at the section of the Grand Cañon (Fig. 3) he will perceive that it is a cañon within a cañon. The walls are in two leaps with an intermediate terrace. The upper or outer cañon is usually from three to six miles wide, and the inner cañon meanders within the upper, sometimes close to one upper wall, sometimes to the other, but usually with a middle terrace on both sides. The inner and the outer cañon represent two periods, the outer one of course being formed first—formed no doubt originally as a narrow gorge—which was widened while the river was unable to cut vertically. The middle terrace is the final flood plane of the old cañon. And now the faults come to our assistance in determining the two periods. The outer cañon is older than the faults; the inner one is coeval with them. The reasoning by which we determine this is of the simplest order. If we were to see a fault cutting a particular stratum we should know that the stratum was older than the fault. By parity of reasoning we know that the outer cañon is older than the faults because they cut its trough and dislocate its floor transversely. If the faults were older the river would have

FIG. 4.—Pa-ru-nu-Weep Cañon, Virgin River, a tributary of the Colorado.

planed an even grade across them regardless of the dips of the strata just as it is doing to-day 3,000 feet below. As it is—if the side gorges would permit us to travel along the middle terrace—we should be compelled every time we crossed a fault to clamber up or down its face.

Thus, then, as we draw near the close of the Pliocene

period only the outer cañon was completed. When was it commenced? At present we cannot give an answer, though I hope we may soon be able to do so; but I should be surprised to find its commencement dating beyond Pliocene time. The best conjecture which I am at present able to frame would place the birth of the Grand Cañon since the middle of that epoch. The commencement of the cutting of the inner cañon was contemporary with the commencement of a new uplifting of the Kaibabs—an uplifting which extended as far north as the Wasatch, and southward to unknown regions in Arizona, through a belt having its maximum width just where the Grand Cañon crosses it. The amount of this uplifting was very variable, ranging from 2,000 to 4,000 feet. It was during this period of elevation that the faulting took place. The level of the river's bed was at once disturbed; its old energies were reawakened, and its ancient labours resumed. From that epoch to the present day the river grinding like "the mills of God" has slowly but resistlessly sunk itself to solemn depths in the earth.

Wonderful and impressive as are the great cañons, they are no more so than some other features. Chief among the objects of special interest is the vast array of colossal cliffs, which stretch across the country with seemingly interminable length in a grouping which is half order half disorder. The number is indeed very great, their altitudes generally impressive—1,500 feet being common, and 2,000 feet not very uncommon, while the distant view of cliff rising above cliff, one beyond another, yet seeming to be united, is often seen. Each stratigraphical series has a series of cliffs, planned, sculptured, and painted in a style peculiarly its own, and the several styles differ, as decidedly and constantly as human architecture among distinct races of men. These distinguishing characters developed under one homogeneous process can be traced to the lithological composition and texture of the rocks which are powerfully contrasted between any two series. The constancy of result in any given series may also be traced to the constancy with which that series preserves one set of characters over a great extent of country. I may be mistaken—perhaps from the circumstances it is more apparent than real—but I imagine no region in the world hitherto explored exhibits rocks where the texture and lithological characters are so strongly pronounced, so strongly contrasted among themselves, and yet where there is so little horizontal variation in the characters of each group over vast areas.

In the Plateau Country we have to do with an arid region, and the aridity tends to reduce the amount of disintegration. On the other hand it is a lofty country giving a rapid descent to all its waterways, and their transporting power is of a very high order—the rocks are swept bare of *débris* and kept naked to the attacks of the elements. This tends powerfully to quicken the disintegration. The number of inches of annual rain is less than one-sixth the number in the Mississippi valley, but every inch in the plateaux may do sextuple work. Probably, however, the rate in the plateaus is on the whole slower, but the disproportion is much less than might have been anticipated if we had considered the rainfall alone.

To comprehend the origin and perpetuation of cliffs it is necessary to expand these general relations into some detail. I have stated that the attack of erosion is directed against the edges of the strata and but slightly against the horizontal surfaces. These surfaces being but little inclined, water has but little energy, as it courses over them, either to erode or to transport. But in the myriads of gulches the steepness of their sides enables the water to keep the edges of the strata naked, and the water is assisted powerfully by the aridity of the climate and the absence of vegetation. Now when the edges of a thick series of vertically heterogeneous strata are exposed, there will always be some stratum softer or more readily disintegrated than the others. The elements attack it, and soon a long under-cut is formed, and the rocks above robbed of a part of their support, cleave off vertically, and a great slab falls in ruins. The fallen fragments and rubble form a talus, but being now in a comminuted state, they become a much easier prey to dissolution than when in the solid wall, and they gradually moulder away. All that is necessary is that the talus should dissolve fast enough to keep the perishable stratum exposed to attack, and this is almost universally the case. The great cliffs are massive beds of sandstone and limestone, resting upon perishable calcareous and gypsiferous shales. The rapidity with which the cliff wastes away and recedes by erosion is measured by the power of resistance to weathering in the shales below and not by the massive beds on its face. By further analysing the details of erosive action, we have no difficulty in explaining the origin and causes of the different styles of architecture, the sculpture of the repetitive forms, and all their train of phenomena, both normal and abnormal.

And now a few words about the cause of cañons. This problem has been so admirably and satisfactorily solved by Messrs. Powell and Gilbert that I have no better excuse for saying anything about it than a desire to fill what would otherwise be a serious gap in the discussion.

The fall of the Colorado through the cañons is between seven and eight feet per mile—nearly twenty times as great as that of the Ohio and Mississippi and nearly seven times as great as that of the Missouri below the Yellowstone. It is a fierce torrent—a series of quickly-recurring rapids. Its lateral gorges have usually a greater descent. The tools with which the river works are sand and gravel held in suspension by the water, hurled along at race-horse speed, and scouring like a sand-blast machine the naked rocks of its bed. But there is one thing more, and it is a crucial point. The Platte has about the same fall through the plains as the Colorado through the cañons; it has its sources high up in the same mountains; it flows through a desert; it carries a huge load of sand, but from Denver to Plattesmouth has not the semblance of a cañon. The trouble with the Platte is that it carries *too much* sand. A river of given volume and velocity can carry in suspension only a definite load of sediment of given coarseness. When that limit is exceeded the excess will be precipitated upon the bottom protecting it from the scour of the gritty particles which are carried in suspension. But if the supply of sand be not in excess of the power of the current to keep it in suspension, none will be deposited except locally, and the bed-rock will experience the full attrition of the sand-blast. The Platte is the case of an overloaded stream while the Colorado is slightly underloaded and in a condition to produce the maximum erosion.

The study of the Plateau Country has during the last nine years been the work of the Survey under Prof. J. W. Powell. Comparatively little has been published about it because it has been felt by him that until the subject could be presented in systematic and thoroughly intelligible form it would be a mistake to accumulate fragmentary literature and encumber a splendid subject with a chaos of unconnected observations. But the work approaches completion and has developed into form in the minds of the workers, and it is hoped that the results will soon be before the world. If the geology of the Plateau Country shall therein be set forth in a manner commensurate with its importance, and full justice done to the revelations it affords, I believe that physical geology will have received important additions. I cannot close without paying a just tribute to Prof. Powell, the director of their work. His direction of the Survey has not been limited to the perfunctory duties of an administrative officer. On the contrary he has furnished those whom he has called to his assistance with methods of

observation and principles which have worked like a master-key in opening to our understanding the meaning of this wonderful region. Without those methods and principles it would have been of comparatively little utility to attempt to solve the problems of such a region. Those whose privilege it has been to carry them into practice will ever be glad to acknowledge how great is their indebtedness.

INCLINATION OF THE AXES OF CYCLONES AND ANTICYCLONES

I HAVE during the last seven years endeavoured, though apparently without much effect, to direct the attention of meteorologists to a law which I conceive to be of very high importance in relation to the theory of the movements of the atmosphere. The law to which I refer is this:—The movements of the upper-currents prove that the axis of a progressive cyclonic circulation is commonly inclined, so that the extremity nearest to the earth's surface is considerably in advance of that in the higher regions of the atmosphere. A barometric minimum consequently occurs at any locality on the earth's surface some hours before the corresponding minimum in the higher regions passes over the same spot ("Laws of the Winds Prevailing in Western Europe," pp. 156 to 162, 1872; *Meteorological Magazine*, vol. x. pp. 92 to 93, 1875; *Quarterly Journal of the Meteorological Society*, October, 1877, pp. 440 to 445). I have also pointed out that the axis of an anticyclonic circulation has, at least in some instances, a similar inclination; a point which will be discussed more fully in a future paper.

I hope that the results, strongly confirmatory of this law, which Prof. Loomis has recently derived from his examination of the wind and barometer reports from Mount Washington, Mount Mitchell, and Pike's Peak, will attract more attention than my own deductions from upper-current observations have done. In his tenth paper of "Contributions to Meteorology" (*American Journal of Science and Arts*, January, 1879), Prof. Loomis shows that with very few exceptions the barometric minima occur at the base of a mountain considerably earlier than at the summit, the retardation amounting to about one hour for an elevation of from 900 to 1,300 feet, and that the maxima appear to follow the same law.

Other points of agreement between the results of cloud observations in Europe, and those obtained from the reports of the mountain observatories in America, seem to me to be of great interest. I would especially call attention to the substantial coincidence of these results as regards, first, the rarity of easterly upper-currents, as compared with easterly surface-winds; and secondly, the higher, and also less variable, value of the angle made by the northerly, than that made by the westerly upper-currents, with the direction of the centre of lowest pressure at the earth's surface. W. CLEMENT LEY

BARTOLOMEO GASTALDI

SINCE the last anniversary of the Geological Society many distinguished men among its members, both in this and foreign countries, have been removed by death. We regret to have to add to the sad list the name of Prof. Gastaldi, the well-known head of the Italian Geological Survey.

Bartolomeo Gastaldi was born at Turin, in the year 1818, and was originally destined by his father for a legal career; his fondness for geological studies, however, proved too strong to be repressed, and he was eventually entered as a student at the École des Mines at Paris. Here, and throughout his subsequent career, he enjoyed the friendship of Quintino Sella, who afterwards became so distinguished alike in Italian scientific and political circles.

Gastaldi had reached the age of twenty-eight before his first scientific memoir was published, and his earliest essays in this direction were devoted to anatomical and palæontological questions. Before long, however, he seems to have discovered that the true bent of his genius was towards physical geology. In his studies in this department of science he was greatly aided by his powers as a pedestrian, and he soon made himself familiar with all the southern spurs of the Alpine chain. In company with his friend Sella he founded the Italian Alpine Club, of which he was the second president.

He succeeded Sella as Professor of Geology at the Engineering School of Turin, and subsequently became Professor also at the University. During the later years of his life the work of the Geological Survey, of which he was made director by the Italian Government, occupied much of his attention, and to his energy and capacity much of the success which has already attended that important work is due.

No less than thirty papers on various branches of geological science have proceeded from Gastaldi's pen. He was an advocate, during his later years, of extreme views upon glacial subjects, and many of the views which he propounded on this and on other questions of Alpine geology have not been generally accepted by the geologists of other countries. In some of his speculations, indeed, his boldness seems to have outrun his caution. Those who had the happiness of a personal acquaintanceship with Gastaldi describe him as a most sanguine and earnest student and a warm-hearted friend.

Prof. Gastaldi was a Corresponding Member of the Geological Society of London, and received similar honours from the academies of many other foreign countries. In Turin, where he spent the greater part of his life, and where he occupied the position of a Common Councillor, he was very greatly respected and beloved; this fact is testified to by the circumstance that at his funeral more than three thousand people followed his remains to the cemetery.

ON THE DETERMINATION OF ABSOLUTE PITCH BY THE COMMON HARMONIUM[1]

THE methods described depend upon the principle that the absolute frequencies of vibration of two musical notes can be deduced from the *interval* between them, *i.e.*, the *ratio* of their frequencies, and the number of beats which they occasion in a given time when sounded together. For example, if x and y denote the frequencies of two notes whose interval is an equal temperament major third, we know that $y = 1{\cdot}25992\,x$. At the same time the number of beats heard in a second, depending upon the deviation of the third from true intonation, is $4y - 5x$. In the case of the harmonium these beats are readily counted with the aid of a resonator tuned to the common over-tone, and thus are obtained two equations from which the absolute values of x and y may be found by the simplest arithmetic.

Of course, in practice, the truth of an equal temperament third could not be taken for granted, but the difficulty thence arising would be easily met by including in the counting all the three major thirds which together make up an octave. Suppose, for example, that the frequencies of c, e, $g\sharp$, c' are respectively $x, y, z, 2x$, and that the beats per second between x and y are a, between y and z are b, and between z and $2x$ are c. Then,

$$4y - 5x = a,$$
$$4z - 5y = b,$$
$$8x - 5z = c,$$

from which

$$x = \tfrac{1}{3}(25a + 20b + 16c),$$
$$y = \tfrac{1}{3}(32a + 25b + 20c),$$
$$z = \tfrac{1}{3}(40a + 32b + 25c).$$

[1] Abstract of a paper read before the Musical Association, December 2 1878, by Lord Rayleigh, F.R.S.

In the above statements the octave $c-c'$ is for simplicity supposed to be true. The actual error could be readily allowed for, if required; but in practice it is not necessary to use c' at all, inasmuch as the third set of beats can be counted equally well between $g\sharp$ and c.

Although at first sight the method just sketched looks satisfactory, it is not practical in the case of the harmonium, in consequence of the pitch of the various notes not being sufficiently constant for the purpose, even when the blowing is carefully conducted with the aid of a pressure-gauge. A small variation in the absolute pitch of a chord when sounded under slightly varying pressures, would not be of much importance, but the slightest change of *interval* is fatal to the success of the method, and such a change actually occurs.

In order, therefore, to apply the fundamental principle with success, it is necessary to be able to check the accuracy of the interval which is supposed to be known, at the same time that the beats are being counted. If the interval be a major tone (9 : 8), its exactness is proved by the absence of beats between the ninth component of the lower, and the eighth component of the higher note, and a counting of the beats between the tenth component of the lower and the ninth of the higher note completes the necessary data for determining the absolute pitch.

The equal temperament whole tone (1·12246) is intermediate between the minor tone (1·11111) and the major tone (1·12500), but lies much nearer to the latter. Regarded as a disturbed major tone, it gives slow beats, and regarded as a disturbed minor tone it gives comparatively quick ones. Both sets of beats can be heard at the same time, and when counted give the means of calculating the absolute pitch of both notes. If x and y be the frequencies of the two notes, a and b the frequencies of the slow and quick beats respectively,

$$9x - 8y = a$$
$$9y - 10x = b,$$

whence

$$x = 9a + 8b$$
$$y = 10a + 9b.$$

The application of this method in no way assumes the truth of the equal temperament whole tone, and in fact it is advantageous to flatten the interval somewhat by loading the upper reed with a minute fragment of soft wax, so as to make it lie more nearly midway between the major and the minor tone. In this way the rapidity of the quicker beats is diminished, which facilitates the counting.

It is impossible, of course, for the same observer to count both sets of beats, and the counting of even one set without the aid of resonators would present difficulties to most unpractised persons. Great assistance may be obtained by the choice of a suitable position. A room in which a pure tone is sounded is traversed by surfaces at which the intensity of sound is very much reduced in consequence of the superposition of vibrations reflected from the walls and ceiling. By choosing as the place of observation a position where the intensity of the beats which are not to be counted is a minimum, and with the aid of a resonator tuned to the pitch of the beats which are to be counted, the listener is able to work with ease and certainty.

The course of an experiment is then as follows:—The notes *C* and *D* are sounded, and the listeners begin counting the beats at a given signal, whose pitch is about d'' and e'' respectively. At the expiration of a measured interval of time a second signal is given, and the number of both sets of beats is recorded.

In my experiments the interval of time was ten minutes (in one case eleven minutes), and the rapidity of the beats was about four a second. The listeners counted up to ten only, after each set of ten making a stroke with a pencil on a piece of paper. The number of strokes was afterwards counted, multiplied by ten, and added to the number which the listener was saying at the instant of the second signal. The following are the details of the actual observations:—

September 16, 1878.—Period of observation ten minutes. Numbers of beats 2392 and 2341.

$$a=\frac{2392}{600},\ b=\frac{2341}{600},\ \text{giving } x=\frac{9\times2392+8\times2341}{600}=67{\cdot}09,$$

for the frequency of the lower note C.

September 17.—Period of observation ten minutes.

$$a=\frac{2423}{600},\quad b=\frac{2302}{600},\ \text{giving } x=67{\cdot}04.$$

September 18.—Period of observation ten minutes.

$$a=\frac{2476}{600},\quad b=\frac{2261}{600},\ \text{giving } x=67{\cdot}29.$$

September 19.—Period of observation eleven minutes.

$$a=\frac{2663}{600},\quad b=\frac{2547}{600},\ \text{giving } x=67{\cdot}19.$$

The discrepancies are hardly greater than may be attributed to errors in giving the signals, by which the intervals may have been unduly lengthened or shortened by about a second. On each day after the counting of the beats between C and D, the harmonium was compared with a Kœnig fork whose nominal frequency was 64. In order to obviate any objection arising from a mutual influence of the notes of the harmonium, *both* C and D were sounded at the same time as the fork. The beats between C and the fork were counted for about ninety seconds, during which time the fork was not bowed. In this way the pitch of the fork came out on the four days respectively as 64·06, 64·07, 64·17, 63·98, that is somewhat sharper than its nominal pitch, a result in agreement with that obtained by other methods.

The object of the experiments referred to was rather to prove the practicability of a method so unusually independent of special apparatus, than to obtain a result competing in point of accuracy with those of Prof. Macleod and other experimenters on this subject. Nevertheless it is believed that very accurate results might be obtained by the introduction of certain modifications. Ten minutes is near the limit of time over which beats can be conveniently counted by a single listener, but experiment proved that it is perfectly possible for one listener to relieve another without any break in the regularity of the counting. Even without an extension of time a more accurate result would be obtained if the listeners were able to fix the time for themselves, as they might do for example if they could conveniently observe the swinging of a clock pendulum. In this way the error in the time interval might be reduced to $\frac{1}{4}$ second, which would amount to but one part in 2400 in the case of a ten minutes' observation. In consequence, however, of the imperfect constancy of the pitch of the harmonium notes, even when the blower is assisted by a pressure-gauge, further attempts at accuracy would be useless unless the comparison with the fork were simultaneous with the other observations. In that case the result would be entirely independent of variations in the harmonium notes, and no difficulty would be experienced in carrying out the method excepting the necessity for more observers.

THE FISSURES OF THE CEREBRAL HEMISPHERES IN UNGULATA

An important memoir by Dr. Krueg on the cerebral hemispheres of Ungulata has recently appeared in the *Zeitsch. wiss. Zool.* After a review of the previous papers that have appeared on this subject—but few in number—Dr. Krueg describes his method of investigation.

He made drawings of the hemisphere of each species; compared, first, line for line the hemispheres of different individuals of the *same* species, and then took as characteristic of that species every sulcus that was constantly present in all the individual hemispheres.

The different species in a genus and the different genera in the order were compared in the same way, so that by elimination he at last obtained a schematic drawing of the sulci constant throughout the order. In his drawings he represents those sulci peculiar to the individual, species, &c., by variously dotted lines, those running through the whole order by thick black lines.

Since the most constant or chief sulci are the first to appear in the course of development, he gives drawings of fœtal brains of the sheep, cow, and pig. With regard to these fœtal brains, it is highly important to note that in no instance were transitory or temporary sulcus markings met with; Meckel himself admits that he did not find these temporary sulci in brains of other animals, though he described them in the human fœtus. Dr. Krueg regards them as entirely artificial. The following, then, are the chief fissures or sulci (*Hauptfurchen*) constant throughout the Ungulata, and of these the first six are the most important in relation to those of the Carnivora:—

1. Fissura sylvii; ant., post., processus acuminis.
2. ,, splenialis.
3. ,, supra-sylvia, ant., post., supr.
4. ,, coronalis.
5. ,, præsylvia.
6. ,, lateralis.
7. ,, diagonalis.
8. ,, postica.
9. ,, genualis.
10. ,, rostralis.

h. ,, hippocampi.
rh. ,, rhinalis.
C.ca. Corpus callosum.

The earliest fœtus he possesses shows two fissures, namely, F. sylvii and F. splenialis. The sylvian fissure develops radially, just as Ecker has shown in the human fœtus, and since in the latter, the parieto-occipital fissure appears almost contemporaneously with the sylvian fissure, he considers the Fissura splenialis of Ungulates to be the homologue of the human parieto-occipital. This view derives some strength from the fact that as development proceeds, the anterior extremity of the Fissura splenialis turns upwards and gains the median border. Moreover, here it is opposite to the processus acuminis Sylvii, which is homologous with the posterior or horizontal ramus of the human Sylvian fissure.

The positions of the fissures are shown in the accompanying diagrams. As regards the fœtal forms it is noteworthy that in the Suillidæ, from its earliest appearance the posterior process of the F. supra-Sylvia is directed downwards as well as backwards; this we shall find is a family characteristic of importance.

Before enumerating the slighter distinctive characters of the families, or comparing the Ungulata with the Carnivora, it will be well to mention the results which are of more general importance. Dr. Krueg believes that he has established the following propositions:—

1. That the forerunners of the adult fissures are never transitory radial markings, but always present (though incompletely) the adult form.

2. That the two important fissures (F. hippocampi and F. rhinalis) common to all mammals, are the first to appear, and that next those characteristic of the Ungulata commence.

It is remarkable that the fissures peculiar to the individual, may appear contemporaneously with, or even precede, the last few of the chief fissures. This fact of itself would cast some doubt on the morphological value of these later chief fissures, and comparison with the Carnivora also diminishes their importance as diagnostic marks.

3. On no occasion was a fissure once formed ever broken up by a bridging convolution. The reverse of this may and often does occur, viz., that two originally distinct fissures may, by extension of their neighbouring extremities, so mingle as to form one large fissure. From this fact he concludes that when in adult brains we meet with a well-known fissure bridged over, originally this fissure was developed as two distinct ones. This would certainly explain the remarkable cases lately published by

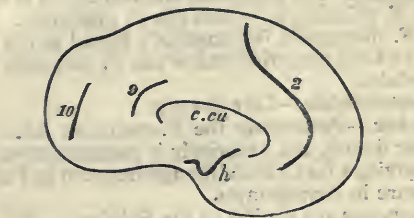


Fig. 1.—Median surface of typical hemisphere.

Henschel, in which the fissure of Rolando was bridged over, and such a state of things would be due to atavism.

4. The influence of the size of the animal on the shape of the hemisphere and its details is very important, and may be regarded as threefold.

(*a*) The number of accessory fissures increases with the size of the brain and the size of the animal.

(*b*) The shape of the hemisphere differs. Thus, in the larger animals it is broader, and more rounded, whereas, in the smaller animals it is distinctly narrower, and tapers more to a point anteriorly.

(*c*) In the smaller animals it is noticed at once that fissures (such as the splenialis), which in the schematic brain are situated on the median surface, in these smaller individuals, often appear on the upper. Such a condition may be supposed to result from a rotation of the upper or median border around the Island of Reil as a centre. This rotation Dr. Krueg has named "supination," and that in the opposite direction and occurring in the larger animals "pronation." The posterior extremity of the F. coronalis almost always presents a "horn" (Bügel) directed inwards. This branch becomes of importance, as it often joins the F. splenialis, and offers a homology with the fissura cruciata of the Carnivora.

The following are the main family characteristics:—

Tragulidæ.—Supination marked. F. coronalis communicates with the processus anterior supra-Sylvii. The Tragulidæ (like the antelopes) present strong elephantine characters as regards their fissures.

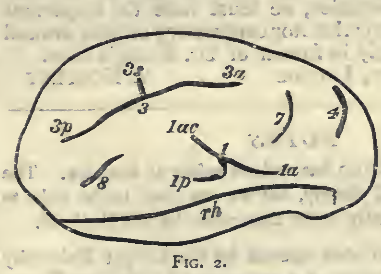


Fig. 2.

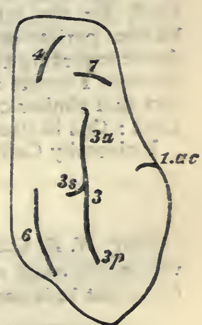


Fig 3.

Fig. 2.—Lateral outer surface of typical hemisphere. Fig. 3.—Upper surface of typical hemisphere.

Elephants.—F. coronalis communicates with the processus anterior supra-Sylvii, and also either with the F. splenialis or ends just behind it.

The *Giraffes* present no generic characters.

Cavicornidæ.—In the majority the F. coronalis does *not* communicate with the F. supra sylvii, the exception being *Bos taurus.* The processus acuminis Sylvii is broken

by "individual" fissures, and, moreover, the angle formed between its anterior and posterior extremities is raised, and into this space a small accessory branch from the Fissura rhinalis is directed.

Tylopoda.—Pronation is so marked that the F. lateralis is actually situated on the median surface. The F. coronalis is directed from the middle line forwards and outwards.

Suillidæ present several characters in common with the Carnivora. The processus anterior Sylvii is continued into the F. rhinalis, as is also the F. præsylvia.

The F. diagonalis is constantly joined to the F. supra Sylvia, and the posterior end of the F. coronalis joins the F. splenialis.

Hippopotamidæ.—Like the pig.

Tapiridæ.—Posteriorly the F. coronalis does not join any other fissure, but anteriorly it communicates with the F. præsylvia, a fact which, though frequent in Perissodactyls, is rare in Artiodactyls.

Nasicornidæ are similar to the Solidungulates.

Solidungulata.—The peculiarities are very constant. Thus there are cross-fissures between F. Sylvii and F. supra Sylvii. Further, there is an accessory longitudinal and parallel fissure on either side of the F. lateralis. Pronation is marked. The F. coronalis is united to the F. supra Sylvii, and the posterior "horn" of the F. coronalis is not united to that fissure, but commencing behind and above the anterior end of F. splenialis, runs forwards and outwards, ending before reaching F. coronalis.

Comparison of the convolutions of the Ungulata with those of the Carnivora shows that in the latter order the first six chief sulci of the Ungulata have undoubtedly their homologues. The question as to whether a F. diagonalis can be said to exist must still be left open, and the remaining three certainly do not always exist in the brain of the Carnivora, and when present are accessory only.

Dr. Krueg thinks that possibly the posterior "horn" (Bügel) of the Fissura coronalis is homologous with the Fissura cruciata in the Carnivora, but this is very uncertain, for it is questionable whether in the Cavicornia the posterior limb of the F. coronalis is homologous with the distinct one described above in the Equidæ. Certainly that of the Perissodactyl is very similar to the Fissura cruciata as regards its relation to the F. coronalis; but while in the Perissodactyl the posterior extremity begins *above* the F. splenialis, in all Carnivora it begins *below*. Further, the anterior end of the F. splenialis turning up to the median border has a plausible homology itself with the Fissura cruciata. It would be very interesting to have the time of appearance of the Fissura cruciata fixed, for the latter theory would compare it to the human parieto-occipital fissure. As confirmatory evidence regarding the homology of the Ungulate and Carnivorate Fissuræ coronales, we must note the important fact that similar cortical motor centres are situate around them: this is strongly in favour of Dr. Krueg's view.

University College, London V. HORSLEY

NOTES

PROF. W. K. CLIFFORD has arrived safely at Madeira. The voyage was rough and stormy, but we are glad to be able to report that he is markedly better than when he left England.

A SUBSCRIPTION has been opened by the Dorpat University for the erection at Dorpat of a monument to the late K. von Baer.

ON January 10 the Imperial Russian Academy of Sciences at St. Petersburg held its annual meeting, which was largely attended this year. The meeting was opened with the reading of the long list of deaths of members, foreign and Russian, during last year, and among whom we notice Regnault, Claude Bernard, Granville, and Bienaimé, at Paris; Hugo Hildebrandt, of Jena; Friis, of Upsala; Tornberg, of Lund; Lers, of Königsberg; and the well-known Russian archæologist Polyenoff; Kovalsky and Khanykoff, orientalists; and Davydoff, mathematician. Count Orloff-Davydoff, Baron Bühler, and Col. Prjvalsky were elected honorary members; and General Maiefsky, mathematician; General Stebnitzky, geodesist at Tiflis; Mr. Hind (as noted last week), Dr. John Muir (Edinburgh), Clausius (Bonn), Boisier (Geneva), Lavrofsky and Veselago were elected correspondent members of the Academy. The report on the museums of the Academy showed several most important acquisitions, among which we notice the immense and very rich collections of birds, fishes, and reptiles brought in from Central Asia by Col. Prjvalsky, during his second journey; a collection of skulls and bones of Steller's sea-cow, which inhabited, during the last century, the shores of the White Sea, but is now extinct, the collection being made by M. Phillipens on the shores of Behring Strait; and a complete skull of an Elasmotherium, presented by M. Knoblauch. There were, until now, only some teeth of this immense quaternary horse-like rhinoceros at the museum of the Academy, and a part of a skull at the British Museum, which had offered, we are told, a large sum of money to M. Knoblauch for the rarity. The skull was found close by Sarepta, on the banks of the Volga River. The Academy proposes to open next year for the public a large anthropological museum, the materials for which are already in the hands of the Academy; we heartily commend this step, as the museums of the Academy, when open to the public each Monday, are visited by masses of people (as many as 12,000 persons daily on holidays). The report on the works of the philological and historical branch of the Academy was presented by Prof. Suklomlinoff, who dwelt at length on the works of Prince Vyazemsky; and a very interesting paper on the correspondence between Catherine II. and Grimm was read by Prof. Groth. In this branch we notice a great undertaking by the Academy for the next year, being a dictionary of the Kurd language. This language has been very well studied, but there are no dictionaries of it. Now, the Academy will publish a complete one, the materials for it being given by the French orientalist, M. Szabo, and completed by M. Yulpi, who will be intrusted with this important publication.

WE notice an interesting work, just issued in Russia, by Prof. Rumishevich at Kieff, being a complete catalogue of all the medicinal and veterinary literature published in Russia during 1876.

WE learn that the St. Petersburg Academy of Sciences has intrusted M. Keppen with the publication of a complete catalogue of animals living in European Russia.

LORD DUFFERIN, Lord Rosse, and Prof. Roscoe received the degree of LL.D. from Trinity College, Dublin, on Tuesday.

THE "Telectroscope" is the name of a new apparatus, the plan of which was, *Les Mondes* states, recently submitted to MM. du Moncel and Hallez d'Arros by M. Senlecq, intended to reproduce telegraphically at a distance the images obtained in the camera obscura. This apparatus is based on the well-known sensitiveness of selenium to various shades of light.

PROF. EDWARD S. MORSE, we learn from the New York *Nation*, has written an interesting paper on the "Traces of an Early Race in Japan," which throws light on a subject hitherto wholly obscure. A race of men called Ainos are believed to have come down from Kamtchatka and to have taken possession of Japan, which they held until displaced in their turn by the Japanese from the south. Of the two races, the Ainos and the Japanese, authentic records exist, but nothing has been known concerning the ancient people whose territory was appropriated

by the Ainos. The only knowledge obtained of them has been ingeniously acquired by Mr. Morse by a careful study of "shell-heaps" in all respects similar to those found along the shores of Denmark, New England, and Florida. The deposit discovered by Mr. Morse near Tokio contained pottery and broken bones, many of which were human. It is generally admitted by ethnologists that a people that has once acquired the art of pottery will always retain it; but as neither the Esquimaux, the Kamtchatdales, nor the Ainos are essentially earthen-pot-makers, these remains naturally point to the former existence of a race in Japan who preceded the Ainos. Again, both the human and the deer bones found in this shell-heap were broken in a manner to facilitate the extraction of the marrow or to enable them to be placed in a cooking-pot, a circumstance which points to the existence of cannibalism among the people by whom the shell-heaps were made. On consulting Japanese scholars and archæologists Mr. Morse learned that the Ainos were not only not cannibals, but were of an especially gentle disposition. The existence of an ancient race of cannibals in Japan, before the occupation of that country by the Ainos, is therefore made very probable. We hope to see another paper before long containing an account of Prof. Morse's later researches.

PROF. HUMPHRY, F.R.S., of Cambridge, will deliver the biennial oration in memory of John Hunter in the theatre of the Royal College of Surgeons on the 14th proximo.

MR. THOMAS SOPWITH, M.A., F.R.S., F.G.S., who died at Westminster, on Thursday last, was born in 1803, at Newcastle-on-Tyne. He was for nearly fifty years extensively engaged as a civil engineer in mining, railway, and other works, both in this country and on the Continent, and was the author of several works on architecture, isometrical drawing, and mining. In 1838 he was appointed Commissioner for the Crown under the Dean Forest Mining Act, and in the same year a communication made by him to the British Association led to the establishment of the Mining Record Office. He was a member of many of the leading scientific societies, and one of the early members of the Institution of Civil Engineers.

IN connection with our article last week on a proposed Scottish observatory, it may be interesting to state that one day last August Mr. Milne Home, chairman of the Meteorological Society for Scotland, accompanied by Mr. Colin Livingston, headmaster, Public School, Fort William; Mr. Thompson, student; and Mr. David Doig, contractor, ascended Ben Nevis and made several observations with the view of erecting a station on the summit. They found the top enveloped in a mantle of snow—a circumstance which rendered it an extremely difficult task to select suitable spots for the erection of a dwelling-house and observatory. After a careful survey Mr. Home came to the conclusion that the plateau immediately beyond the spring affords the best site. The recommendation this spot has is its contiguity to the water-supply. But it might be questioned whether, as accurate observation is the thing required, it would not be better to erect the observatory on the plateau on the very summit, as there must, no doubt, be a difference between the temperature of the two places, the first-mentioned plateau being 350 feet lower. It is proposed to construct the buildings after the following plan: first, a wall of stone with an inside lining of wood and an inner coating of felting, and the outside of the wall to be covered with corrugated iron. An external wall of stone would also be erected to serve as a protection from the blast. The estimated cost of the structure is 500*l*.

WE learn from the *Colonies and India* that rich discoveries of copper have been made at Howe Sound, a few miles from New Westminster in British Columbia, and that the ore resembles that of the famous Australian Burra Burra mine.

THE Royal Society of Arts and Sciences of Mauritius has recently lost its secretary, Mr. L. S. Bouton, the only surviving founding member of the Society. This society was founded on August 24, 1829, under the name of Société d'Histoire Naturelle de l'Ile Maurice, by a few lovers of science; its first secretary was Julius Desjardins, who contributed many papers on the fauna of Mauritius, and also formed a good collection of specimens, which were afterwards given up to Government by his heirs, and became the nucleus of the present museum. The volumes published by the Société during a period of ten or fifteen years contain much interesting information on the natural history of Mauritius. On Desjardins' death in 1840, Mr. Bouton was appointed secretary, and kept up the post till his death; during that long period he chiefly applied himself to the investigation of the flora of Mauritius, and though he never published any complete works he contributed specimens and notes to Prof. Decandolle for the Prodromus, and to Kew, for the Flora of Mauritius by Baker. He wrote a paper on the medicinal plants of Mauritius, and a very interesting paper on the forests of Mauritius, besides a great quantity of notes in the Society's *Transactions*, and in the newspapers of the colony. In 1846 the Society was allowed by Government an annual subsidy of 200*l*., which has been continued up to this day. Mr. Bouton was also curator of the Museum, but he was, we believe, though completely devoted to his duties of secretary, a rather bad curator. The Museum, although containing some very interesting specimens of the natural history of Madagascar, and of the extinct fauna of Mauritius, was allow to decay rapidly. No exchanges were carried on to increase the collections, and the space being too limited, the existing specimens are so crowded as to be of no use whatever to the public. The subject attracted the attention of the late much-esteemed governor, Sir Arthur Phayre, and he applied for a Report from the Council of the Society, and on its recommendation the following decisions have been come to by the Council of Government:—That a proper building be provided for the transfer of the Museum now heaped up in a room at the Royal College; that the funds allowed by Government each year be applied to the formation of a local museum, fully illustrating the fauna and flora of the Mascarene Islands, Madagascar, and the islands along the east coast of Africa; that a general collection, of which the present museum should be the nucleus, be gradually formed by means of exchanges to illustrate only the principal genera in each branch of natural history, and give to the public a general view of the natural world; that on vacancy (which is now come) a competent curator be provided from home, who will be at the same time Professor of Natural History at the Royal College, receiving a salary of Rs. 5,000 per annum; that the staff of the Museum be composed of an assistant, who will be also a collecting n^ uralist, sent round every year to Madagascar or some other place, receiving a salary of Rs. 2,000 and his travelling expenses, a taxidermist, a clerk, and servants; that, as a good taxidermist does not now exist in Mauritius, the services of a proper person be secured from home for two years, to instruct people in stuffing and set up the first collection, receiving Rs. 2,000 and passage-money. We hope that these changes will be insisted upon and that competent men will apply for the vacant posts to the Government at home, and give them a better opportunity of making a good choice. Mauritius is an admirable place for studying the riches of the sea, and a sort of zoological station, like the one at Naples would make many interesting discoveries. Prof. Möbius, of Kiel, who spent some months at Mauritius, said that several years would be necessary for him and many assistants to work up the collections from these seas. Evidently a good opportunity is presented for the promotion of important departments of natural history, and we trust all concerned will seek only to advance the interests o science and the true interests of the colony.

THE Commission of the Municipal Council of Paris has drawn up a report on the working of the electric light, which has been printed, and was discussed on the 14th instant. A certain number of important facts are stated. A Jablochkoff lamp may be said to give a quantity of light equal to eleven gas lamps, consuming each 140 litres per hour. The quantity of gas consumed to produce the same quantity of illumination would be 1,540 litres per hour. The price paid by the city to the gas company for 1,000 litres being 0 fr. 15c., the expense would be of 0 fr. 23 c. The expenses of each Jablochkoff lamp are officially stated as follows, for 62 candles per hour of light, 77 horse-power:—Machinery, 3 fr. 20 c.; coals for working the several steam engines used, 6 fr. 64 c.; oil for lubricating, 1 fr. 23 c.; pay of men for changing candles and superintending illumination, 3 fr. 20 c.; expenses of 62 candles at 0 fr. 50 c. each, supposed to last during an hour, 31 fr. Total, 45 fr. 27 c., or 73 c. for each candle. The Commission proposes to pay to the Jablochkoff company—which accepts 0 fr. 30 c. per candle during one year—for 62 candles at the Avenue de l'Opéra, 15 on the Place de la Bastille, and 6 in a pavilion of the Halles Centrales: in all 83. The total number of burning hours is estimated at 2,073 for each of the street candles, and 4,000 for each of the pavilion candles: altogether, 55,000. The expense paid to the gas company for illuminating the same places is 21,041 fr. The excess of expense for the city will be 34,044 fr. But this credit is asked for in the interest of science. It is hoped that during one year the Jablochkoff company will realise material improvements, and it is supposed that other electric light companies will tender some fresh propositions for comparison. In the meantime, the Commission proposes to accept a tender made by the gas company to improve the illumination of the Rue du Quatre Septembre, Place du Château d'Eau, and a pavilion of the Halles Centrales, with an excess of consumption of 260,000 cubic metres. At the sitting of the 14th, the gas company refusing to accept the price offered to them as a compensation for their expenses, proposed to supply the gas gratis, which was agreed to. Consequently a regular competition will be carried on between gas and electricity before the Parisian public during one year, on a grand scale, at the expense of only 34,044 fr.

IT was recently affirmed in the French Academy that chromic acid might be substituted for vanadic acid in the manufacture of aniline black. An industrial chemist of Rouen, M. Witz, now points out to the Academy (by recommendation of Prof. Girardin) that this is an illusion, and that vanadium is absolutely necessary. Chromium gives a greenish product quite different. M. Witz insists on the small quantity of vanadium which suffices to develop the reaction. It appears that the black is produced in presence of a weight of vanadic acid equal to only the *hundred millionth* part of the weight of the aniline employed. In practice, a thousandth of this weight is quite sufficient, and it will be seen that notwithstanding the high price of vanadium, the use of it in such small quantities is quite practicable for manufacture.

THE Anthropological Exhibition which will be held at Moscow next summer promises to be a highly interesting one. A large series of graphic illustrations of the life of prehistoric man will be supplemented by numerous models of caves, skeletons, and other prehistoric objects. So-called "kurgane" (prehistoric tombs) will be represented containing models of the skeletons and other objects found in them, their various positions being exactly reproduced. Prehistoric skulls will form a separate department of the Exhibition.

WE have received the *Proceedings* of the Cleveland Institution of Engineers, containing the address of the president, Mr. John Gjers, at the annual meeting of November 11. The address refers to various topics of much interest to engineers and even to men of science. Among other things Mr. Gjers, speaking of the variation in the production of the soil, gives it as his opinion that it is undoubtedly connected with the variation in the number of sun-spots. The December number of the *Transactions* of the Institution of Engineers and Shipbuilders in Scotland contains a paper by Mr. James Howden, "On the Action of the Screw Propeller," followed by a long discussion, and another by Mr. W. G. Jenkins, "On the Scientific Form of Harbours as applied to the Port of Melbourne."

A FOREIGNER, who fears the disappearance of bears in the Alps, the killing of these animals being largely paid for in Switzerland by the State and by the communes, and several wild animals having already disappeared in Switzerland in this way, proposes to form a society which will pay for each disaster caused by bears, and prohibit the hunting of them.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. D. Orpen; a Black-faced Spider Monkey (*Ateles ater*) from South America, presented by Earl Brownlow, F.Z.S; a Common Seal (*Phoca vitulina*) from Scotland, presented by the Earl of Hopetown; a Dufresne's Amazon (*Chrysotis dufresniana*), a Yellow-Fronted Amazon (*Chrysotis ochrocephala*) from South America, presented by Mrs. T. Smith; a Noddy Tern (*Anous stolidus*) from Ascension Island, presented by Morris H. Smyth Long, Lieut. R.N.; a Tuberculated Lizard (*Iguana tuberculata*) from the West Indies, presented by Dr. Stradling; a Superb Tanager (*Calliste fastuosa*), a Yellow-winged Blue Creeper (*Cœreba cyanea*) from South America, two Merrem's Snakes (*Liophis merremi*) from Monte Video, deposited; two Cuming's Octodons (*Octodon cumingi*), born in the Gardens.

ON THE LAVAS OF HEKLA, AND ON THE SUBLIMATIONS PRODUCED DURING THE ERUPTION OF FEBRUARY 27, 1878

BUNSEN in a Memoir "On the Processes which have taken place during the Formation of the Volcanic Rocks of Iceland," published in Poggendorff's *Annalen* in 1851, classifies the rocks of the island into two principal groups, which he calls respectively the *normal trachytic*, and the *normal pyroxenic*. The one possesses the largest proportion of acid, and the other of base, and their composition may be approximately stated in the following analyses:—

	Normal trachytic Composition.	Normal pyroxenic Composition.
Silica	76·67	48·47
Alumina and protoxide of iron	14·23	30·16
Lime	1·44	11·87
Magnesia	0·28	6·89
Potash	3·20	0·65
Soda	4·18	1·96
	100·00	100·00

The trachytic rocks represent a mixture of bisilicates of alumina and of the alkalies potash and soda, while protoxide of iron, lime, and magnesia are almost wanting. On the other hand, the pyroxenic rocks are basic silicates of alumina and protoxide of iron, in combination with lime and magnesia, and insignificant quantities of potash and soda. In the trachytic rocks the percentage of alumina is from 10 to 12, and that of protoxide of iron from 2 to 4; while in the pyroxenic rocks the percentage of alumina is from 10 to 18, and that of protoxide of iron from 12 to 20. Normal trachytic rocks are found in great abundance on the banks of the Laxá, at Laugarfjall, near the great geyser, and at Krafla in the north-east of Iceland. The normal pyroxenic rocks are found on and around Hekla, on the banks of the Thjórsá, and at Thingvellir. Bunsen by an admirable induction, supported by a number of analyses, has proved that the rocks of Iceland which do not closely approximate in composition either to the normal trachytic or the normal pyroxenic, are intimate mixtures of these two classes of rocks,

and that hence in all probability there are but two separate volcanic foci.

Dr. Genth examined the various lavas from the western slope of Hekla, among them the lava erupted in 1845, which was found to contain—

Silica	56·76
Alumina and protoxide of iron	27·47
Lime	6·75
Magnesia	4·04
Potash...	2·63
Soda	2·35
	100·00

The lavas of Hekla are trachytic rather than pyroxenic. In the geological map of Iceland which appears in von Leonhard's *Vulkanen Atlas*, a broad strip, including more than half the area of the island, is designated trachyte. It is inclosed by lines running approximately north-east by south-west,—that on the west from Skjalfandik to Reykjavik, and that on the east from Héraðfloi, to the Orœfa Jökull.

I was surprised when I visited the scene of the eruption of February 27, 1878 (*vide* NATURE, vol. xviii. p. 596), to find how precisely the lava of this eruption resembled a very old lava in close contiguity to it, but flowing from a distant crater. Moreover the lava of 1845, on the other side of the mountain, and more than four miles from the craters of 1878, was observed to be quite the same in character as the most recent lava, which undoubtedly possesses a composition differing but little from that of the analysis given above.

The most notable feature of the last eruption appears to be the quantity of hydrochloric acid evolved from the beds of lava, and the considerable sublimations of sesquichloride of iron. Bunsen asserts that hydrochloric acid plays a less important part in the volcanic phenomena of Iceland than at Vesuvius and Etna.

"The hydrochloric acid fumaroles," he writes, "which not unfrequently occur on a large scale near the Italian volcanoes, and are then generally accompanied by a very considerable sublimation of chloride of sodium, appear to be of less importance in Iceland. I was only able to detect traces of hydrochloric acid in a free state in the crater fumaroles a few months old, which owed their origin to the last eruption of Hekla" (in 1845), "as well as in the exhalations of vapour from the lava which was then erupted."

For the future we must recognize hydrochloric acid as one of the products of the volcanic action of Hekla. During the last eruption it was produced in considerable quantity.

I had not proceeded far by the side of the lava of 1878, erupted five months previously, before I saw patches of brilliant red and yellow sublimations on the lava. These I naturally mistook for sulphur, but on a closer approach, warm vapours of hydrochloric acid were found to be issuing from the lava, and the sublimations when removed from the lava speedily deliquesced, forming an intensely acid and corrosive solution of sesquichloride of iron. I only succeeded in bringing one specimen of this sublimate to England, and this can scarcely be wondered at, when we remember that it had to be carried over 150 miles of very rough and pathless country before reaching the sea-coast. Moreover, as ill-luck would have it, the pony which was carrying my minerals, took fright during the last hour of a journey of many days, and within a few miles of Reykjavik our final destination; the box containing the specimens was broken to pieces, and they were scattered on the ground, but fortunately without much injury.

The specimen of chloride of iron sublimate has been qualitatively analysed in our school laboratory, by H. M. Elder, who finds it to contain in addition to sesquichloride of iron and free hydrochloric acid, chloride of aluminium, and very small quantities of the chlorides of ammonium, sodium, and calcium.

During our journey to the scene of the eruption of 1878, we frequently saw large patches of this sublimate, and near one of the new craters, in an inaccessible portion of the lava field, an area of several hundred square yards was covered with it. Most clearly therefore a notable feature of the eruption of 1878 has been the emission of large quantities of hydrochloric acid.

The formation of this substance during the eruption is easy to account for. Sublimations of a white substance were frequently visible in the crevices of the new lava. These, according to Herr Nielssens of Eyrabakki, consist of chloride of sodium, not of chloride of ammonium. Professor Silvestri found in different sublimations in the lava of Etna, erupted in 1865, quantities of chloride of sodium which varied from 50 to 90 per cent. (*I Fenomeni vulcanici presentati dall'Etna nel* 1863-6, page 139-142). Chloride of sodium, if it be heated in contact with silica and steam, undergoes decomposition, silicate of sodium and hydrochloric acid being formed. Bunsen has pointed out the fact that hydrochloric acid fumaroles can only exist when the high degree of temperature necessary for the decomposition of the chloride of sodium, has not receded far below the surface. For if it has so receded, the hydrochloric acid before reaching the surface will necessarily act upon the contiguous rocks, with the formation of chlorides which do not possess a sufficient degree of volatility to be brought to the surface. In the case of the sublimations in the lava of 1878, I noticed both free hydrochloric acid and sublimated chlorides, but the former was small in quantity, and no doubt the sublimations are receding deeper into the mass as the lava cools, and the next observer may find no trace either of the hydrochloric acid, or the sublimate of sesquichloride of iron.

G. F. RODWELL

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

IN addition to the regular course of instruction in the principles and practice of weaving at the Weaving Branch of the Glasgow Technical College, the Directors have made arrangements for three special courses of lectures which are now in course of delivery. These comprise a series on the history and development of the power loom, by Mr. John Watson, author of a "Treatise on Weaving"; another on Dyeing, by Mr. Noble; and a third series on Vegetable Fibres, by Mr. James Paton, F.L.S., Curator of Kelvingrove Museum.

THE Government of St. Petersburg, as we learn from the annual report just issued, had on December 1, 1878 (exclusive of the capital) 53 primary schools of the Ministry of Public Instruction, with 2,262 boys and 1,022 girls; 295 schools depending upon the School Boards, with 10,023 boys and 3,519 girls, and 21,975*l.* yearly expenses; and about 30 schools of separate institutions, with 1,380 boys and 1,533 girls.

WE learn that a Russian lady, Mme. Berladskaya, has just received the degree of Doctor of Medicine at the University of Paris, after having defended at a public meeting her thesis, "On the Structure of Arteries." This paper was spoken of in the highest terms by Prof. Charcot. Mme. Berladskaya is the second lady who has received the degree of Doctor of Medicine at Paris, the first having been Mme. Goncharoff.

SCIENTIFIC SERIALS

THE *Proceedings of the Linnean Society of New South Wales*. Vol. ii. part 4, and vol. iii. part 1. Part 4, vol. ii. contains: Prof. R. Tate, descriptions of three new species of helix from South Australia; Rev. J. E. Tenison-Woods, on the extra-tropical corals of Australia, three plates; the same, on the Echini of Australia, supplementary; W. Macleay, on the fishes of Port Darwin, four plates; John Brazier, on the mollusca of the Chevert expedition; the same, on some recently-found mollusca from Port Jackson and New Caledonia; E. P. Ramsay, on a new species of Rhipidura and of Eopsaltria from the Rockingham district, with remarks on some rare Queensland birds; the same, on a specimen of *Arses telescophthalmus*, on *Arses kaupi*, and on the young of *Cracticus quoyi;* the same, note on *Casuarius australis*, one plate; W. Stephens, the President, the annual address. Part 1, vol. iii. contains: E. P. Ramsay, on a new species of Ptilotis from Torres Straits; on a species of Myolestes from Fiji; notes on list of Australian birds; and descriptions of five new species of birds from Torres Straits and New Guinea; Rev. J. E. Tenison-Woods, on an Australian variety of *Neritina pulligera;* on a new genus (Arachnopora) of Milleporidæ; on a new species of Passamoseris; on a new species of Desmophyllum, and on a young stage of *Cycloseris sinensis;* on some Australian Littorinidæ; W. Macleay, note on a species of Therapon found in a dam at Warialda; on some new fishes from Port Jackson and King George's Sound; on a new species of Hoplocephalus; on the powers of locomotion in the Tunicata; C. Jenkins, on the geology of Yass Plains; Count de Castelnau, on the fishes of the Norman River.

SOCIETIES AND ACADEMIES

London

Royal Society, January 9.—"On the Electromagnetic Theory of the Reflection and Refraction of Light," by George Francis Fitzgerald, M.A., Fellow of Trinity College, Dublin. Communicated by G. J. Stoney, M.A., F.R.S., Secretary of the Queen's University, Ireland.

I have thrown the expressions for the electrostatic and electrokinetic energy of a medium given by Prof. J. Clerk-Maxwell in his "Electricity and Magnetism," vol. ii. part iv. chap. 11., into the same forms as M'Cullagh assumed to represent the potential and kinetic energy of the ether, in "An Essay towards a Dynamical Theory of Crystalline Reflection and Refraction," published in vol. xxi. of the *Transactions* of the Royal Irish Academy. Following a slightly different line from his, I obtain, by a quaternion and accompanying Cartesian analysis, the same results as to wave propagation, reflection, and refraction, as those obtained by M'Cullagh, and which he developed into the beautiful theorem of the polar plane. Of course, the resulting laws of wave propagation agree with those obtained by Prof. Maxwell from the same equations by a somewhat different method. For isotropic media, the ordinary laws of reflection and refraction are obtained, and the well-known expressions for the amplitudes of the reflected and refracted rays.

In the second part of the paper I consider the case of reflection at the surface of a magnetised medium, adopting the expressions Prof. J. Clerk Maxwell has assumed in "Electricity and Magnetism," vol. ii. part iv. § 824, to express the kinetic energy of such a medium.

I show that the method adopted in my former paper on Magnetic Reflection in the *Proceedings* of the Royal Society for 1876, No. 176, is justified, and that it is legitimate to consider an incident plane polarised ray as composed of two oppositely circularly polarised rays, each of which is reflected according to its own laws. I consider next the cases of the magnetisation being all normal to the surface, and all in the surface and the plane of incidence, and obtain the following result: When the incident ray is plane polarised, and the plane of polarisation is either in or perpendicular to the plane of incidence, the effect of magnetisation is to introduce a component into the reflected ray perpendicular to the original plane of polarisation, which vanishes at the grazing and normal incidences, and, in the case of iron, attains a maximum at about the angle of incidence $i = 63° 20'$.

I do not obtain any change of phase by reflection in any case; and this is to be expected, as this change of phase probably depends on the nature of the change from one medium to another, which, following M'Cullagh, I have uniformly assumed to be abrupt. Apart from this question of change of phase, my results conform completely to Mr. Kerr's beautiful experiments on the reflection of light from the pole of a magnet, as published in the Philosophical Magazines for May, 1877, and March, 1878.

"On Dry Fog," by E. Frankland, D.C.L., F.R.S., Professor of Chemistry in the Royal School of Mines.

January 16.—"Concluding Observations on the Locomotor System of Medusæ," by George J. Romanes, M.A., F.L.S. Communicated by Prof. Huxley, Sec. R.S.

The principal bulk of the paper is devoted to a full consideration of numerous facts and inferences relating to the phenomena of what the author terms "artificial rhythm." Some of these facts have already been published in abstract in the *Proceedings* of the Royal Society" (vol. xxv.), and to explain those which have not been published would involve more space than it is here desirable to allow. The tendency of the whole research on artificial rhythm, as produced in various species of Medusæ, is to show that the natural rhythm of these animals (and so probably of ganglio-muscular tissues in general) is due, not exclusively to the intermittent nature of the ganglionic discharge, but also in large measure to an alternate process of exhaustion and restoration of excitability on the part of the responding tissues—the ganglionic period coinciding with that during which the process of restoration lasts, and the ganglionic discharge being thus always thrown in at the moment when the excitability of the responding tissues is at its climax.

Light has been found to stimulate the lithocysts of covered-eyed Medusæ into increased activity, thus proving that these organs, like the marginal bodies of the naked-eyed Medusæ, are rudimentary organs of vision.

The polypite of *Aurelia aurita* has been proved to execute movements of localisation of stimuli somewhat similar to those which the author has already described as being performed by the polypite of *Tiaropsis indicans*.

Alternating the direction of the constant current in the muscular tissues of the Medusæ has the effect of maintaining the make and break stimulations at their maximum value; but the value of these stimulations rapidly declines if they are successively repeated with the current passing in the same direction.

In the sub-umbrella of the Medusæ waves of nervous excitation are sometimes able to pass when waves of muscular contraction have become blocked by the severity of overlapping sections.

Exhaustion of the sub-umbrella tissues—especially in narrow connecting isthmuses of tissue—may have the effect of blocking the passage of contractile waves.

Lithocysts have been proved sometimes to exert their ganglionic influence at comparatively great distances from their own seats—contractile waves, originating at points in the sub-umbrella tissue remote from a lithocyst, and ceasing to originate at that point when the lithocyst is removed. A nervous connection of this kind may be maintained between a lithocyst and the point at which the waves of contraction originate even after severe forms of section have been interposed between the lithocyst and that point.

When the sub-umbrella tissue of *Aurelia* is cut throughout its whole diameter, the incision will again heal up, sufficiently to restore physiological continuity, in from four to eight hours.

Chemical Society, January 16.—Dr. Gladstone, president, in the chair.—The following papers were read: On the action of isobutyric anhydride on the aromatic aldehydes, by W. H. Perkin. The author has studied the action of isobutyric anhydride on cuminic aldehyde, hydride of benzoyl, cinnamic aldehyde paroxybenzoic and anisic aldehydes. The bodies formed are respectively β isopropylbutenylbenzene, β butenylbenzene, butenylcinnamene, parabutenylphenol, and β parabutenylanisoïl.—On two new methods for the estimation of minute quantities of carbon and their application to water-analysis, by Drs. Dupré and Hake. The first consists in burning the substance in a current of oxygen in a combustion tube with oxide of copper, absorbing the carbonic acid in a Pettenkofer tube with baryta water, filtering off the barium carbonate with great care, converting it into chloride, then into sulphate, and weighing; in the second, the carbonic acid passes into a 2 per cent. solntion of basic acetate of lead, and the turbidity compared with that produced by a solution of carbon of known strength in a Mills colorimeter.—On stannic ethide, by Dr. Frankland and Mr. A. Lawrance. By treating zinc ethyl with successive quantities of fused stannous chloride, the authors have prepared stannic ethide with great facility, they have also examined its properties, and specially investigated the action of sulphurous acid.—On aurin, by R. S. Dale and C. Schorlemmer. The authors have prepared pure aurin with great care, and confirmed the formula which they have already assigned to it. They have also studied ammonia aurin, tetrabromaurin, and the compounds formed by aurin with acetic, sulphuric, hydrochloric, and nitric acids.—On the derivatives of diisobutyl, by W. Carleton Williams.—On the action of chlorine upon iodine, by J. B. Hannay. The author confirms his previous conclusion, viz., that a body containing one atom of iodine and four atoms of chlorine does not exist.

Geological Society, January 8.—Henry Clifton Sorby, F.R.S., president, in the chair.—Charles Barrington Brown, Carl Fischer, M.D., F.L.S., William Coles Paget Medlycot, were elected Fellows; and Dr. F. V. Hayden, Washington, and M. Jules Marcou, Salins, Foreign Members of the Society.—The following communications were read:—On some tin-deposits of the Malayan Peninsula, by Patrick Doyle, C.E. (Communicated by the Rev. T. Wiltshire, F.L.S.) The tin-ore of the Malayan Peninsula is obtained from "stream-works" in an alluvial plain extending between a range of granitic mountains and the sea. The author describes the mines of the district of Larut Perak. The ore is got in open workings at an average depth of about 10 feet. The tin-bearing stratum has an average thickness of 4·87 feet; it is overlain by stratified sand and clay, and rests upon either porcelain clay or, sometimes, a sandstone. The ore varies from a fine sand, near the sea, to a coarse gravel, near the mountains, and is mixed with quartz, felspar, mica, and schorl. The author is of opinion that the stratum of ore has been derived from the granite of the mountain range, in which it still occurs in veins,

by denudation, and under conditions which still exist, though in a modified form.—Description of fragmentary indications of a huge kind of Theriodont reptile (*Titanosuchus ferox*, Owen), from Beaufort West, Gough Tract, Cape of Good Hope, by Prof. R. Owen, C.B., F.R.S. The author stated that among the fossils recently sent to the British Museum from the Cape of Good Hope by Mr. T. Bain, there were two boxes containing specimens of a most unpromising character, there being in them no entire bones, but only numerous more or less water-worn fragments. Among these was found a portion of a maxillary showing some traces of teeth; and sections having been made of this bone, the remains of several teeth were displayed, including a canine, the preserved portion of the socket of which was 4½ inches long. From the number and mode of implantation of the teeth, the author concluded that the animal to which they belonged resembled the Theriodont genera *Galesaurus* and *Galenops*. The anterior portion of the left ramus of the lower jaw, measuring 7½ inches in length, showed teeth presenting close analogies with those of Theriodonts, and this alliance was confirmed by the study of other fragments. Some of the characters presented by these remains seem to suggest affinities with the carnivorous mammalia, such as have been already indicated by the humeri of Theriodonts and Carnivores. The canine tooth of the new South-African reptile, which the author proposes to name *Titanosuchus ferox*, was six times as long as that of the allied form *Lycosaurus*; and we have in *Titanosuchus* evidence of a carnivorous reptile of more carnassial type than *Machairodus* and other felines. The author suggests that *Titanosuchus* found its prey in the contemporary *Pareiosauri*, Oudenodonts, and Tapinocephalans of the same locality.—Notes on the consolidated beach at Pernambuco, by J. C. Hawkshaw, M.A., F.G.S. The consolidated beach at Pernambuco, which has already attracted considerable notice, is a ridge of sandstone from 25 to 75 yards wide, and, as shown by borings made under the author's direction, from 10 to 13 feet thick. The landward or higher edge is nearly at the spring-tide high-water level, and it slopes seaward; the river (with a depth of 28 feet at low water 60 feet from the rock) flowing along the former face. The rise and fall of spring tides is 7 feet. Beneath the above rock is a stratum of sand with shells and stones about 8 feet thick, and then a second layer of sandstone rock. The consolidated beach is cemented by carbonate of lime, which the author considers to have been deposited by the action of water percolating through the rock, probably when the level of the land differed somewhat from what it is at present. He thinks it possible that this and other similar beaches on the Brazilian coast may mark periods of repose in the slow vertical movements which the coast has undergone.

Zoological Society, January 14.—Prof. Newton, F.R.S., vice-president, in the chair.—Dr. Traquair, F.R.S.E., exhibited a specimen of the Hackled Pigeon (*Alectorœnas nitidissima*) recognised, last September in the Museum of Science and Art in Edinburgh, by Prof. Newton, F.R.S., M.A., who made some remarks on the species showing (1) that it was peculiar to Mauritius, (2) that it is now wholly extinct, and (3) that only three specimens of it are known to have been preserved.—The Secretary read an extract from a letter received from Commander Hoskins, R.N., of H.M.S. *Wolverine*, on the subject of the range of the Mooruk, stating that no traces of the existence of this bird could be found in New Ireland.—An extract was read from a letter addressed to the secretary by the Rev. George Brown, giving additional particulars on the same subject.—The Secretary read an extract from a letter addressed to him by Mr. R. Trimen, F.Z.S., of Cape Town, on the subject of the true locality of the Black Spurwinged Goose (*Plectropterus niger*), which he had ascertained had been brought to Cape Town from Zanzibar.—A communication was read from Dr. Morrison Watson and Dr. Alfred H. Young, on the anatomy of the Spotted Hyæna (*Hyæna crocuta*).—A communication was read from Mr. A. D. Bartlett, giving an account of the habits and changes of plumage of Humboldt's Penguin, as observed in a specimen which had been recently living in the Society's Gardens.—A communication was read from Dr. O. Finsch, C.M.Z.S., containing an account of a collection of birds made by Mr. Huebner, on Duke of York Island and New Britain.—A communication was read from Mr. Edward J. Miers, F.Z.S., describing a collection of crustacea, made by Capt. H. C. St. John, R.N., in the Corean and Japanese Seas. The present paper related to the Podophthalmia of the collection, of which groups twenty-six species were described as apparently new to science.—A communication was read from Count T. Salvadori, C.M.Z.S., containing critical remarks on Mr. Elliot's paper on the Fruit-pigeons of the genus *Ptilopus*, lately published in the Society's *Proceedings*.—A communication was read from the late Marquis of Tweeddale, F.R.S., containing the twelfth of a series of contributions to the ornithology of the Philippines. The present paper gave an account of the collection made by Mr. A. H. Everett in the Island of Basilan.—Dr. A. Günther, F.R.S., gave an account of the mammals, reptiles, batrachians recently collected by Mr. Everett in the Philippine Islands, and called special attention to a new form of snakes of the family Calamariidæ, of which one example had been obtained. This snake, which was remarkable as possessing no external rudiments of eyes, was proposed to be called *Typhlogeophis brevis*.

Mineralogical Society of Great Britain and Ireland, January 7.—General Meeting.—Mr. H. C. Sorby, F.R.S., president, in the chair.—The following papers were read or taken as read:—On pilolite, an unrecognised species, by Prof. M. F. Heddle, M.D.—On so-called green garnets from the Urals, by Prof. A. H. Church, M.A.—On the magnetism of rocks and minerals, by J. B. Hannay, F.C.S.—On the celestine and baryto-celestine of Clifton, by J. N. Collie, communicated by W. W. Stoddart, F.G.S.—On some silicates of copper, by Wm. Semmons, president of the Liverpool Geological Society.—Contributions towards a history of British meteorites, by T. M. Hall, F.G.S.—Notes on some crystals of iron, by Amos Beardsley, F.G.S.—Notes on massive and crystallised cronstedite from Wheal Jane, by A. K. Barnett, F.G.S.—A large number of Members and Associates were elected by the Council previous to the meeting.

EDINBURGH

Royal Society, January 6.—Prof. Kelland, president, in the chair.—Mr. James Blyth gave notes on some experiments with the telephone. When the ends of two wires attached to the telephone were rubbed against one another and kept at a high temperature a grating sound was heard in the telephone, which diminished as the temperature was lowered. The sound, however, did not quite cease when the ends of both wires were cold. In this case the sound was louder and more distinct when the wires were attached to two files which were rubbed against each other. The experiment was modified by attaching one wire to the file and the other to a vice. Different substances—brass, carbon, zinc, iron, steel—were then screwed into the vice and rubbed by the file, but not much difference was observed between the effects produced. Another modification consisted in attaching the second wire to the axle of the fly-wheel of a lathe. In the last case the sound was very loud and distinct when the file attached to the other wire was held hard against the wheel as it revolved. A sound was also heard in the telephone when a hammer was made to strike a body—the hammer and the body being each connected to one of the telephonic wires. The sound was distinct but not so loud as with the rubbing. The sound was very loud when a large toothed wheel driven fast was used, and against which a strong spring struck, the one wire being attached to the wheel the other to the spring. Here there is a combination of striking and friction. Mr. Blyth suggested that these currents might be due to thermo-electric action or might be the electricity which Sir Wm. Thomson considers as the probable cause of friction.[1] The experiment was again varied by connecting one wire to the style of a phonograph and the other with the screw; there were two Bunsen cells in the circuit, which was completed by the style and cylinder. When the phonograph was spoken into, a person in a distant room could hear by means of the telephone. This seemed to show that the style presses unequally on the tinfoil and hence that although magnified copies of the curve on the tinfoil may be obtained by multiplying levers, these copies do not necessarily represent the motion of the style.—Prof. Tait gave a note on the measurement of beknottedness. The former measure was the smallest number of crossings whose signs must be changed to take off all the knotting. An objection to this was that these seemed to have no direct connection with the electro-magnetic measurement. The new method consists in drawing the knot in two parallel curvilinear lines easily distinguishable from each other by colour or formation, the one knot being thus wholly within the other. A knot is cut across through the symmetrical angle, and the ends joined again.—Prof. Tait gave a preliminary note describing some experiments

[1] Bakerian Lecture, 1856, *Phil. Trans.*, foot-note to second page.

he was making for the purpose of measuring what is known as the "Thomson Effect," viz., the convection of heat by electric currents from a cold to a hot part of a bar, or *vice versâ*. The method had occurred to him while testing the electric conductivity of bars heated for Forbes' conduction experiment.—A paper was read by Dr. Macfarlane and Mr. P. M. Playfair, on the disruptive discharge of electricity, in confirmation of former experiments of a similar nature. They found former anomalies with sparks of more than a certain length between two spheres, to be due to discharge by small sparks, and beyond that to escape into the air from the insulated wire. In the case of discharge between a plate and a point, there was a gradual increase in the difference of potential. Up to a certain limit the sparks were white; beyond that the sparks were violet, and there was very slight increase in the difference of potential required. On discharging through solid paraffin it was found that the first spark was by far the largest, and on examination the paraffin was found perforated in a zigzag manner, and the sides of the perforation were charred. The solid paraffin had twice as great electric strength as the same paraffin in the liquid state, and five times the electric strength of air. They found that the electric strength was a very definite method of distinguishing between different paraffins, but somewhat difficult of application.—Prof. Tait showed some pieces of sheet or tape india-rubber which Mr. Maclachlan of Mitcham had used to insulate wires, and which, after being stretched for some years, were found to be permanently strained; but they immediately regained their former dimensions on being dipped into hot water. The same phenomenon was true, he found, of india-rubber which, while warm, was stretched out nearly to rupture, and then kept stretched till cold. Prof. Clerk Maxwell had found a similar property true of gutta-percha pulled out when cold after being boiled. On heating it before a fire it took a peculiar form.

VIENNA

Imperial Academy of Sciences, December 5, 1878.—On twins; a contribution to human physiology, by Dr. Göhlert.—On the diffusion of liquids, by Prof. Stefan.—Determination of the path of the third comet of 1877, by Herr Zellr.

December 12, 1878.—On the fish species in the two lakes of Lower Austria, the Erlaph and Lunzer Lakes, by Dr. Fitzinger.—New observations on sounding air columns, by Prof. v. Lang.

PARIS

Academy of Sciences, January 13.—M. Daubrée in the chair.—The following papers were read:—On the construction of bridge-arches realising the maximum of stability, by M. Villarceau.—Researches on ozone and on the electric effluve, by M. Berthelot. Oxygen (1 vol.) and hydrogen (2 vols.) do not combine under action of the effluve, though the tension be nearly that which gives, through air, sparks 7 to 8 ctm. long. O will combine with the metals, sulphurous acid, nitrogen, &c., under such conditions. CO (2 vols.) and O (1 vol.) combine under like tensions; but the reaction is incomplete; and even with excess of O it is so. The effluve, acting on a mixture of CO_2 and O partly decomposes the former, and the O contains ozone; acting on pure CO_2 in a space without mercury or oxidable bodies, the effects point apparently to the existence of percarbonic acid.—On the formation of ethers of hydracids in the gaseous state, by M. Berthelot.—Are there, among low organisms, species exclusively *aërobies*, and others exclusively *anaërobies?* Should all these beings be ranged in two or three classes (Pasteur) or in one only? by M. Trecul. He argues for one class only, each species being capable of presenting at once one or several *aërobian* states, and one or several *anaërobian*.—Reply to M. Berthelot, by M. Pasteur.—Researches on the compressibility of gases, by M. Cailletet. He describes the manometer he uses; a tube of soft steel wound helically round a vertical cylinder, by turning which the tube is sent down a deep pit or wound up again. The lower end of this tube is connected with a laboratory-tube, in which is inclosed the piezometer containing the gas, and mercury is introduced into the apparatus. This tube is suspended by a fine graduated steel wire, the length of which unrolled measures the pressure. M. Cailletet tabulates his numerical results with nitrogen, which, it appears, contracts at first more than according to Mariotte's law; its compressibility then decreases (as in the case of air). It is about a pressure of 70 metres of mercury, that the gas presents this curious maximum.—The polymorphism of *Agaricus melleus*, Vahl., by M. Planchon.—Experiments relating to the action of waves on beaches and on artificial rock-work, by M. De Caligny. He reproduces in an artificial canal effects noticed at the rock-work of Cherburg, where large waves which, at low water, rolled the blocks towards the summit of the talus, had an undermining effect at high water.—M. Monot presented some specimens of results he has obtained in manufacture of various kinds of crystal.—The phylloxera in Panama, on the *Vitis caribæa*, D.C., by M. Collot.—On the employment of oil of asphalte against phylloxera, by M. Berton. Some one told him, when exploring the Dead Sea, that this oil had saved the vineyards of Judea from a worm (phylloxera?).—Letter to the President of the Commission on phylloxera, by M. Truchot.—MM. Felson and Chartre communicated a detailed catalogue of those erratic blocks most remarkable as regards the history of glacial phenomena.—The General-Inspector of Navigation presented data concerning flood and low-water of the Seine in 1878.—Observations of Saturn's satellites, at the Observatory of Toulouse, in 1877 and 1878, with the large Foucault telescope, by M. Baillaud.—New compound prism for direct-vision spectroscope of very great dispersive power, by M. Thollon. This sulphide of carbon prism is closed laterally by crown glass prisms, whose refringent angles are in opposite direction to that of the sulphide. The compound prism gives the enormous dispersion of 2'·0 angular distance of the D-lines, as compared with 45" for sulphide of carbon. Substituting the new prisms in his former spectroscope, he got a dispersion equivalent to that of 16 sulphide of carbon prisms of 60°, or 31 prisms of index 1·63. It gave 12' angular distance in the D-lines, and it presents quite new aspects of the spectrum. All the lines (newly) resolved were found to belong to different substances.—On M. Thollon's spectroscope, by M. Laurent.—On determination of the variations of level of a liquid surface, by M. Renou. A claim of priority.—Synthesis of uric derivatives of the series of allophane, by M. Grimaux.—Action of diastase, saliva, and pancreatic juice on starch and glycogen, by MM. Musculus and De Méring.—New observations on the development and metamorphoses of Taenias, by M. Megnin. Certain unarmed and armed Taenias are two adult forms of the same worm, their differences due exclusively to the conditions of their development.—Observations on Majorca and Minorca (continued), by M. Hermite.—New observations on the danger of use of powdered borax in meat-preserving, by M. Le Bon.

CONTENTS

THURSDAY, JANUARY 30, 1879

THE ART OF SCIENTIFIC DISCOVERY

The Art of Scientific Discovery, or the General Conditions and Methods of Research in Physics and Chemistry. By G. Gore, LL.D., F.R.S. (London: Longmans, Green and Co., 1878.)

IT is not easy to say when scientific research, using the expression in its strictest sense, was first commenced. M. Libri remarks: "Les recherches des Pythagoriciens sur les vibrations des corps, sont les plus anciennes expériences de physique qui soient parvenues jusqu'à nous." Archimedes must certainly be credited with some knowledge of research; and to a lesser extent Ptolemy the astronomer, and Hero, of Alexandria. But, as a matter of fact, experimental researches in physics were not made before the epoch of Galileo, nor in chemistry before the epoch of Lavoisier. The discovery of new methods of mathematical analysis on the one hand, and the invention of instruments of precision on the other, were necessary forerunners of the development of research. Moreover, the advocacy of the abandonment of that blind reverence for authority which had retarded the progress of the sciences for many centuries, tended in the same direction. In this respect, whatever we may say of Campanella, Nizolius, Telesius, and others, our own Francis Bacon did more true service than any of his predecessors; and we must always regard his writings as the most potent engine concerned in the overthrow of Aristotelianism, Scholasticism, and the method of pure logic, and in the substitution of the experimental method blended with just logical induction and deduction.

Mr. Gore, whose own devotion to experimental research well entitles him to act as an interpreter of the art of scientific discovery, has in the course of sixty chapters of condensed matter discussed the various lines of thought and of action which converge towards that bright central focus in which new truths lie hidden. His object has been to describe the nature, the methods, and the conditions of success of original scientific research; to point out the causes of failure, the mental and manual discipline by which they may be overcome; and the special modus of thought by which we may hope to ascend from the known to the unknown.

With this object in view he has divided the work into five parts, the first of which contains a general view of the subject:—the nature of scientific ideas, terms, and beliefs, the criteria of scientific truth, and the great principles of science. In the second part he has discussed the general conditions of scientific research:—the starting points, chronological order of discovery, importance of qualitative knowledge, and necessity of classification. The third part is devoted to the personal preparation for research; the fourth to the actual working in original research; and the fifth to special methods of discovery. This latter is divided into ten parts, which treat respectively of discovery:—

1. By extending undeveloped or neglected parts of science.
2. By the use of new or improved instruments.
3. By the investigation of likely circumstances.
4. By devising hypotheses and questions, and testing them.
5. By means of new experiments and methods of working.
6. By means of additional, new, or improved observations.
7. By classifying and comparing known truths.
8. By means of study and inference.
9. By means of new or improved methods of intellectual operation.
10. By means of calculations based upon known truths.

In the discussion of these subjects, the history of various scientific discoveries is traced, and we are not only brought into contact with the investigator's particular train of thought throughout all the steps which led up to the discovery, but we are often taken into the minuter labyrinths and shown the many collateral ideas which were evoked during the course of the research. The influence of previous discoveries upon the main subject at issue is also developed, so that we gain important information regarding the history of the sciences, while at the same time we are becoming acquainted with the art of original research.

In that portion of the work which relates to method, we are not surprised to find that the author has often quoted Lord Bacon. In fact, Mr. Gore's style is sometimes thoroughly Baconian. So penetrated is he by the spirit of the "Novum Organum," that he sometimes unconsciously embodies its aphorisms with his own; for example, when he says: "Science is the interpretation of nature, and man is the interpreter. Original research is the chief source of new scientific knowledge." His work may almost be called a nineteenth century continuation of the second book of the "Novum Organum"—a sort of *newest* organum. He also quotes pretty frequently the "History of the Inductive Sciences," and sometimes the "Novum Organum Renovatum," with which the "Art of Scientific Discovery" has many points of contact. We are surprised to find Descartes so rarely alluded to, albeit portions of the work relating to Method are thoroughly Cartesian in spirit. Here, for example, is an excerpt from the *Regulæ ad Directionem Ingenii*, which we recommend to Mr. Gore's notice for the second edition:—"By method I understand rules certain and easy, such as to prevent any one, who shall have accurately observed them, from ever assuming what is false for what is true, and by which with no effort of mind uselessly consumed, but always by degrees increasing science, a person will arrive at a true knowledge of all those things which he will be capable of knowing." Also we commend to his notice the answer to *Quid sit Cogitatio* ("Principia," Pars 1, ix.); and to that very notable assertion ("Principia," Pars 2, xxiii.), *Omnem materiæ variationem, sive omnem ejus formarum diversitatem pendere à motu.*

Early in the work Mr. Gore points out a fact which we too seldom recognise. "Original research," he writes, "is not a science; it is not a collection of laws. It is an art, because it is composed of rules which must be followed. It is the method of finding new truths by means of study, observation, travel, or other means." Now although we think that an investigator must be born and cannot be made, and that no one can frame his methods upon hard

and fast lines of thought or operation, we are quite of opinion that those who make original researches, or are about to make them, may gain much from orderly methods of manipulation, and a knowledge of the right application of logical inference. Bacon attempted to describe such methods in his "Inquisitio de Forma Calidi," and failed, because at that time there did not exist a sufficient basis upon which to found an exhaustive experimental treatment of the subject.

Two interesting chapters in the first part of the book are devoted, the one to the facts and propositions in science, and the other to the criteria of scientific truth. We are reminded herein of an interesting treatment of these subjects in the "Philosophie Méthodique" of M. de Strada, to which we venture to refer our author.

Among the conditions of success in research Mr. Gore very justly enumerates enthusiasm. Of this he quotes several examples. Becher, of Phlogiston fame, after speaking of the chemists as "a strange class of mortals impelled by an almost insane impulse to seek their pleasure among smoke and vapour, soot and flame, poisons and poverty," adds: "Yet among all these evils I seem to myself to live so sweetly, that may I die if I would change places with the Persian King!" The fascination of original research is undoubted, the enthusiasm which it sometimes inspires is unbounded. We remember an instance of a schoolboy who seriously proposed staying at school for several days at the beginning of the Christmas holidays, when "home, sweet home," is doubly sweet, in order to continue a research. And truly, were it not for the enthusiasm which it engenders, the amount of original work done in the world would be much less than it is, seeing that it is usually accompanied by numberless vexations and disappointments, and that it requires unwearied application and perseverance, joined to the possession of an undaunted spirit.

Of Mr. Gore's work as a whole we may say that it exhibits great industry in the collection of facts and a considerable amount of logical acumen in their discussion. Perhaps, however, the arrangement might be simplified. The mass of matter to be digested is so great that any increased modes of classification of the subjects that could be adopted would add to the value of the book. This could best be effected by numbering the paragraphs; by adding marginal references giving the gist of each paragraph, and by making some of the chapters more aphoristic in character. These changes could be easily effected in a second edition.

G. F. RODWELL

LEISURE-TIME STUDIES

Leisure-Time Studies; chiefly Biological; a Series of Essays and Lectures. By Andrew Wilson, Ph.D., F.R.P.S.E., &c. With numerous Illustrations. (London: Chatto and Windus, 1879.)

THIS volume of Essays and Addresses does not profess to contain anything new, either in the way of observation or theory. Neither is the author's style sufficiently brilliant, or his treatment of the subjects sufficiently original to raise them much above the level of the average lectures of a well-informed naturalist. They will, however, afford some useful and interesting information to the general reader, and may serve to attract attention to the question of the introduction of biology into ordinary education. This is the special subject of the first address, which, however, though somewhat lengthy and profuse, does not attempt to grapple with the difficulty of finding competent teachers of biology for *all* our schools. It is indeed suggested, that "the amount of knowledge required to pass even the primary stage of the biological subjects, in the government examinations, held under the auspices of the Science and Art Department," should fit its possessor for imparting elementary instruction in biology. But we greatly doubt whether the examiners would be of this opinion; and we rather think it would be a distressing sight to witness a teacher, whose whole knowledge of the subject was derived from a course of study just sufficient to enable him to pass such an examination, exposed to the questions of a lot of intelligent country boys and girls, whose practical acquaintance with native plants and animals was far more extensive and accurate than his own. If biology is to be taught in schools it must not be by the regular school-teachers qualifying themselves by a few months' training in London, but by the employment of good naturalists to give lectures, demonstrations, and out-door excursions to all the schools of a district in succession.

In the succeeding address, on "Science-culture for the Masses," too much stress is laid on the teaching of science as "a pleasant system of mental gymnastics." This seems to us altogether a wrong ground to go upon. Science is not to be taught in order to strengthen the mind to do something else by and by, but because it opens the mind to a more adequate conception of the universe in which we live, and is in itself, truly, the knowledge which is power.

The lecture on "The Sea-serpents of Science" is interesting, both as giving a very fair summary of the most recent evidence on this subject, and as showing that the age of incredulity is past, and that naturalists are now prepared to admit that several distinct kinds of oceanic monsters probably exist, of which no single specimen has yet been obtained. Recollecting, however, the number of clever hoaxes to which this subject has given rise, we think that the newspaper account at p. 104, of the declaration before a Liverpool J.P., made by the master and crew of a merchant-ship, to the effect that they had seen a huge serpent twice coiled round a sperm whale, and a similar serpent with its head raised "sixty feet perpendicularly in the air," should not have been inserted as evidence without first ascertaining that such a declaration was actually made before the magistrate named. The trouble of writing a single letter would probably have been sufficient, and would have settled the preliminary question of whether the whole story, from beginning to end, was not a pure newspaper *canard.*

The article on "The Genesis of Life" repeats the now often-told tale of the fluctuations of opinion as to spontaneous generation, and will be interesting to those who have not read it elsewhere. Dr. Wilson tries his best to be impartial, and to place before his reader the exact position of the question at the present time. He acknowledges that "isolation" and "destruction" are the two great points of all experiments on the subject, and that if

these are perfect the question can be settled. It is not denied that hermetically sealed flasks give complete isolation, the only question remaining being, to secure complete destruction of whatever organisms, with their germs, may be within the flasks at the commencement of the experiment. He refers to Dr. Bastian's experiments on the death-point of minute organisms and their germs, which was invariably found to be 158° F., and he points out no fallacy in these experiments. Yet if they are conclusive, Dr. Bastian's numerous other experiments, confirmed as they are by Dr. Burdon-Sanderson and others, demonstrate the production of living organisms from dead matter. The elaborate experiments of Prof. Tyndall are referred to as giving results directly opposed to those of Dr. Bastian; but it is not sufficiently pointed out,—firstly, that in Dr. Tyndall's experiments "isolation" was not effected in the only perfect manner by hermetical sealing, and that many contradictory results hence ensued;—and secondly, that all the results opposed to those of Dr. Bastian were negative, and could therefore not disprove the latter's positive results. Dr. Bastian in his test experiments did not use "old hay," the germs in which are said to be "indurated," but infusions of turnip and cress, and after these were subjected in sealed flasks to temperatures of 270° F., and to 230° F. for upwards of an hour, they produced living organisms of such varied types as *bacteria, torula, protamœbæ*, and *monads*. ("Evolution and Origin of Life," p. 175-180.) As *similar organisms* and their germs, *produced in similar infusions* have been proved to be killed by a temperature at least 100° lower than that employed in the above experiment, what we require to settle the question is, not thousands of quite different experiments, whose results one way or the other cannot settle the point at issue, but a repetition of the same experiments by other observers with the object of detecting the fallacy, if any, that lurks in them.

The only other article we can here refer to, is that on "The Law of Likeness and its Working," which deals with the question of heredity, and Mr. Darwin's theory of Pangenesis. But no notice is taken of Mr. Francis Galton's very important "Theory of Heredity," published in he *Journal of the Anthropological Institute*, vol. v. p. 329; which, though it may be considered as a mere modification of that of Mr. Darwin, really differs from it in many important points, and affords a more complete and satisfactory explanation of many of the most curious facts; such as the *unlikeness* of children to their parents, the appearance of diseases and even of mental qualities, in alternate generations, and many others. Every one wishing to comprehend this most difficult yet most interesting subject, should study Mr. Galton's paper as a necessary supplement to the theory of Pangenesis.

At p. 70 of Dr. Wilson's book, a letter from the *Times* is quoted, describing the formation of the bees' cell, as due entirely to the *pressure* of opposing bees in adjacent cells. This is not strictly correct; and Mr. Darwin's observations should have been refered to, showing that the cell-walls are first built very thick, and are *gnawed* down to the requisite thinness. There is also some obscurity in the suggested explanation of the "apparent movement" of the crocodile's upper jaw, when it opens its mouth. The fact appears to be that the crocodile, opening its mouth when on land, *must* raise its upper jaw and head (by bending the neck) simply because the lower jaw has not room to move downwards. The movement of the upper jaw is therefore, under these circumstances, real, and not only "apparent" as stated. One of the most interesting chapters is that on "Animals and their Environments," in which an account is given of the curious changes during the growth of flat fishes, and the still more remarkable phenomena which have been recently observed in the metamorphoses of the axolotl, and the alpine salamander.

A. R. W.

OUR BOOK SHELF

An American Geological Railway Guide, giving the Geological Formation at every Railway Station; with Notes on interesting Places on the Routes, and a Description of each of the Formations. By James Macfarlane, Ph.D. (New York: Appleton and Co., 1879.)

DR. MACFARLANE has added a new pleasure to railway travelling, or rather, by means of this geological guide-book, he has done much to make it both enjoyable and instructive. The idea of the book is excellent, and the plan seems to us thoroughly satisfactory. Now that we have this manageable little flexible book before us, it seems strange that such a guide has not been thought of before, but perhaps not so strange that our enterprising friends on the other side should set an example to their Old-World brethren. We believe the prospectus of some such guide was issued in this country several months ago; if so, we recommend its compilers to obtain a copy of Dr. Macfarlane's book, and take several leaves out of it. If no such work is being prepared for the forlorn traveller of our islands, we advise some of our Survey-men to lay their knowledge together, and produce a similar guide as speedily as possible; they will be doing a public service, and if the result is as satisfactory as in the case of the book before us, they will, we feel confident, reap something more substantial than thanks. In compiling his tables, Dr. Macfarlane has had the assistance of some of the most eminent geologists in the States, and some of the information has not before been published. The tables are very similar to railway time-tables, having on the left hand side of the names of the stations, the miles between each, and on the right, instead of the times, the names of the leading formation to be seen along the route. The tables are so constructed as to be useful for a continuous journey through the States in any direction, and, at the same time, to give an idea of the leading geological characteristics of each state. Appended is an index to railroads, and a general geological map of the States. Prefixed are about fifty pages of instructive information, consisting mainly of methodical descriptions of the various formations of North America, and containing Dana's and Hunt's tables of formations. By carefully studying this the traveller will be in a fair position to profit by the tables, and by the faithful use of these much practical knowledge of geology may be acquired even by the ignorant, while to the geologist they will be a constant source of enjoyment; the handy volume is much more easily managed than a map. We may state that the tables refer to Canada as well as the States.

Comment le font les Miracles en dehors de l'Église. Wilfrid de Fonvielle. (Paris: Dreyfous.)

WHY does not M. Dreyfous date his books? We are sure M. de Fonvielle cannot have noticed the suspicious omission. M. de Fonvielle is already pretty well known in France as a popular gossiper on what may be called the eccentricities of science. The present volume is quite equal in interest to anything he has published, and is likely, we should think, to be widely read in France. It

is a series of stories, told with raciness and touches of sarcasm and humour, of the many impostures which have been perpetrated on the public by those who knew how to turn to account some of the results of science. Of course spiritualism comes in for a large share of notice, while the real miracles of science are pleasantly described in one or two concluding chapters. The book is altogether a very curious one, and evidences considerable research in out-of-the-way corners on the part of its author.

On Foot in Spain; a Walk from the Bay of Biscay to the Mediterranean. By J. S. Campion. Illustrated by Original Sketches. (London: Chapman and Hall, 1879.)

MAJOR CAMPION has already proved his power as a charming *raconteur* in his "On the Frontier," and although in artistic finish and exciting incident the present work is not equal to the former, still it is a well-told story of a free and easy walk through a comparatively little-known country. Major Campion did not encumber himself with more baggage than he could carry himself, and with his gun and genial manners and tact he got on without difficulty wherever he went. We should think his work is likely to increase the number of pedestrian tourists in Spain, about the obstacles to travel in which many delusions exist. Major Campion has much to tell in his own way about the places and people he saw, and every now and then we are glad to stumble on a scrap of information about the geology or natural history of the country. His book ought to have many readers.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Leibnitz's Mathematics

Tempora mutantur, et nos, &c., seems to hold true for Dr. Ingleby, though not for myself, in this matter. His tone has completely changed since December 4, 1871; and he now puts "scientist" (*in quotation marks*) where you had written "scientific man," although in his former letter he said he "hated" the far less obnoxious word "physicist."

That I did not reply to his former letter was mainly because he said that "the question is not susceptible of *proof* until the Council of the Royal Society, who so grossly disgraced themselves in 1712, shall do" a certain "simple act of justice." Recourse to the spiritualists is our only chance in such a matter, and these have now an admirable opportunity for demonstration of their claims.

All I said of Leibnitz was "who, I fear, was simply a thief as regards mathematics." This was completely justified to my mind at the time, partly by my own reading, mainly by some curious documents which Sir David Brewster once showed me. These appeared to me to leave no doubt whatever as to the dishonesty, not only of Leibnitz, but of several of those who have been, at different times, connected with his side of the dispute.

Your allusion to Gregory's series seems to me to bring in no "collateral question" but a very important central one:—it seems to go, in fact, to the root of the matter. For, if Leibnitz was dishonest, this was probably his first (known) offence. Mr. Bottomley's apt quotation looks like a desperate attempt at justification of conduct which the writer felt to be, to say the least, suspicious.

Instead of appealing to the Royal Society Council of 1712, Dr. Ingleby should demand from the proper authorities the publication of that conclusive MSS. of Leibnitz for which Dr. Sloman *asked in vain* in 1858.

P. G. TAIT

The Magnetic Storm of May 14, 1878, Observed in North America

NOTICING in NATURE (vol. xviii. p. 617, and vol. xix. pp. 148, 173, 220) references to the "magnetic storm" of May 14, 1878, I have had prepared by Mr. C. A. Schott, the assistant having direction of our magnetic observatory at Madison, Wisconsin, the inclosed memoranda relative to observations made at that point, as an item of interest to your readers, illustrating the general character and almost simultaneous action of that great magnetic disturbance.

C. P. PATTERSON,
Supt. Coast and Geodetic Survey

Washington, January 6

THE extensive magnetic disturbance of May 14, 1878, of which accounts have been given in NATURE, vol. xviii. pp. 617, 641, 668, and which was observed in China, Australia, and England, was also making its record in North America at our magnetic observatory, established at Madison, Wisconsin, in the winter of 1876–77. This observatory is in latitude 43° 4′ 29″·5, and in longitude 5h. 57m. 36·5s. W. of Greenwich; in it are mounted a set of Brooke's magnetographs, and daily photographic traces of the changes in magnetic declination and in the horizontal and vertical forces have been produced since March, 1877, and are intended to be kept up for some years. The declination traces for several days preceding the 14th were normal, but about midnight, May 13–14, a series of disturbances commenced consisting in part of some large oscillations to the eastward and westward, and in part of a great number of small and rapid oscillations. The characteristic features of the trace may be given as follows:—

	Madison mean time. h. m.		Greenwich mean time. h. m.
The disturbances in *declination* commenced about	14th, 0 00 A.M.	...	14th, 5 58 A.M.
A principal westerly[1] extreme reached...	,, 1 05 ,,	...	,, 7 03 ,,
,, easterly ,, ,, ...	,, 2 36 ,,	...	,, 8 34 ,,
Range of motion 16′·5; after this a series of smaller oscillations continue to past noon; a maximum westerly position is reached about	,, 0 16 P.M.	...	,, 6 14 P.M.
And an easterly extreme at	,, 0 53 ,,	...	,, 6 51 ,,
Extreme westerly deflection at	,, 3 26 ,,	...	,, 9 24 ,,
A sharp motion to eastward commences at	,, 5 40 ,,	...	,, 11 38 ,,
A principal easterly extreme reached at	,, 6 24 ,,	...	15th, 0 22 A.M.
Range of motion of principal disturbance 31′·7.			
There is also a westerly extreme about...	,, 7 10 ,,	...	,, 1 08 ,,
And an easterly extreme about	,, 9 16 ,,	...	,, 3 14 ,,
After 10 P.M. the irregularities gradually subside.			
Last extreme easterly position	15th, 1 10 A.M.	...	,, 7 08 ,,

It will be noticed that at the Greenwich Observatory the storm commenced on May 14 at 6h. 5m. A.M., at Stonyhurst Observatory at 6h. 4m., at Ti-ka-wei, near Shanghai, also at 6h. 4m. (G. T.), and at Melbourne, supposed at 6h. 20m.; the storm may therefore be taken as simultaneous at these places. At Greenwich the north end of the needle moved eastward between 6 and 9 A.M., but at Madison the general motion was westerly. Again, the sharp deflection commencing at 5h. 40m. Mad. T. (11h. 38m. P.M. G. T.) was to the *eastward* at Madison and to the westward after 11h. 45m. at Greenwich, thus deflecting the magnets in opposite directions.

	Madison mean time. h. m.		Greenwich mean time. h. m.
The northern component of the *horizontal* force was sharply affected at Madison, the force diminishing at ...	14th, 0 05 A.M.	...	14th, 6 03 A.M.
The disturbance continued, but between 2.30 and 7.30 A.M.			
The trace is too indistinct to be read; the small oscillations continue to about 11h. 45m., when they become superseded by a series of larger waves culminating in a maximum extreme at ...	,, 2 30 P.M.	...	,, 8 28 P.M.
And in a minimum at	,, 3 58 ,,	...	,, 9 56 ,,
The large disturbances continue till about 10h. 20m. P.M., having reached a maximum extreme about	,, 4 50 ,,	...	,, 10 48 ,,
And a minimum extreme at	,, 9 03 ,,	...	15th, 3 01 A.M.

Range between max. at 2h. 30m. and min. at 9h. 3m $= \frac{1}{154}$ of the horizontal force, nearly.

In the Greenwich account it is stated "The first start in the trace of the declination magnet at 18h. 5m. (A.R.) is most distinct;" now, within two minutes of this time occurs the first and sharpest deflection in our horizontal force trace at Madison, thus

[1] Referring to the end of the magnet.

marking distinctly the commencement and simultaneousness of the storm.

The disturbance in the *vertical* force commenced about May 14, 1h. 10m. A.M. (7h. 8m. G. T.), and terminated about 3h. 30m. A.M. Between this time and 5h. 45m. P.M. the trace was smooth, but between 5h. 45m. and 6h. 45m. a sharp deflection took place in the opposite direction, the maximum force occurring at 6h. 11m. P.M. (May 15, 0h. 9m. A.M. G. T.); the deflection or increase amounted to $\frac{1}{510}$ of the vertical force.

The vertical force trace did not exhibit any of the tremulous motion noticed in the two other curves.

I may state that a description of the Madison Magnetic Observatory, together with the discussion of the first year's observations and results, is nearly ready in MS. for publication.

Coast Survey Office, Washington, D.C., January 6 — CHARLES A. SCHOTT, Assistant, C. and G. S., in charge of Observatory

Migration of Birds—A Suggestion

I BELIEVE that the migration of birds is one of the most interesting problems to the many who dabble in the narrative branch of natural history. It must also be of some interest to those who study biology in its more strictly scientific form. And yet there appears to be but little accurate information on the subject; nor, as far as I can discover, have any attempts been made to collect careful simultaneous observations over a wide extent of country, which would appear to be the readiest, if not the only means of collecting reliable data.

I am therefore induced to suggest that you should devote a small space in NATURE weekly, at certain seasons of the year, to the record of observations made in all parts of the world, from which correspondents can be induced to send them, of the passage of any migratory birds.

The record should, I think, be in a tabular form, giving the place, the date of appearance or disappearance of any migratory bird, the name of the observer, and some few remarks that may appear pertinent, such as the state of the weather, the direction of the wind, the character of the night, whether dark or moonlight, in the case of birds which are supposed to travel at that time. The necessity for recording other facts will no doubt be suggested by those more conversant with the subject. From these suggestions and the experience gained in a short time, detailed instructions might with advantage be framed for observers. The observers should be careful to state whether they were in a position to know with certainty that the date given was that on which the birds really arrived or left, or whether it was the first time their attention had been called to the circumstance.

I should imagine you would soon have a large number of careful observers interested in the subject not only in the country but abroad also. Among these some means should be taken to enlist captains of ships, or other seafaring men. There appear to be few, if any, recorded observations of birds of passage being seen on the wing at sea—though I may be wrong in this, for I do not profess to have gone carefully into the subject. Still, it is curious that one does not hear oftener of flights of such birds as the woodcock, millions of which come to and leave this country every year, being seen at sea, over long stretches of which they are believed to fly.

J. F. D. DONNELLY

The Formation of Mountains

THE letter of the Rev. O. Fisher in NATURE, vol. xix. p. 266, is conclusive as to the more rapid cooling of the interior than the outer crust of a heated globe under the conditions of our earth, and I thank him for clearing up the point. But the question remains, whether the amount of contraction of the interior, and consequent crumpling of the crust, thereby produced in a definite time, is sufficient to account for the elevation of our mountains. It is necessary to take account of the following facts:—

1. That the greater part of the elevation of all our chief mountain ranges occurred during the eocene and miocene periods.

2. The warmer climates of those periods (certainly due to external and not to internal heat) would have tended to diminish the rate of cooling and consequent contraction of the earth.

3. The Rev. O. Fisher appears to have demonstrated that, even allowing for the total shrinkage due to the earth's cooling for the last hundred million years (from a mean temperature of 7,000° F., as calculated by Sir William Thomson), the amount of elevation thereby caused would be *very much* less than that of existing lands and mountains. But we know that these have been lowered by denudation, and again elevated many times over during that period.

The inadequacy of the alleged cause for the production of our existing mountains would therefore seem to be conclusively established.

ALFRED R. WALLACE

MR. WALLACE's letters raise three separate difficulties: How can the interior of the earth be cooling faster than its surface? How can this surface be cooling so slowly (or not cooling at all) as it is assumed to be? and, How can the different rates of cooling of interior and surface account for the corrugations observed in the surface?

As to the first question, Mr. Fisher's explanations should remove the difficulty.

As to the second question. So long as the amount of heat radiated from the surface into space equals the amount received by the surface from the sun and from the interior together, its temperature cannot fall. The rate of radiation depends on the difference between the temperatures of the surface and space. The supply of heat from the interior depends on the difference between the temperatures of the interior and the surface. Since the temperature of the interior is falling, the supply of heat from interior to surface must diminish. Hence the temperature of the surface must also fall. We cannot doubt that it does, though at present imperceptibly. The extreme slowness is due partly to the fact that the difference of temperature between interior and surface must many times exceed that between the surface and space, even if the latter be at absolute zero; partly to the low transmissive powers both of earth and atmosphere; partly to the immense mass of the earth, from which immense quantities of heat must pass away to lower the temperature of the interior but a trifle.

As to the third question, whether this small fall in temperature will account for observed corrugations, I will offer no opinion. The size of the earth must be allowed for. A diminution of a foot in radius would diminish the surface by more than nineteen square miles.

I do not understand why corrugations should be confined to the surface. I should expect them to extend as deep as the solid crust. They are possible in any stratum which is contracting more slowly than interior strata.

E. HILL

St. John's College, Cambridge

Bees' Stings

CAN any of your readers inform me why the working honey-bee has such an imperfect weapon of defence as its sting manifestly is? For purposes of self-defence it is apparently worse than useless, for in nearly every case, almost without exception, the bee lays down its life with the sting. The possession of a sting therefore only leads to its own destruction instead of to its preservation so far as the individual bee is concerned. No doubt the hive generally gains an advantage from all its active members having stings and so indirectly do individual bees from the fact that the welfare of the hive, speaking generally, means the welfare of the individuals that compose it. Directly, however, the possession of a sting can only be a disadvantage to the individual bee, unless there are certain enemies from which bees after inflicting a wound can withdraw their stings and escape with life. This so far as my observations go appears to be very unlikely, and therefore no bee can have any knowledge from experience of what a weapon of offence he possesses for he has never used it, nor can he have knowledge from experience of the consequence of using it. All smaller pests bees attack with their jaws. Is it possible, then, that they are so intelligent as to be well aware of the power for mischief to themselves as well as to others which they carry about with them, and that it is only when they altogether lose control over themselves, either through severe pain or through terror lest their queen should be injured that they sign their own death-warrants on our hands and faces? In the death of a few worker-bees a hive suffers very little loss, perhaps none at all; yet it may have gained much in the shape of security from molestation. Are bees so intelligent as to know this fact and communicate it from one to another, or can their conduct be explained on the lower ground of instinct?

It seems that an interesting point is here raised which perhaps

has been fully discussed elsewhere without my knowledge of it. Is the fact that the sting of the worker-bee is an imperfect weapon of defence, a result of its having nothing to do with the propagation of its species, this being left to the stingless queen and drones? Consequently any tendency to develop a more effective sting in one generation of worker-bees has no hereditary effect on succeeding generations, nor apparently have the worker-bees any influence whatever on the worker-bees that succeed them, except by the way in which they feed and educate them, unless, indeed, they can impress their tendencies on the drones or on the future queen before she leaves the hive. If they have no such power, it seems likely that they will always have to lament the use of a weapon which nature might have made as effective as the sting of a wasp. Finally, are there any other insects in the same predicament as worker-bees, *i.e.*, unable to use their weapons of defence without doing themselves more injury than they inflict on their adversary, and unable to help their successors by the transmission of a continually accumulating instinct? R. A.

Manningtree, January 22

Molecular Vibrations

MR. CHAPPELL is certainly right in stating that "the noises in a belfry are most discordant." He might have said (what no doubt he meant) that the sounds emitted by each single bell are most discordant. Every bell which is at all tolerable, possesses, it is true, one predominating note due to the thick part of the bow, where the clapper strikes, but there are also innumerable other notes, some of which *may* be harmonics, while the majority are not so at all. This is presumably often owing to flaws and other defects in casting, but there is another cause common to every case, which is due to the following fact:—

All bells are cast of a conventional shape, with varying diameters from bow to crown. Now every part of a bell, taken vertically, comes into vibration when struck, and in order to give a true note, each horizontal section ought to have a certain exact thickness of metal proportional to its diameter. This is easily verified to the ear by tapping the bell gently at all parts from the bow upwards. Every inch gives a different rate of vibration, and, consequently, a different pitch.

About the time when the second "Big Ben" was cast, which is a long time ago, I tried experimentally to ascertain what the law was which regulated the thickness of the metal in relation to the diameter of the bell, so that every section might be of identical pitch. This was done by casting a series of bell metal rings of varying diameters, and tuning them, by turning in a lathe, to exact unison. So far as my recollection now serves me, the following was the result:—

Measuring all the rings by their outside diameters, no undeviating rule was apparent, and the same was the case when the inside diameters were compared. When, however, a circle was taken whose circumference was, as nearly as possible, one-third from the outside of the thickness and two-thirds from the inside, then the law came out distinctly that the thickness of the metal must be proportionate to the square of the diameter of such circle. It occurred at once that this circle must, in fact, constitute the neutral axis of vibration. Working on this principle, it seemed worth while to try whether a bell could not be constructed free from discordant sounds. I may shortly say that this proved to be possible, but only by turning the actual casting with great care and accuracy in a lathe. It became evident that the slightest variation in the true thickness vitiated the unisonal character of the tone. A "miss was as good (or as bad) as a mile," and consequently the process of casting itself was too rough for obtaining the desired end.

It may fairly be gathered from Mr. Chappell's letter that he is not enamoured of a "triple bob major," and that he does not class bells generally as musical instruments. I am much afraid he never will. If the present shape and mode of construction (and let me add, the present mode of change ringing) is adhered to, a peal of bells which will quite satisfy a musical ear may be regarded as a practical impossibility. R. H.

Missing Nebulæ

IN the note on missing nebulæ in NATURE, vol. xix. p. 221, I find the nebulæ G. C. 132, 4570, and 5051 mentioned together with the Merope nebula as being diffused objects which are "overlooked in very large telescopes, though obvious in much smaller ones." This alludes, no doubt, to the occurrence of these objects in the list of nebulæ not found with Lord Rosse's 6-foot reflector (*Phil. Trans.*, 1861, p. 745).

With regard to the first object, G. C. 132, it has only been looked for once at Birr Castle, and in the N.P.D. 111° 30′ it is possible to account for its non-appearance either by a tilting of the speculum or by the haziness of the sky in this low altitude. G. C. 4570 has been seen three times, and only twice searched for in vain, both times in twilight. G. C. 5051 was set for twice and not found, but 15° north of the zenith the tilting of the speculum almost always changes the index-error of the setting-circle considerably, as expressly stated by the observer on one of the two occasions alluded to. The Merope nebula was last winter seen very distinctly, and roughly sketched with a low power and large field. J. L. E. DREYER

The Observatory, Dunsink, Co. Dublin, January 13

Time and Longitude

Now that mankind begin to have settlements, even continental, as appears from Mr. Latimer Clarke's account of Sitka, subject to the inconvenience that he and Mr. Layard point out, is it not time that we agreed to make the line dividing "yesterday from to-morrow" avoid all continents, by taking advantage of two very convenient, if not providential, facts, which are certain, though each was *à priori* highly improbable? First, there were great chances against a globe with our existing proportion of land to water, of coast-lines to area, and of large and small lands to each other, having any Behring Strait, admitting one degree of longitude, or thereabouts, to enjoy the above property. But next, there was still greater chance, perhaps, against the exact opposite degree to the strait covering several national observatories; not only more of them, I think, than any equally narrow meridional band, but the only one that, on historical grounds, we can conceive distant civilised nations accepting without jealousy as a common centre. The antimeridians of Copenhagen, Uraniburg, Leipzic, Munich, Padua, Venice, and Florence, seem to avoid both continents; possibly also those of Christiania, Gotha, Verona, and Modena. Those of Berlin, Prague, Naples, and Palermo, seem a very few miles too far east. Europe proper, and its present railways, are very closely bisected by this street of observatories; the local time of the furthest points each way varying but an hour and a half from it. But the chief coincidence is yet unnamed. Would the pride of any existing land, except China, refuse to make a standard meridian of Rome?

The very Chinese must allow Europe a sort of scientific precedence, not as the metropolitan, but the learned continent—earth's university. Europe alone is the adult continent, if there be one; and no other has in a strict sense a metropolis. The history of no other has so turned upon one pivot city as that of Europe has on Rome, nor is likely ever to do so. Some one says that "what a church is to a city, Palestine is (or may some day be) to the world;" but it is less disputable that what the market-place is to a city, Europe is to the world—perhaps permanently. And what the tribunal is to the market-place, Rome has been to Europe, as long as Europe was growing. Observe, too, that in this special connection both our civilised time-reckonings, "Old Style" and New, have come from Rome. Might we not also supersede the distinction of E. and W. longitude, by calling Rome 180°, and reckoning all round, from Behring to Behring, leaving the 0° as yet unmarked? E. L. G.

[E. L. G.'s proposal has been already made by M. de Beaumont. See NATURE, vol. xix. p. 247.—ED.]

Shakespeare's Colour Names

I FEAR you will think that the correspondence on this subjec is becoming a mere criticism on Shakespeare's text, and therefore out of place in your columns, but I trust you will afford me space for a short rejoinder to Mr. Ingleby's letter (NATURE, vol. xix. p. 244).

I am obliged to him for pointing out that Sir T. Hanmer had already suggested the substitution of "keen" for "green" in the passage from "Romeo and Juliet," Act iii. Sc. 5. This had escaped me, but I cannot agree with him that the alteration has been *rightly* rejected by subsequent commentators. I have not at present any opportunity of examining the eyes of any living eagles, but in opposition to Mr. Craig-Christie's evidence (NATURE, vol. xix. p. 221) I must point out that all our best

British ornithologists—Yarrell, Macgillivray, Gould, Meyer, and Morris—describe the eye of the Golden Eagle (the less rare of our two British species, and the one usually referred to by our poets) as *hazel* or *brown*. The eye of the Sea Eagle is described by the same authorities as yellow. I cannot think that so accurate an observer of nature as Shakespeare would call either hazel or yellow eyes *green*. Can Mr. Ingleby cite any authority for such a comparison as "green as is an eagle's eye"? while the keen piercing sight of the bird is as proverbial as the swiftness of its flight. I am well aware that green eyes were held in high estimation by the old poets, especially by those of Spain; Shakespeare, however, does not seem to me to have shared in this predilection, as, setting aside the doubtful play of "The Two Noble Kinsmen," and the passage now in question, he uses the epithet three times only, I think, as applied to the eye, and then always *in malam partem*, viz., "green-eyed jealousy," "Merchant of Venice," Act iii. Sc. 2; "It is the green-eyed monster," "Othello," Act iii. Sc. 3; and in "Midsummer Night's Dream," Act v. Sc. 2, where the "eyes as green as leeks" are met with in conjunction with "lily lips," "cherry nose," and "yellow cowslip cheeks." I cannot think with Mr. Murphy (NATURE, vol. xix. p. 197), that the eyes which the old poets so admired as green were what we call blue; they were more probably *grey*, which often has a shade of green in it—the "eyen grey as glas" of Chaucer's "Prioresse." These green or grey eyes were, I think, usually an attribute of feminine rather than masculine beauty, as in the passage from "The Two Noble Kinsmen," Act v. Sc. 1, where they are mentioned in an address to Diana (not Neptune, as Mr. Ingleby has it). Shakespeare well distinguished between the different colours of eyes—see "Two Gentlemen of Verona," Act iv. Sc. 4, and "Twelfth Night," Act i. Sc. 5, for grey eyes; "As You Like it," Act iii. Sc. 2 for blue eyes; "Romeo and Juliet," Act ii. Sc. 4 for black and grey eyes, and Act iii. Sc. i. of the same play, where hazel eyes are mentioned.

ROBERT BREWIN

Exeter, January 20

Intellect in Brutes

SIR HARRY LUMSDEN allows me to publish the following little incident:—Late last autumn some partridges, which he had tamed and kept about the house, disappeared as usual and became wild. When the excessive cold set in and Aberdeenshire was deep in snow, Sir H. Lumsden was greatly pleased and surprised one morning to find his old friends on the doorstep waiting to be fed. Next morning they appeared with a wild covey of eleven birds, and the tame cock sat on the doorstep and crowed to the wild birds, evidently encouraging them to come and eat the food, which, however, they declined to do till it was put further from the house. Soon after the tame birds appeared with two covies. How did they entice the wild birds except by actual bird talk?

WALTER SEVERN

Feeding a Python

THE attack of a constrictor, at all events in confinement, is very often unsuccessful; but perhaps this may be because the reptile is not hungry. I have often seen the constrictors in the London Zoological Gardens strike several times at birds, pulling out feathers and even getting a firm hold and then releasing their prey, to renew the attack presently either with or without success. When the membrane over the eye is becoming opaque in consequence of the change of skin they frequently fail to hit the prey at all, but still persist until they secure it. I saw one of the large pythons take a rabbit in a way which must be unusual, I think. The rabbit was hopping about near the snake's coils when the reptile suddenly made a loop in its body, and firmly inclosed the victim without touching it at all with the mouth, or even raising its head. The rabbit died there, but the snake paid no attention to it for a quarter of an hour and subsequently swallowed it very leisurely.

ARTHUR NICOLS

THE GRAHAM LECTURE, ON MOLECULAR MOBILITY

THIS lecture, the institution of which was referred to in NATURE, vol. xix. p. 254, was delivered on the 22nd inst., by Mr. W. Chandler Roberts, F.R.S., Chemist of the Mint, before the Philosophical Society of Glasgow, in the hall of the University, where Graham graduated in 1824.

The audience, which was very large, included most of the professors of the University.

Mr. James Mactear, president of the Chemical Section, pointed out that they were doubly fortunate in having secured the services of Mr. Roberts, whose co-operation in his work Graham repeatedly acknowledged in the warmest terms, and in the fact that Mr. James Young, F.R.S., of Kelly, the life-long friend of Graham, had consented to preside on this occasion; he therefore vacated the chair in favour of Mr. Young, who introduced the lecturer.

Mr. Roberts briefly traced the influence of Black and Thomson in turning the attention of Graham to the study of molecular physics, to which he patiently devoted his life. In connection with the law of the diffusion of gases the lecturer claimed that Priestley made in 1799 an observation on the escape of hydrogen from a cracked jar. The subsequent and independent discovery of this phenomenon by Doebereiner in 1823 has hitherto been considered the starting-point of the experimental study of gaseous diffusion to which it undoubtedly attracted Graham's attention. After a brief review of the influence of Eastern and Greek thought on the study of molecular movement, allusion was made to Sir Christopher Wren's model representing the effects of all sorts of impulses that result from the impact of hard globulous bodies, which, according to Dr. Sprat, historian of the Royal Society, he proposed as the principles of all demonstrations in natural philosophy, it being considered "that generation, corruption, and all the vicissitudes of nature are nothing else but the effects arising from the meeting of little bodies, of different figures, magnitudes, and velocities."

Herepath's revival of Bernoulli's view as to the movement of gaseous particles was considered, and Mr. Roberts then described in detail the experiments that enabled Graham to establish the law of the diffusion of gases, and he illustrated experimentally the passages of gases through porous bodies, such as unglazed earthenware and artificial graphite, as well as through a layer of the hard translucent variety of opal known as hydrophane. The mode in which Graham studied the diffusion of the momentum of gases, by observations on viscosity as indicated by rates of flow through capillary tubes, was then described. It was pointed out that his law of diffusion forms the basis of the science of molecular mechanics, and his measurements of the rates of diffusion prove to be the measure of molecular velocities which have been so profoundly investigated mathematically by Clerk-Maxwell, Clausius, and Boltzmann, and experimentally by Loschmidt in developing the dynamical theory of gases. The lecturer then considered the passage of gases through colloid or jelly-like bodies which have no sensible pores, dwelling more especially on the separation of oxygen from air by the transmission of air through a thin film of india-rubber, a circumstance of special interest from a physiological point of view.

The liquefaction of gases formed the subject of one of Graham's earliest papers, in 1826, and it occupied his attention at intervals during his life. He held the view that hydrogen when absorbed by palladium is reduced to the metallic form, a supposition which has received strong confirmation from the success that has attended M. Raoul Pictet's efforts to solidify this gas; and that distinguished physicist stated in a letter to Mr. Roberts that it is probable Graham's indication of the density of solid hydrogen will prove to be nearly correct. Allusion was then made to Graham's opinion that the various kinds of matter now recognised as different elementary substances may possess one and the same ultimate or atomic molecule existing in different conditions of movement, the varying degrees of rapidity of this movement constituting, in fact, the difference between the elementary

bodies. In other words, if the molecular energy of a so-called element could be changed, the element would be dissociated, a view of special interest in relation to the researches of Lockyer. The lecture was illustrated by many effective experiments, and concluded with the statement that it had not been instituted from the merely special interest of Graham's researches to the physicist and chemist, but in honour of the labours of a life the memory of which will be as enduring as its work, and to stimulate others to investigate as patiently and earnestly the varied phenomena whose basis is "molecular mobility."

Sir William Thomson, in proposing a vote of thanks to the lecturer, called attention to a diagram on the wall recording the rates of passage of gases by diffusion, effusion, transpiration, and by the peculiar passage through such "colloid septa" as non-crystalline metals or india-rubber; and he stated that before Graham's time these valuable physical constants were absolutely unknown. They had listened with much interest to the connection which had been traced between Graham's law of diffusion and the science of molecular physics, as well as to the account of Graham's work generally, so carefully set before them by Graham's pupil and friend.

PRELIMINARY NOTE ON THE SUBSTANCES WHICH PRODUCE THE CHROMOSPHERIC LINES[1]

HITHERTO, when observations have been made of the lines visible in the sun's chromosphere, by means of the method introduced by Janssen and myself in 1868, the idea has been that we witness in solar storms the ejection of vapours of metallic elements with which we are familiar from the photosphere.

A preliminary discussion of the vast store of observations recorded by the Italian astronomers (chief among them Prof. Tacchini), Prof. Young, and myself, has shown me that this view is in all probability unsound. The lines observed are in almost all cases what I have elsewhere termed and described as *basic lines*; of these I only need for the present refer to the following:—

b_3 ascribed by Ångström and Kirchhoff to iron and nickel.
b_4 ,, Ångström to magnesium and iron.
5268 by Ångström to cobalt and iron.
5269 ,, ,, calcium and iron.
5235 ,, ,, cobalt and iron.
5017 ,, ,, nickel.
4215 ,, ,, calcium, but to strontium by myself.
5416 an unnamed line.

Hence, following out the reasoning employed in my previous paper, the bright lines in the solar chromosphere are chiefly lines due to the not yet isolated bases of the so-called elements, and the solar phenomena in their totality are in all probability due to dissociation at the photospheric level, and association at higher levels. In this way the vertical currents in the solar atmosphere, both ascending and descending, intense absorption in sun-spots, their association with the faculæ, and the apparently continuous spectrum of the corona and its structure, find an easy solution.

We are yet as far as ever from a demonstration of the cause of the variation in the temperature of the sun; but the excess of so-called calcium with minimum sun-spots, and excess of so-called hydrogen with maximum sun-spots follow naturally from the hypothesis, and afford indications that the temperature of the hottest region in the sun closely approximates to that of the reversing layer in stars of the type of Sirius and α Lyræ.

If it be conceded that the existence of these lines in the chromosphere indicates the existence of basic molecules in the sun, it follows that as these lines are also seen generally in the spectra of two different metals in the electric arc, we must be dealing with the bases in the arc also.

[1] Paper read at the Royal Society on January 23, by J. Norman Lockyer, F.R.S.

ON A THEORY OF THE VISCOSITY OF THE EARTH'S MASS[1]

IN these two papers the investigation is continued of the physical results which follow from the theory that the mass of the earth is either viscous or imperfectly elastic. In the first paper of the series (which was read before the Royal Society on May 23, 1878, and of which an account appeared in NATURE, vol. xviii. p. 265) the theory of the bodily tides of such a spheroid was considered. In that paper it was shown that the bodily tides would lag, and that this lagging would produce an acceleration of the time of high water of the oceanic tides relatively to the nucleus. The author's attention was directed to the tidal reports of the British Association by Sir W. Thomson, and he has tried to find whether the tidal observations give any indications of a yielding of the earth's mass. The theory of the semi-diurnal and diurnal oceanic tides is so imperfect that it is impossible to say whether or not high water takes place earlier than it would do on a rigid nucleus; the long-period tides are those from which alone any indications are to be expected.

The fortnightly tide is the most marked of these, but its height is very small, and the results in the tidal observations show so much irregularity that it cannot be asserted with certainty that they represent the true fortnightly tide. Nevertheless, it is interesting to learn that, out of eleven years of observation at Ramsgate, Liverpool, Hartlepool, Brest, and Kurrachee, the fortnightly tide appears to be accelerated in eight cases and only retarded in three. Although the accelerations are exceedingly irregular, it may perhaps be maintained that these observations give some indications of a tidal yielding of the earth's mass.

The first of the two papers of which we are here speaking deals with the effects of the tidal distortion of the spheroid on its rotation, and with the reaction on the tide-raising satellite. An account of some of the results of the investigation was read before the British Association at Dublin, and an abstract appeared in NATURE, vol. xviii. p. 580, and therefore the principal results will be here merely repeated.

For convenience of diction the spheroid is spoken of as the earth and the tide-raising body as the moon.

It was found, then, that the obliquity of the ecliptic, the length of day and of the month, become variable, and that, if we look into the remote past, we find the obliquity less, and the day and month very much shorter than at present. When the changes were traced backwards as far as possible it was found that the whole diminution in the obliquity was about 10°, and that the beginning from which the earth and moon must have started was a state in which they rotated, as though fixed rigidly together, in 5h. 40m., the moon being then only 10,000 miles distant from the earth's centre.

In the second paper (read before the Royal Society on December 19) some other problems were considered. The first of these is concerning the secular distortion of the spheroid. Under the attraction of the moon the earth becomes distorted into an ellipsoidal shape, with the longest axis in the plane of the equator, but, since the tide lags, this longest axis does not point directly towards the moon. The excess of the attraction of the moon on the nearer protuberance above that on the further one gives rise to the tidal frictional couple. This couple tends to retard the earth's rotation; but it is clear that unless the tidal protuberance has some special form

[1] An account of two papers, "On the Precession of a Viscous Spheroid, and on the Remote History of the Earth," and "Problems Connected with the Tides of a Viscous Spheroid," by G. H. Darwin, read before the Royal Society on December 19, 1878.

the whole earth cannot be retarded exactly as though it were a rigid body. Now the tidal protuberance has not this required form, and therefore there results a slow secular distortion of the earth arising from the unequal distribution over the surface of the forces which constitute the tidal frictional couple.

The greater part of the pull which retards the rotation is applied in the equatorial regions, and therefore the rotation of those regions will be more rapidly retarded than that of the polar regions. As the earth's rotation is from west to east, it follows that the polar regions will outstrip the equator and will move very slowly from west to east relatively to the equatorial parts.

The exact mathematical solution for this kind of a distortion of a viscous spheroid shows that it consists in a simple cylindrical motion round the axis of rotation, each point moving from east to west with a linear velocity proportional to the cube of its distance from that axis.

The distortion of the surface of the globe consists of a motion in longitude from west to east, relatively to a point in the equator, the rate of change of longitude being proportional to the square of the sine of the latitude.

Numerical calculation shows, however, that in the later stages of the earth's history (the development being supposed to follow the laws found in the paper on "Precession") the distortion must have been very small. With a certain assumed viscosity it is found that, looking back 45,000,000 years, a point in latitude 60° would lie 14′ further east than at present. From this it follows that this cause can have had little or nothing to do with the crumpling of geological strata.

As, however, the distorting force varies inversely as the sixth power of the moon's distance, it seems possible that in the very earliest stages this cause may have had sensible effects. It is therefore noteworthy that the wrinkles raised on the surface would run north and south in the equatorial regions, with a tendency towards north-east and south-west in the northern hemisphere, and north-west and south-east in the southern one. The intensity of the distorting force at the surface varies as the square of the cosine of the latitude.

An inspection of a map of the earth shows that the continents (or large wrinkles) conform more or less to this law. But Prof. Schiaparelli's map of Mars[1] is more striking than that of the earth, when viewed by the light of this theory; but there are some objections to its application to the case of Mars. If, however, there is any truth in this, then it must be postulated, that after the wrinkles were formed the crust attained sufficient local rigidity to resist the obliteration of the wrinkles, whilst the mean figure of the earth adjusted itself to the ellipticity appropriate to the slackening diurnal rotation: also, it must be supposed that the general direction of the existing continents has lasted through geological history.

The second problem considered in this paper is concerning the distribution of the heat, which would be generated by the internal friction of the tidal distortion.

It was shown in the preceding paper that a very large amount of heat might be thus generated, and it appeared at first sight as though this might serve to explain in part the observed increase of underground temperature; but the solution of a certain problem concerning the cooling of an infinite slab of rock 8,000 miles thick, in which heat is being generated according to a certain law of distribution, shows that the frictional heat could not possibly explain a rate of increase of underground temperature near the earth's surface of more than 1° Fahr. in 2,600 feet.

It follows, therefore, that Sir W. Thomson's investigation of the secular cooling of the earth cannot be sensibly affected by this cause.

[1] *Memorie della Società degli Spettroscopisti Italiani*, 1878, vol. vii.

The last part of the paper does not lead to results of interest to the general reader, as it is concerned with the part played by inertia in the tides of viscous, fluid, and elastic spheres.

INDIAN METEOROLOGY[1]

IN the article "Atmosphere" of the *Encyclopædia Britannica* it has been justly remarked that one of the most important steps that could be taken towards the development of the science of meteorology would be extensive series of observations from such countries as India, which offers splendid contrasts of climate at all seasons, has a surface covered at one place with the richest vegetation, and at others with vast stretches of sandy deserts, and presents extensive plateaus and sharp ascending peaks, all which conditions are indispensable for collecting the data required for the solution of the problem of atmospheric physics. In working out this problem it is necessary, owing to its extreme complexity and difficulty, to give attention, not merely to questions immediately bearing on the physics of the atmosphere, but also to climatic and other practical inquiries, which may be handled with comparative ease and which afford results that contribute indirectly but very materially to the solution of the higher problem. The publications enumerated below admirably follow up this two-fold line of inquiry, and even already several important practical and theoretical conclusions seem not far from the point of being reached by the meteorologists of India.

The "Report on the Meteorology of India" is the second Annual Report issued since the administration of the Indian Meteorological Establishment was concentrated in the Central Office at Calcutta for the whole of India including British Burmah and the Islands of the Bay. In the scheme of publication of the monthly results of the observations made at the various stations over India, we note with satisfaction that the form proposed by the Permanent Committee of the Meteorological Congress at Vienna has not been adopted in some of its more important details. Thus in Mr. Blanford's tables, instead of a general monthly mean of atmospheric pressure, the mean monthly pressure for each hour of observation is given—an essential requisite for the presentation of the data required in discussing various of the more important problems of international meteorology. Indeed these tables possess the very high merit of being, with perhaps one exception, entirely suited for the discussion of climatic questions of an international character—the single exception being the lumping together of the two or four daily observations of the winds into one monthly mean, instead of a monthly mean for each hour of observation as is so admirably carried out by Professor Rubenson in his annual reports of Swedish meteorology.

The most interesting part of this report is that which deals with the failure of the rains in Western and Southern India which resulted, as is only too well known, in one of the most terrible and wide-spread famines of recent years. The mode of treatment is grounded on the practice adopted by the Office, in framing forecasts of coming seasons to which we have several times drawn the attention of the readers of NATURE (vol. xiii. p. 66, &c.), and which may be described as proceeding on the assumption that there is a certain persistency in meteorological conditions; that, for instance, the longer a given state of weather has lasted, the less the probability of a speedy change; and that as regards the distributions of pressure, on which weather is so dependent, certain states of the atmosphere tend to perpetuate or reproduce themselves in the same region in such a manner as to maintain a

[1] "Report on the Meteorology of India in 1876." By Henry F. Blanford. "The Indian Meteorologist's *Vade-Mecum*." By Henry F. Blanford. "Indian Meteorological Memoirs;" issued under the direction of Henry F. Blanford. Vol. i., part 2. "The Meteorology of the Bombay Presidency." By Charles Chambers, F.R.S.

constant difference between the average pressure of two neighbouring regions which, though protracted, is not permanent, but disappears after a longer or shorter time. Mr. Blanford largely inclines to trace the failure of the rains to an unusually great expanse of snow covering the southern slopes of the Himalayas, much of which fell very late in the season, and which acted as a cooling agent, bringing about an abnormal distribution of pressure, and consequently of winds, temperature, and rainfall, conditions which, once fairly established, went on reproducing themselves so that cyclonic and anti-cyclonic areas of an abnormal character gained a certain persistency over those parts of India where the rainfall was deficient and where it was in excess. Should future observations confirm this hypothesis, the result will be one of the most important yet arrived at in practical meteorology.

The least satisfactory part of the report, perhaps, is that referring to the relation of rainfall to the sun-spot period, in which too much stress appears to be laid on the results of data collected from a wide geographical superficies, and too little stress upon data referring to limited regions; the data of which regions, it may be added, require for their satisfactory discussion to be examined with reference to their seasonal as well as annual variations during the sun-spot periods.

The practical part of the "Indian Meteorologist's *Vade-Mecum*" being part 1 of the work, is in many respects a model-handbook for the observers for whose use it is intended. The clearness with which the difficulties attending the making of real observations of temperature are apprehended is altogether admirable; and the provisions and precautions as regards instruments, hours, and modes of observing actually taken are of such a nature as likely to secure observations of a high quality, owing to an increased intelligence, and efficiency on the part of the observers who work in accordance with the principles and instructions laid down for their guidance.

Mr. Chambers' book is an elaborate and important work on the Meteorology of the Bombay Presidency, based on all the observations made in the Presidency down to 1874. Its splendid porte-folio of eighty highly finished maps and diagrams printed in colours, as well as its excellent typography with 159 tables of results, many of them being wholly or in part laborious and elaborate analyses of the different data of observation, render the work an *édition de luxe*. The contributions with which this work enriches Indian meteorology are twofold, viz., the results of the hourly observations made for many years at Kurrachee, Deesa, Bombay, Poona, and Belgaum; and the monthly averages for numerous stations throughout the Presidency, from which the temperature, rainfall, and winds of this part of Asia are charted with a fulness and consequent approximation to the truth not hitherto attainable. The influence which the broad physical features of the region, such as its lofty mountain ranges, high plateaus, river valleys, and extensive sandy deserts, has on the climatology of the Presidency is worked out with great skill and ability. Still more able are the discussions of the hourly observations of pressure, temperature, humidity, and cloud, made at the five chief stations, together with many suggestive reflections on the results developed, which will well repay the reader's best attention, even though he may sometimes not see his way to agree with the opinions expressed.

A healthy feature of Indian meteorology is the vigorous manner in which the making of hourly observations is pushed at many stations which have been admirably chosen as respects the objects sought to be attained, and the comparatively full and prompt discussions of the results which are published from time to time. Of the problems handled in those discussions the most frequent as well as the most important is that of the diurnal oscillations of the barometer. To this very difficult problem Mr. J. Eliot, for example, makes a valuable contribution in a paper on two storms in Bengal during 1876, which were accompanied with increased atmospheric pressure, and the apparent reversal of the normal diurnal oscillation of the barometer. This reversal was found to be accompanied with an instantaneous and complete change of wind direction and a great fall of temperature, which, as they occurred before the rain began to fall, proved that they were not due merely to an inrush of a strong humid current from the Bay of Bengal. The sudden chilling of the air, accompanied as it was by an increase of pressure, also proved that the changes were not due to the internal action of a mass of air or to horizontal or surface currents from the interior, which would have been warm currents, but that they were probably produced by the down-rush of a cold upper current, a conclusion which will doubtless receive further examination not only from its bearing on barometric fluctuations but also on the theory of storms.

OUR BIG GUNS

WE may leave the explanation of the disaster on board the *Thunderer* for the present to those who have been appointed to inquire into the matter. But in the mean time it will be well to consider what are the elements of weakness, if any, in the construction of our big guns.

The system of building up large guns by shrinking coiled iron tubes over a central steel tube seems extremely well adapted to prevent a *lateral* explosion; for even when the steel tube has had a *longitudinal* crack, the gun has been frequently fired without any further evil consequence.

But our guns are manifestly deficient in *longitudinal* strength, for the steel tube is the only tube continuous from end to end. If, then, there should be any ring-crack in the steel tube, there is little to prevent its separation into two parts beyond the friction of the coiled tubes, and the dove-tailing by which it is attempted to join the coiled cylinders together.

Now considerable longitudinal stress on the steel tube must be caused every round by the rifling necessary to give the shot its proper rotation, and occasionally, by a jamming of the shot. Also every discharge of the gun must cause a violent vibration in every part, and should the junction of the 1 B coil with the C coil and breech-piece work rather loose, this would be likely to cause a ring-crack in the steel tube in that neighbourhood.

When rapidly-exploding powder was used in the service the guns were very properly rifled with an *increasing* twist with a view to remove every possible obstruction to the *initial* motion of the shot. The increasing twist is still in use notwithstanding all the efforts that have been made to manufacture a powder that will burn slowly, so as to make the propelling pressure on the shot more nearly uniform. With a view to distribute the work of giving rotation to the shot uniformly along the bore, the rifling should be calculated to give a nearly constant pressure on the studs. But this depends upon the law of explosion of the powder. And this law is very variable, and very little understood. Only we know this—that the more nearly the force propelling the shot becomes constant, the more nearly the rifling approaches the uniform twist in order to obtain a constant pressure on the studs. Now the objection to the increasing twist is that it throws the chief part of the work of giving rotation towards the muzzle, where the gun is weakest. Also there is a difficulty in arranging the studs on the shot, and it now appears that the increasing twist allows the shot to slip forward when the gun is depressed. It seems, therefore, desirable to revert to the uniform twist of rifling now an improved powder is used.

But in order to give the gun additional strength in direction of its length, it seems desirable that the steel

tube should be supported by an outer tube of equal length and thickness, but composed of more tenacious metal—wrought iron or gun metal.

If this cannot be satisfactorily accomplished, then the steel tube might be covered with at least two layers of coiled tubes—breaking joint. In this case the tubes should be *screwed* as well as shrunk over each other. If the screwed part was slightly conical it would be possible to adjust the tension with nicety. B.

January 25

THE ELECTRICITY OF THE TORPEDO

THE recent researches of Prof. Marey on the electric discharge of the torpedo have been presented by the author in an extended memoir published last year.[1] We propose to present to our readers the main conclusions reached by M. Marey, and the experimental demonstrations on which the principal of these are based. But before entering into details of the experiments let us indicate summarily the processes employed by M. Marey.

In previous researches,[2] made in 1871, he had at his disposal only the reactions of the muscles of the frog to analyse the electric phenomena of the torpedo; he caused to be recorded, upon an inclosed plate, the shock of a frog's muscle produced by the discharge of the electric apparatus of the torpedo. The instant of the excitation of an electric nerve or of the nervous centres of the torpedo was recognised; and it was seen that the movement of the foot of the frog presented, at the instant of excitation, a considerable retardation, equal, *e.g.*, to four-hundreds of a second, measured on the chronographic scale. But into this total retardation entered several diverse elements, which M. Marey took into account by causing the muscle of the frog to contract by an excitation directly acting upon it. The time lost by the muscle of the frog representing nearly the half of the total retardation, it was concluded that the time-test by the electric apparatus is equal to that of the muscle of the frog.

Since these first researches, M. Marey has been able to study more directly the electricity of the torpedo by making use of the electro-magnetic signals of M. Deprez and of Lippmann's electrometer.

M. Deprez's signal is composed of a small electro-magnet provided with an extremely light armature of soft iron, which is applied to the coils when the current which traverses them is closed, and which is drawn from it, without delay in demagnetisation, at the moment of the rupture of the current, by the contraction of the tight india-rubber thread. The armature is provided with a style which traces on the inclosed cylinder the closures and ruptures of a current, the duration and frequency of these successive acts, with such perfection that it is easy thus to obtain the record of 1,000 vibrations per second. In the tracing underneath the apparatus (Fig. 1) is seen the signals which it furnishes when acted on by a non-continuous scale of 500 simple vibrations per second.

It is this electro-magnetic signal which M. Marey placed in the circuit formed by the torpedo, whose apparatus was held between two metallic plates joined to the coils of the apparatus by two conducting-wires. We shall see, further on, what use he has been able to make of this.

The second instrument by means of which certain special points of the experiments have been made is Lippmann's capillary electrometer. This apparatus is formed essentially of a column of mercury sustained by capillarity, in a tube of extremely fine glass, the extremity of which is plunged in a bath of dilute acid. When the mercury of the apparatus and the acidulated water are placed in connection with two points of an electric circuit of unequal tensions the capillary column is displaced and is carried towards the side of strongest tension. This displacement is instantaneous, and if the variations of electric tension are produced successively with great rapidity we need not fear the inertia of the capillary column. All the variations are signalled whatever be their frequency. But as the movements of the capillary column cannot be registered themselves, M. Marey has had recourse to photography in a certain number of experiments.

Let us now consider the results following the order which we have indicated at the outset.

1. *A torpedo's discharge is not a continuous current; it is formed of a series of successive waves added one upon another.*

The fundamental experiment upon which the demonstration of this proposition rests was performed with the electro-magnetic signal (Naples, October, 1876). Having compressed one part of the apparatus of an active torpedo just drawn from the water between two metallic plates furnished with conducting wires, M. Marey placed the signal-machine of M Deprez in contact, and the magnet being stimulated he heard a shrill noise resembling that made by filing the end of a hard splinter of wood. The vibrations of the armature, therefore, had been produced by a series of successive electric acts. In defining these vibrations one is justified in stating that the discharge of the torpedo produced by the animal as the result of a local excitation, was composed of a variable number of waves or currents succeeding each other. Fig. 2 represents two tracings so produced. The great advantage resulting from the use of the electro-magnetic signal was to show definitely that the discharge is complex, an analysis which was not possible with the frog's-foot signal. The muscle used as reagent does not in fact react by means of the shocks apart from impulses which are sudden and frequent; it remains in a state of permanent contraction.

But the electro-magnetic signal, whilst showing the dissociation of the torpedo discharge, furnished no other result. It did not indicate how those successive waves follow each other, it seemed even to lead to the conclusion that one wave is quite completed when the next succeeds. At this point the induction is interrupted and the experimentalist adopts another mode of solving this question of the succession of waves in a discharge. M. Marey, in fact, being convinced that the electric action of the torpedo and muscular action should be assimilated, and wishing to see in the discharge the analogue of induced tetanus and even of voluntary contraction, could not resign himself to the admission of an absolute discontinuity between the successive acts constituting a discharge. Yet the electro-magnetic signal apparatus seemed to pronounce his theory wrong. But on passing through Lippmann's electrometer a slight current from the total discharge, M. Marey observed that the column underwent a series of successive impulses, the effects of which unite together. This progression by successive jerks indicated an increase of the intensity of the discharge, an increase in which each new wave is joined to what remains from those which have preceded it. Thus we derive the proof that the electric waves are *partially* united to one another like the muscular shocks of a tetanised muscle.

This first fact being gained, it was necessary to follow up the analysis of the torpedo-discharge, determine the nature of each of the independent electric acts which the electro-magnetic signal had revealed, measure their duration, phases, &c. These different points have been elucidated, each in its turn.

2. *To measure the duration of the electric-wave in the torpedo,* M. Marey has had recourse to the method devised by Guillemin for determining that of very short current, and used afterwards by Bernstein to measure the

[1] "Compte Rendu des Travaux du Laboratoire de M. Marey." T. iii. Paris: G. Masson. 1877.

[2] "Annales de l'École Normale Supérieure." 2e s., t. i., pp. 86-114.

duration of the negative variation of nerves and muscles. Guillemin's method is applicable in every case where a current passes several times in succession through a metallic circuit, with duration always the same. The electric condition of the circuit is investigated during a succession of very short intervals, beginning at the moment when current is complete.

The apparatus used by Guillemin and Bernstein was the galvanometer; M. Marey preferred to use a frog's foot, which, in the successive investigation, gives a movement which can be graphically recorded, as often as there is an electric current.

The graphic method, by which each duration is transformed into a length easily measured on the paper, is easily applied in performing those experiments of which we are about to explain the principle.

Let the point O (Fig. 3) correspond to the moment of electric excitation of a torpedo-nerve, and let the successive points 1, 2, 3, &c., denote successive hundredths of a second, which correspond to very short intervals during which the torpedo apparatus is put in contact with a metallic circuit passing through a frog's foot. In the two first trials, 1 and 2, after the excitation of the electric nerve, there are no signals recorded; the frog's foot remaining motionless shows that the discharge of the torpedo has not reached it, because, in fact, the phenomenon has not yet had time to take place. But at the instant 3 the frog moves, which is expressed in the diagram by a vertical stroke; at the instants 4, 5, 6, 7, 8, 9, and 10, the frog receives shocks which are indicated on the diagram by vertical lines; and finally, at the instant 11, and those succeeding, the frog shows no action, whence we conclude that the electric wave of the torpedo was finished before these last trials; and we see that, according to the tracing, the wave began three-hundredths of a second after the instant of nerve-excitation and finished ten-hundredths after the same instant.

Fig. 1.

It was exactly in the same way that M. Marey proceeded to measure the duration of the electric wave in the torpedo. An arrangement easily fixed induced electric action in *e* (Fig. 4) at constant intervals. A metallic contact, susceptible of being displaced at will, allowed him, during very short intervals of different lengths, to complete the circuit made by the electric wave of the torpedo to reach the frog's-foot-signal. Moreover, to avoid confusion of the curves which were registered by the successive experiments, he took care to change the position of the style each time, so that the curves appeared one under the other in order.

Fig. 4 shows that the first appearance of the electric wave took place at instant 1; that in a series of successive trials, each later than the preceding, after the excitation of the nerve, the wave was indicated at the instants 2, 3, 4, 5, and 6; and that at the 7th trial the frog gave no signal. The wave, therefore, was completed. Finally, by bringing the instant of trial nearer to that of nerve-excitation, the wave was retraced in experiments 8, 9, 10, 11, and 12; but in the 13th, occurring too soon after the instant of nerve-excitation, it was shown that the electric wave no longer existed.

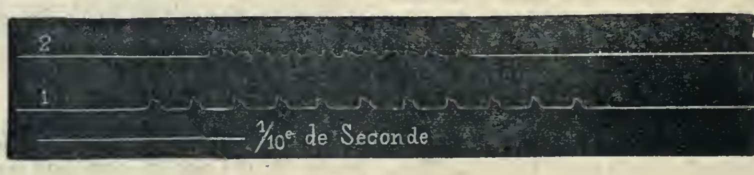


Fig. 2.

The approximation of these measurements necessarily depends on the number of successive trials, and is more delicate in proportion as they succeed each other more frequently.

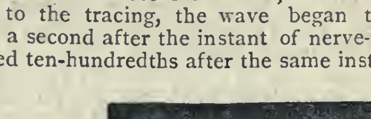


Fig 3.

3. *Each electric wave presents a phase of suddenly increasing intensity, followed by a phase of gradually decreasing intensity.*

On examining the tracings of electric waves obtained by the electric-magnetic signal, we observe an apparent contradiction between the indication, the wave-duration furnished by this apparatus, and that which we have just seen determined by the frog's foot. The waves traced by the signal of Deprez seem to measure not more than one-hundredth of a second; by Guillemin's method, on the contrary, their duration is much more considerable, being seven-hundredths of a second. This apparent contradiction results from the fact that in the torpedo the waves have not sufficient energy during the whole of their duration to act upon the signal, whereas, from beginning to end of their course they can act upon the frog's muscle, which is much more sensitive. There are, then, in every electric wave, phases of increasing intensity and decreasing intensity which remain to be determined.

M. Marey has endeavoured to obtain a tracing of these phases of variable intensity by a modification of the apparatus of M. Deprez. Instead of limiting the excursion of the style between two fixed obstacles, he allowed it an excursion which varies and is proportional to the intensity of the currents acting upon it.

With this object an india-rubber thread, bent over two bridges, was stretched horizontally between the soft iron bars of the armature (Fig. 5). The bars had a groove filed on the top to receive two demi-cylinders of metal which were soldered to the lower part of the armature. In this way the nearer those parts are brought which are subjected to the magnetic attraction the greater is the resistance. Thus if we consider the armature in its different stages when gradually lowered, first it meets the elastic thread with the two demi-cylinders borne on its lower surface, and then the extensibility of the thread is very great. But as the thread is lowered more and more, it rests on points more and more separated, and becomes less and less extensible. Lower down the india-rubber thread stretched

over the groove made in the soft-iron bars is still less extensible; and finally, when the thread has taken the curvature of the surrounding parts, it opposes any further descent of the armature with the resistance which a stretched thread of india-rubber presents against being pressed or crushed.

This apparatus, to which M. Marey has given the name *electrodynamograph*, has still to receive further improvements, but even as it is, it has already furnished some interesting evidence as to the decrease in volume of the electric waves from the beginning to the end of the discharge; also as to the shape of these waves and the occurrence in the electric tetanus produced by strychnine, &c. Of these different results we shall at present consider only one—the form of a wave is traced by the electro-dynamograph:—an investigation which brings us to the analysis of the wave-phases.

In Fig. 6 the continuous line *b* is the tracing of a single wave obtained with the electro-dynamograph. From it alone we already have evidence that the ascending phase is much more sudden than the descending phase as also takes place in a muscular shock. We can

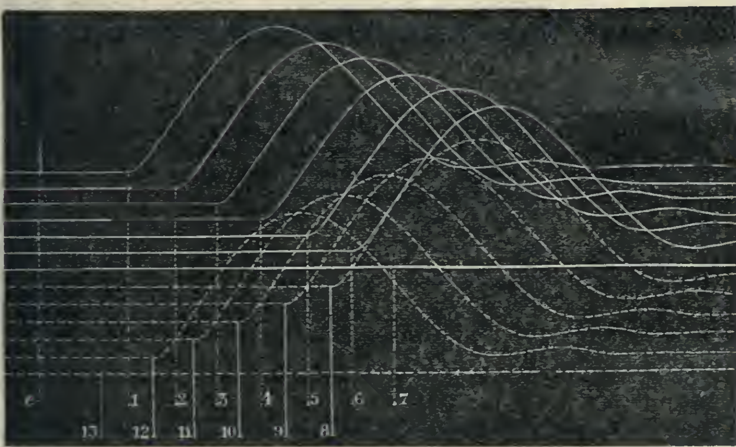


FIG. 4.

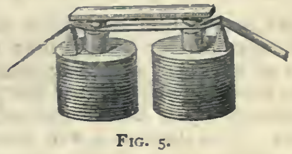


FIG. 5.

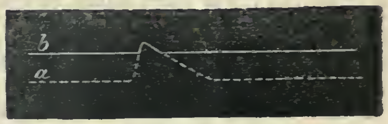


FIG. 6.

further theoretically complete the curve by taking into account what we have learned by Guillemin's method in the preceding paragraph about the duration of a wave. All that is necessary is to produce downwards the two ascending and descending lines till they intercept between them a distance equal to that which represents (on the time-line) the duration of the whole wave. Thus (Fig. 6) the pointed line *a* represents the actual position of the axis of abscissas and that part of the tracing which the instrument was unable to trace on account of its insufficient sensibility.

It is true that this curve is only probable, but there are great presumptions in favour of its reality. The points of origin and termination can determine it experimentally, as we have seen done by Guillemin's method (see 2).

We can now understand the reason of the special characteristics presented by currents induced in a secondary coil by the waves of a torpedo discharge which have been passed through an inducting coil. The phase of sudden increase of each wave is alone capable of giving birth to an inducted current. FRANCOIS FRANCK

(*To be continued.*)

GEOGRAPHICAL NOTES

AT the meeting of the Geographical Society on Monday, Sir H. Rawlinson read a paper, On the road to Merv from the Caspian. After some interesting remarks on the comparative geography of the eastern shores of the Caspian Sea, Sir Henry read some portion of the Russian letters on the earlier stages of the road to Merv, of which a summary appeared in our last issue, and afterwards gave from Russian official documents an account of two ancient cities, the probable relics of Khowrasmian times—Mestorian, or Mestdovran, and Meshed. The former in past ages was one of the most important cities of Central Asia, if one may judge from the remarkable aqueducts leading into it, which were the chief arteries of an entire system of irrigation canals thoroughly watering the whole country, and from the number of its buildings, the remains of which exist to this day. The course of the aqueduct was explored by General Lomakine's orders some two or three years ago, and was traced to the Sumbar, a tributary of the Attrek, a length of some sixty-five versts. The city of Mestorian appears to have consisted of a citadel and of two other inclosures with thick, high walls built of enormous bricks. The mass of the *débris* at the place is so extensive and in such good preservation, that it would be possible, we are told, to make use of it for building a large new town! The bricks, it may be added, are stated to be as hard as stone, and often carved and ornamented with friezes in relief, arabesques, and well-executed inscriptions; the last are sometimes in various colours, illuminated with flowers, and the letters about seven inches in height. Five versts from Mestorian is another remarkable place, known in the country as Meshed; it is, strictly speaking, an ancient necropolis. Here, according to report, is an open coffer holding the sacred books, a hanging lamp, and vases for ablutions, and although in a desert place and wholly unprotected, no one dreams of touching its contents. Sir Henry Rawlinson afterwards dealt at some length with the geography of the country further to the eastward, more especially with that on the northern slopes of the Attock, which is inhabited by three divisions of the Tekké Turcoman tribes.

WE regret to record the death, on Saturday afternoon, at a comparatively early age, of Commander G. C. Musters, so well known as the explorer of Patagonia. His work, "At Home with the Patagonians," is at present the best authority we have on this inhospitable country and its people, and Mr. Musters, as readers of the work know, obtained his information by living with the Patagonians for many months as their "king," and it was only by a *ruse* that he managed to get away from a people who

had learned to adore him. Mr. Musters was a fearless explorer, and a man of unfailing tact and winning manners. Two or three years ago Mr. Musters returned from Bolivia, with which little-known country he had made a thorough acquaintance, and had, we believe, collected material for an interesting work. He recently received the appointment of Consul at Mozambique, and was to have left this month for his post, from which, he was delighted to think, he would be able to do some valuable exploring work in the African interior. By his death Her Majesty has lost a faithful and able servant, and science an eager explorer. Musters was loved by every one who had the pleasure of his acquaintance.

THE prosecution of marine surveys on the coasts of India has been for a long time much hampered by the want of a proper surveying steamer, and we are glad to learn from a Bombay paper that a new vessel—the *Investigator*—has just been launched, which will supply the defect. The steamer is well provided with all the necessary appliances for chart-making, deep-sea sounding, &c.

THE November number of the *Bulletin* of the French Geographical Society contains Dr. Jules Crevaux's account of his exploration in the interior of French Guiana in 1877. Dr. Crevaux, with little assistance and in the face of not a few difficulties, ascended the River Maroni, and striking the River Yary, traced its course to its junction with the Amazon. The two main results of his journey are the crossing for the first time of the Tumuc-Humac chain at the level of the sources of the Maroni, and the discovery and complete delineation of the Yary, an important affluent of the Amazon. The Maroni he describes as a fine river of about 140 leagues in length, with a breadth of 1,200 to 1,500 metres at 20 leagues above its mouth, and from 400 to 500 at 90 leagues. The River Yary, Dr. Crevaux considers as more important than the Maroni; it is 150 leagues long, and both rivers are much obstructed by falls and cataracts. Dr. Crevaux gives some very useful notes on the forests of Guiana and the different species of trees which they contain. The highest summits of the Tumuc-Humac range do not exceed 400 metres above sea-level. In summing up his observations on the geology of the region traversed, he says that all the formations met with from the mouth of the Maroni to that of the Yary have an ancient physiognomy. They are mainly composed of schistose rocks which may be divided into three systems, which are, in order of age—1. The gneiss of the mouth of the Maroni. 2. The schists and mica schists of the middle course of the river. 3. The ferruginous schists and quartzites of the Man-Bari and the Yary; these latter are very wide-spread. All these are frequently traversed by granites and trachytes.

THE possibility of water communication between the Obi and the Yenissei seems to be more and more confirmed by further explorations. Baron Aminoff, after having explored the water-parting between these rivers, arrives at the conclusion that the hydraulic works which would be necessary for the construction of a canal with sluices would not present serious difficulties. The canal would be very short, and the marshes at the sources of the Kas and Yazevaya rivers afford a sufficient amount of water.

THE *Golos* of January 22 says it learns that authentic intelligence respecting Prof. Nordenskjöld's Arctic expedition has been received from Baron Fredericks, Governor-General of Eastern Siberia. According to these advices the steamer *Vega* is ice-bound forty miles from East Cape. The authorities at Jakutsk have been instructed to inform the natives of the dangerous position of the steamer, and to issue a general summons to the people to render assistance to the expedition. At the same time a special expedition has been organised which will attempt to reach the *Vega* by a journey over the ice with the aid of reindeers or dogs. Herr Sibiriakoff has telegraphed to Baron Fredericks, asking him to send a party to the assistance of the Swedish Expedition. He has received a letter from Dr. Lindemann, of Bremen, in which the former says that at the coast where the *Vega* is believed to be lying there is a large native village, and from this village the nearest post of white merchants is distant only about 200 English miles, which may be traversed in winter in three or four days.

A REPORT has been received of a journey by Mr. Baber in the north-west of the Chinese province of Szuchuen. The original intention was to examine, between Suchow and Kiating, the River Tatu, which falls into the Yangtze-Kiang, and then to cross the mountains from Kiating to Fu-lin in long. 103°. At Fu-lin, however, Mr. Baber was induced to extend his exertions into the country further west, and he travelled onwards to a place called Tzu-Tati, the head-quarters of a Sifan chief. Here he heard of the existence of a mountain path to Ta-chien-lu, the French missionary station lying nearest to Tibet. After travelling three days through pine forests, the mountain range was crossed by a snowy pass, and on the northern slope yaks were found grazing, and many slates, inscribed with Sanskrit characters, were noticed. The appearance and language of the people also pointed to the fact that though Tibet proper is many hundred miles west of this point, yet tribes of the Tibetan race and language extend up to the banks of the Tatu River. This confirms the views already expressed by Mr. T. T. Cooper and other travellers.

THE *Japan Gazette* translates an article of some interest from the *Osaka Nippo*, on the subject of Japanese relations with Corea. After commenting on the progress made by Japan during the past few years, the writer remarks that "the Coreans, on the contrary, obstinately cling to the customs of Gio, Wu, and Shin, three Chinese sages who lived 3,000 years ago, and they believe their country is a part of Paradise, next in rank to China, and that western countries are inhabited by barbarians and savages."

COUNT WILCZEK and Lieut. Weyprecht intend to visit the northern coast of Novaya Zemlya in the course of this year, and will remain at that station for a twelvemonth in order to make a series of exact magnetic, electric, hydrographical, and meteorological observations. The cost of the expedition will be defrayed by Count Wilczek himself.

OUR ASTRONOMICAL COLUMN

THE VARIABLE-STAR ALGOL.—Considering the long period during which this star has been under observation, and the many investigations which have been made with the view to reduce its fluctuations of light within some law, much interest attaches to a remark by Prof. Winnecke that the times of *minima* of Algol in the last year have fallen about one hour earlier than those assigned in the ephemerides of variables published by the *Astronomische Gesellschaft*. Dr. Julius Schmidt, director of the Observatory at Athens, to whom we owe the greater number of recent observations on this star, has not yet made known his results for 1878, but we have his determinations of the times of *minima* in 1875-76-77. For comparison with them we may take the last formula given by Prof. Schönfeld in his second catalogue of the variable stars, which appeared in 1875, viz., for Paris mean time:—

Min. ... 1869, Nov. 9, 3h. 39m. 34s. + 2d. 20h. 48m. 53s·67 E.

The following are the differences from the observed times of *minima*, during the last six months of each year, wherein the observations are most numerous:—

1875.	m.	1876.	m.	1877.	m.
Aug. 20 ...	+ 10·1	July 29 ...	+ 26·1	Aug. 20 ...	+ 62·1
,, 23 ...	− 29·6	Aug. 21 ...	+ 7·2	,, 23 ...	+ 18·5
Sept. 9 ...	− 7·2	Sept. 10 ...	+ 9·4	Sept. 12 ...	+ 33·3
,, 12 ...	− 15·8	,, 13 ...	+ 21·9	,, 15 ...	+ 17·1
Oct. 5 ...	+ 7·1	Oct. 3 ...	+ 3·8	Oct. 8 ...	+ 54·0
,, 28 ...	+ 17·2	,, 6 ...	+ 1·8	Dec. 10 ...	+ 70·1
Nov. 20 ...	− 15·1	,, 9 ...	− 1·2	,, 13 ...	+ 30·6
		,, 26 ...	+ 52·0		
		Dec. 11 ...	+ 53·9		

If yearly means of the above differences are taken we have:—

	m.
1875·76	− 4·8
1876·76	+ 19·4
1877·73	+ 40·8

These figures appear to indicate that a perturbation of the period is taking place, which of late has increased the differences between observation and calculation at the rate of about twenty-two minutes annually; the star is evidently one which deserves close attention at the hands of those observers who are following up the variables. With reference to previous observations of Algol and results derived from them, the reader will best consult Argelander in the seventh volume of the Bonn observations, and Schönfeld in *Vierteljahrsschrift der astronomischen Gesellschaft*, vi. p. 60.

The Reappearance of Brorsen's Comet.—It is notified that M. Tempel, director of the observatory at Arcetri, Florence, detected the short-period comet of Brorsen on January 14, in a position north following the nebula No. 4900 of Sir John Herschel's General Catalogue. The ephemeris by Dr. Schulze, of Döbeln, who has carried on the calculation of the perturbations from the last appearance of the comet in 1873 to the present year, does not commence until February 19, so that it appears to have occurred to M. Tempel that, with his advantages of climate and optical means, there was a possibility of an earlier observation of the comet, and he has taken steps to that effect accordingly. Dr. Schulze's elements for 1879 give for the place of the comet on January 14, at 6h. M.T. at Florence, R.A. 23h. 10m. 38s., N.P.D. 118° 57′, which is north—following the nebula named, so that there is no reason to doubt that the object observed, which is described as small, but brighter than the nebula (one of Sir W. Herschel's second class) is really Brorsen's comet. In this case, however, M. Tempel has succeeded in observing the comet, when, according to theory, it possessed a much less intensity of light than at any previous observation. At the time of his observation it would be distant from the sun 1·42, and from the earth 1·915, whence the theoretical intensity of light, represented by $\frac{1}{r^2\Delta^2}$, is 0·135; the smallest value with which it had previously been observed corresponded to the last glimpse of the comet at Berlin on June 22, 1857, viz., 0·337. Dr. Schulze's ephemeris will be found in No. 2220 of the *Astronomische Nachrichten*, commencing, as we have said, on February 19. On February 7 the comet's position at 6h. G.M.T. is in R.A. 23h. 59·9m., N.P.D. 109° 42′, and on February 11, at the same hour, in R.A. 0h. 9·9m., N.P.D. 107° 46′.

SUN-SPOTS AND THE NILE

On the 21st instant Mr. Francis Cobb read a paper on the financial and economical condition of Egypt, at the Society of Arts, in which he of course referred to the periodical rise of the Nile, and spoke of the desirability of discovering some system in the variations of this rise. Mr. Cobb, in considering this subject, has been naturally drawn to an examination of the sun-spot period, and has attempted to discover if any relation exists between this period and the variations in the rise of the river. The period of Mr. Cobb's examination extends from 1866 to 1878, and as might have been expected, he finds no relation whatever between any sun-spot maxima and minima, and the maxima and minima of the Nile floods. The years 1866-67 were sun-spot minima; the rise of the Nile in these years was 28¼ and 24½ feet respectively; 1872 was a maximum sun-spot year, and the rise of the Nile was 25½ feet; 1877 a minimum sun-spot year, with 18 feet rise in the river; last year the rise was 30 feet. As some of the speakers in the discussion remarked, there is at present a desire to find relations between the stupendous cosmical phenomenon of sun-spots, and terrestrial occurrences, without considering local peculiarities. We do not know what might be the result if the records of Nile floods for a century were obtainable, and were compared with the various sun-spot periods during that time; but we should say beforehand that in considering so literally narrow an occurrence as the rise of the Nile, many local considerations would have to be taken into account.

"It is impossible to say," Mr. Cobb went on, "that the rule, maximum spots, maximum rainfall, applies to Egypt. The cause of the irregularities of the Nile must clearly be looked for locally, the Blue Nile and Nyanza lakes having probably more to do with the matter than sun-spots. The telegraph, combined with a vigilant series of the operations of the Upper Nile, especially at the confluence of the Blue Nile, will prove more reliable for the protection of Egypt another year than any calculations based upon solar physics."

We fear Mr. Cobb has but a vague idea of the application of solar physics to meteorology and other terrestrial phenomena. A perusal of the many letters which appear in Nature from our Indian meteorologists, will show that without a careful consideration of local and regional conditions no deduction drawn from sun-spot periods *per se* are of much value.

In the discussion which followed Dr. Mann endeavoured to draw the attention of the meeting to the science of the subject. While he apparently endorsed Mr. Cobb's opinion that the spots on the sun would not be found to have any direct relation to the high and low Niles, he thought it would be perhaps as well to state exactly how this matter stood. The last development of the search after sun-spot influence, Dr. Mann said, took the form of the discovery that the constantly recurring financial crises in this country were due to the sun-spots; and he should like to point out what really was the influence of the sun upon the great physical changes going on in the world. There was no doubt that the presence of sun-spots had relation to the amount of force and energy issuing from the sun, and that when spots were abundant more solar energy was thrown out into space. When that was the case, the earth shared with all the other orbs in getting some increased force from the sun. There was no doubt either that movement of every kind on the earth was dependent on solar action; and when increased energy was thrown out from the sun it told immediately on the water of the earth, and raised more of it into the sky in the form of vapour. But this did not mean that there would be an increased rainfall in one particular spot, but only that, being more vapour, there would be a greater rainfall over the whole earth. In a case like Egypt, the amount of rainfall was due to the presence or absence of an ocean wind blowing over the high grounds of Abyssinia. Therefore, though no doubt the sun-spots had to do with the total rainfall, they had not necessarily anything to do with the local rainfall in one particular country like Egypt.

Dr. Mann explained that in these remarks he did not intend to imply that there was not a periodicity and regular order of some kind in social conditions and events which were connected with the requirements of finance, crisis, and things of that kind. He was quite satisfied that there was. But he thought there was too great an

inclination to refer locally restricted events to large general causes.

Mr. Hyde Clarke, who was in the chair, drew attention to the fact that it was by a paper of his thirty years ago that public attention was first directed in what he might term a scientific form to this periodicity. Prof. Stanley Jevons, who was the great advocate for the application of the sun-spot theory to commercial crises, had reproduced the statements he made thirty years ago, and thus fresh attention had been called to them. For his part, he was no advocate for what was called the sun-spot theory, for he believed the sun-spots had no direct bearing on the periodicity of commercial crises, or upon the height of the Nile; but as what Dr. Mann had said might appear to throw discredit on the periodicity of crises, he would briefly revert to the facts to which he had formerly called attention. He had then gone through the corn harvests, as shown by the prices in England for the last 400 years, for which data could be obtained, and his observations, which had since been repeated by Prof. Jevons, gave a series of facts over six centuries, showing that there was a periodicity in the crops, and consequently in the commercial phenomena dependent on them, of somewhere about ten years. Prof. Jevons had fastened on to that one fact, but had not referred to other observations he had made, which gave the clue to the question Mr. Cobb had raised, whether it was possible to predict these periods. There was certainly, in a long period, a periodicity of about ten years, and if you laid out a diagram you would find this plainly shown, but yet in some places the lines of dearth or plenty would seem to come in the wrong place, and no one has yet been able to hit on the true law. He had stated that, as far as he could discover from the facts before him, there were, besides the periods of ten years, other periods of about twenty-six years, and likewise a period of about 104 years, and the opinion he formed was that these longer periods interfered with the shorter ones, and prevented any absolute calculation as to the future. At the same time the observation of these phenomena was not by any means an idle matter; there was this practical lesson to be drawn from it, that in periods of prosperity we must look forward to a period of adversity and prepare for it. Therefore the observation of Governments, and of the commercial community and financial institutions should be directed to these great phenomena of nature, which, after all, did govern the individual operations of man.

And this is all we contend for. That there is a connection between certain well-known cosmical phenomena, centring in the sun-spot period, is admitted by all whose researches give them a right to pronounce an opinion on the subject. What is the exact nature of this connection has yet to be discovered, though that we are on the road to it every careful reader of NATURE must admit. The immense social and economical results depending on the definite ascertainment of this connection make it the bounden duty and the interest of civilised Governments to do all in their power to further research in this direction, and we have no doubt that when the full truth is known it will be found that even the apparently capricious Nile is obedient to influences that may be regarded as ultimately cosmical.

NOTES

WE are pleased to see a suggestion in the *Midland Counties Herald* that in considering the arrangements for the restoration of the Reference Library, recently almost destroyed by fire, the authorities will not miss the opportunity they now have of supplying an omission in the public institutions of Birmingham, by organising a Natural History Museum, of equal value with the Reference Library which they are doing their best to restore. We heartily endorse this suggestion, and indeed it seems strange that so energetic and intelligent a town as Birmingham, with one of our most enterprising Natural History Societies in its midst, should not have had such an institution long ago. We are sure the matter only needs to be properly brought before the authorities and the citizens to have the blank speedily and properly filled up.

MR. JOHN SADLER, so long assistant to Prof. Balfour, has been appointed to succeed the late Mr. McNab as curator of the Royal Botanic Gardens, Edinburgh.

IT is expected that Russian Turkestan will be very well represented at the anthropological exhibition which will be opened next summer at Moscow. We may already mention a very interesting collection of some dozens of skulls, found at Samarkand and belonging to a very remote epoch. A collection of dresses and implements of the inhabitants of the Zarafshan valley will be accompanied by a collection of ethnographic photographs; and among the inhabitants of this valley, the photographs and the skulls from the Galchi tribe will probably draw the special attention of the scientific world. This tribe, which lives in the clefts of the Hindu-Kush at the sources of Zarafshan river, differs from all other Central Asian tribes, and is said to be the remnant of the army of Alexander the Great; indeed, its features are like those of the Greeks; but the tribe remains almost quite unexplored, because of their wildness and the insecurity of travel in those regions. Altogether, the Zarafshan district sends to the exhibition plenty of very valuable anthropological and ethnographical materials.

THE unveiling of the Humboldt monument in Tower Grave Park, St. Louis, U.S., took place on December 24 last. The monument, as our readers will remember, is cast in bronze and executed after the design of the eminent German sculptor, Herr Ferdinand von Miller.

THE Berlin Humboldt Academy, founded by the Scientific Central Union of that city, was inaugurated on January 13 last.

THE competitive examination held at the Paris Conservatoire des Arts et Métiers for the appointment of a Professor of Physics and Meteorology to the National School of Agriculture is said to have been very brilliant. It has ended by the appointment of M. Duclaux, Professor to the Faculty of Lyons, who was trained by M. Pasteur.

THE *Times* Paris Correspondent telegraphs on January 24 that the eruption of mud at the foot of Mount Etna was still going on, but with varying intensity. For two days after the earthquake of the 24th ult. it was considerably stimulated, but it has since slackened, and the mud is more watery. An area of 7,000 square metres is already covered.

A CORRESPONDENT of the *Colonies and India*, writing from Wellington, New Zealand, on December 7, says that a most important discovery of graphite has just been made in the back portion of the province. The Colonial laboratory has received specimens from boulders found in a creek, and these prove to be the purest and most compact samples yet discovered in the Colonies. The value of the discovery is enhanced by the fact that the existence of coal in immediate proximity is thus indicated. In another spot, between Westport and Keefton, an extensive limestone cave has been discovered, and it is stated that it is traversed by a creek yielding good payable gold. The Geological Survey is being steadily pushed on, and Dr. Hector is now attempting to work his way to Waikato, in order to gather information as to the geology of that hitherto unexplored region.

MESSRS. LECHERTIER, BARBE, AND CO., of Regent Street, have sent us a wonderful shilling moist colour-box, which, in utility and the quality of the colours, surpasses anything we have seen.

It is of japanned tin, can be put in the pocket, has every convenience for immediate use, and is altogether excellent and astonishingly cheap. It is a pity that students of science generally don't know how to use colours to give clearness to their note-books and diagrams.

MR. BRYCE WRIGHT has just issued a catalogue of his extensive mineralogical, geological, conchological, and archæological specimens and collections, with several well-executed illustrations. Those interested in the subjects mentioned would do well to obtain a copy of the Catalogue.

MR. GOWER, an American, who has lectured in America with Mr. Bell on the telephone, has realised an interesting improvement on Bell's instrument. The new telephone differs mainly in the form of the magnet, which has been calculated so precisely that the sounds can be heard at any distance from the speaker in a large room. The telephonic current is so powerful that the contact of a magnet can be worked by it, and a signal given in a central telephonic office.

HERR PETZOLD, of Vienna, sends us several specimens of insects preserved apparently in Canada balsam and mounted on microscopic slides, which surpass in several respects anything of the kind we have seen. Herr Petzold informs us that for years he has been working to devise some means of preserving insects and other small animals in such a way as to prevent their being injured by accident or by any of the numerous enemies of museum collections. By a process of mummification, and inclosure in a transparent material he seems to us to have succeeded. The specimens sent can be clearly seen, are natural and life-like in appearance, and completely protected from all atmospheric influences.

THE fall of sleet which occurred in Central France on January 23 was so terrific that an immense number of large branches were broken by the weight of icicles adhering to the leaves. Almost all telegraphic communication between Paris and Central or Southern France was interrupted by the breaking of the telegraphic wires. The messages from Paris to Marseilles were sent *viâ* London, Lisbon, and Malta. This state of things, which had been anticipated by *Électricité*, raised a number of angry remarks from the principal papers. It is very likely that the German system of entombing the wires of the large lines will be resorted to, and special credits asked from the Chamber of Deputies next session.

THE Postal Microscopical Society is not, as its name would seem to imply, a Microscopical Society for Post Office officials. It embraces a much wider constituency, being commensurate (potentially) with the area of the kingdom embraced by our postal service. It is, in short, an association for the distribution by post of microscopical slides among its members, with facilities for these members making remarks on the slides they receive. From the Report of the fifth annual meeting we see the Society has many members over England, that the organisation is excellent, and works well. Several improvements are contemplated in the sphere of this Society. In consequence of a number of medical men having recently joined the Society, it has been arranged to circulate a special series of histo- and pathological slides. These special slides will circulate almost exclusively amongst the medical members, in addition to the usual fortnightly box of slides which goes the whole circuit of the Society, whether members are medical or otherwise. It is also proposed at the request of many members, to circulate a series of slides devoted to botanical subjects; these, after going the round of the contributors, it is proposed should go the whole circuit of the members. Those desiring to join the Society should apply to Mr Alfred Allen, 1, Cambridge Place, Bath.

AN occasional correspondent of the *Daily Press* of Hongkong, gives a somewhat melancholy account of the condition of affairs in Formosa, where it was hoped that the Chinese were showing signs of progress. The Woosung Railway plant, he says, continues to generate rust, the dredger so urgently needed has not been ordered, and the scheme for introducing Swatow coolie emigrants has fallen through. The accounts of the Kelung Colliery are not hopeful, sickness having prostrated European and native miners alike; in the petroleum region, too, all the members of the exploring party are reported to be laid up with fever. What is worse, however, in that quarter, is, the boring rod has snapped low down, and the American experts are said to have spent three weeks in a vain endeavour to connect it again. The only favourable item of news is that there is every prospect of a large sugar-crop in the south of the island.

A CONTEMPORARY in China states that petroleum is obtainable at several places in both North and South Formosa. Some time back a large spring was discovered in the hills to the south-east of the Port of Owlan, in about 24° 30′ N. lat. and 121° E. long. The principal spring is situated close to the Owlan River, at the foot of a hill. At certain times of the year the river overflows into this spring, and the oil is carried away down the stream. When the discovery of this well was made, the oil could be tasted in the water some distance off. On exploring the hills behind the spring, large fire-holes were found, and a small spring was met with on the top of a high hill. The lofty ranges of hills to the east of the petroleum valley have been explored to some extent, and in almost every range were found evidences of the existence of coal, but none of the veins or seams are being worked.

WE have received the first two numbers of the Italian *La Natura*, a weekly scientific journal, which we understand is the new form of *L'Elettricita*, founded some time since. *La Natura* is mainly devoted to the physical sciences, and, judging from the first two numbers, is likely to take a creditable place among its scientific contemporaries. In the first number Prof. Schiaparelli writes on the Perturbations communicated by Jupiter to Brorsen's comet in 1872, and in the second number on Recent Researches on the Topography and Physical Constitution of the Moon. There are several other good papers relating both to Italian and to foreign science.

Revue d'Hygiène et de Police Sanitaire is the title of a new sanitary monthly edited by Prof. E. Vallin and published by Masson of Paris.

THE *Annuaire* of the Bureau des Longitudes for 1879 contains as usual a great mass of useful and well-digested information. It contains an interesting paper by Dr. Janssen on Recent Advances in Solar Physics.

WE are glad to learn from the *Royal Gazette* of British Guiana that a Bird Ordinance has been promulgated in that colony, which is likely to prevent the stamping out of birds whose feathers are so eagerly sought after by ladies to add to their charms.

WE have received the first number of the *Revue Mycologique*, a three-weekly journal devoted to the subject of fungi, and edited by M. C. Roumeguère. The Paris publishers are Baillière and Sons.

IN the course of a recent excavation for a railway from Persan to Neuilly-en-Thelle, in the north of France, a field has been cut, which contains numerous sepultures, and was probably used as a cemetery at some early period. Nearly all the tombs (M. Millet tells us, in *La Nature*) are of hard stone and composed of two pieces (exceptionally three), with transverse joints, and the cover generally a single piece. One tomb is made of plaster.

Various objects have been met with, arms and armour, vases, ceramics, &c., and in one tomb, it is said, a warrior has been discovered fully equipped, and in such preservation that the beard was intact. M. Millet recalls the fact that in the invasion of Gaul by Julius Cæsar, there was a camp of great importance on the plateau above Gouvieux, some ten kilom. from the place of excavation, and on the route from Chantilly to Persan; the place is still known as Cæsar's Camp. Numerous battles took place in the valley of the Oise, as is attested by the medals, coins, &c., often found by farmers in that region. One of these combats was so murderous that the place where it was probably fought bears the name of *Pres de tuerie;* it is at the foot of Beaumont-sur-Oise. The excavations referred to are still in progress, and will doubtless be watched with interest.

The annual general meeting of the Manchester Field Naturalists' and Archæologists' Society was held on the 21st inst., Mr. John Angell, F.C.S., vice-president, in the chair. Mr. Alfred Griffiths, secretary, read the report for the past year, which stated that 1878 had been devoted to the aims of the Society, with an average success. Mr. Angell gave an address on the science of 1878, in which he reviewed, in an intelligent and appreciative manner, some of the main scientific points of interest during the past year.

The Geological "Landesanstalt" and Mining Academy at Berlin has recently been considerably enlarged. The institution has moved into new buildings which have just been completed, and which contain a rich collection of maps, minerals, rocks, fossils, &c., besides a large library and laboratories for geological, analytical, metallurgical, and technological work.

The existence of a subterranean oak forest in the neighbourhood of Rotenburg, Prussia, was proved last summer by the State geologist of that district, Dr. Moesta, of Marburg. The investigations of this gentleman have shown that in the plain of the Fulda valley an oak forest lies buried at a depth of some two or three metres, the origin of which dates back to the tertiary period perhaps, and of which the river Fulda has laid bare many traces by erosion. The wood of the oak trees thus brought to light has by the long action of the water been stained quite black, but still retains considerable firmness. The size of the trees is very considerable, and it remains yet to be proved whether they belong to the same family as the oaks now existing.

A German paper states that a descendant of the great Copernicus is living now at a small town of Posen, exercising the calling of shoemaker. It is known that Copernicus was a canon of the chapter of Frauenburg, and must be supposed to have died without leaving any issue. It has been said that his true son was Kepler, and that in his turn Kepler was the scientific father of Newton.

The additions to the Zoological Society's Gardens during the past week include an Entellus Monkey (*Semnopithecus entellus*) from India, presented by Mr. J. Mills, R.H.A.; two Prairie Marmots (*Cynomys ludovicianus*) from North America, presented by Miss Agneta B. Dykes; four Common Gulls (*Larus cana*), a Common Widgeon (*Mareca penelope*), four Grey Plovers (*Squatareola helvetica*), three Knots (*Tringa canutus*), a Dunlin (*Tringa cinclus*), European, presented by Mr. F. Cresswell; a Blue and Yellow Macaw (*Ara ararauna*) from South America, presented by F. G. J. Lillingston, Lieut. R.N.; two Coypu Rats (*Myopotamus coypu*), a Brown Coati (*Nasua nasica*), a Chilian Sea Eagle (*Geranoaëtus melanoleucus*), a Dinca Finch (*Dinca grisea*), two Saira Tanagers (*Pyranga saira*), two Dark Green Maize Eaters (*Pseudoleistes virescens*), two Blue-bearded Jays (*Cyanocorax cyanopogon*) from Buenos Ayres, two Garden's Night Herons (*Nycticorax gardeni*), an Ariel Toucan (*Ramphastos ariel*), a White-bellied Thrush (*Turdus albiventris*) from Bahia, a Great Frigate Bird (*Fregata aquila*) from Pernambuco, a Brazilian Blue Grosbeak (*Guiraca cyanea*) from Mexico, purchased; two Cuming's Octodons (*Octodon cumingi*) from Chili, deposited.

EARLY EXPERIMENTS ON THE CONDUCTION OF ELECTRICITY BY SUBMARINE WIRES FOR ILLUMINATING DISTANT PLACES AND PROPOSALS FOR THE DIVISION OF THE LIGHT INTO SEPARATE LIGHTS

I DO not profess to be acquainted with the means which have been recently employed for conveying electricity to illuminate places at a distance or for sub-dividing the electric light, nor is it with the slightest wish to derogate from the merit of recent inventors that I now submit a few facts as to earlier labours in the same field which may perhaps be interesting to the readers of Nature.

So far as I know, the first suggestion of communicating electricity for lighting purposes to distant places was in the fourth volume of the *Trans.* Roy. Scott. Soc. of Arts, vol. iv., 1854. In describing the apparent light on a sunken reef in the sea at the entrance of Stornoway Loch, which was lighted in 1851, I stated that "it occurred to me that in some cases gas-pipes might be laid or even *submarine wires, so as to illuminate a lantern placed on a beacon or buoy.*" I did not, however, consider it safe, "at least in the present state of our knowledge," to adopt either of these plans; but gave the preference to an apparent light illuminated by a beam of rays projected from a lens placed on the shore at a distance of 530 feet from the sunk rock, which plan has been in use since 1851.

In 1852, and therefore not long after the erection of the Stornoway light, Admiral Sheringham used electricity for producing *heat* for the purpose of igniting gas at a buoy.

My friend, Mr. Alan Brebner, C.E., suggested, as referred to in Messrs. Stevenson's Report on the electric light in 1865, that the lighthouses of Scotland might be illuminated from one great central station.

In 1865 I made experiments for the Commissioners of Northern Lighthouses with the sanction of the Board of Trade, on lighting beacons by submarine wires, and on the suggestion of my friend, Prof. Swan, increased the flashes by combining a Leyden jar with an induction coil. On January 13, 1866, I communicated to the Secretary of the Roy. Scott. Society of Arts that the induction spark placed in the focus of lighthouse apparatus gave in all respects satisfactory results at the distance of half a mile, which, owing to intervening objects, was the greatest distance from which it could be seen. The primary current was also kept for a week passing continuously night and day, through 800 feet of wire without any sensible waste of the platinum electrodes. I next attempted to pass the current through a cable under the sea, but without success, when Messrs. Stevenson applied to Dr. Siemens for his assistance in the matter, and he recommended an electro-magnet on the beacon with a contact lever actuated by the armature of the electro-magnet in the manner of a Neft's hammer. The luminous effect was increased by the deflagration of mercury. This plan, as tried at Granton Harbour, was quite successful; but the products of combustion were deposited on the optical apparatus, and some mechanical difficulties interfered with its continuous working.

Being thus thrown back on the old plan of the induction spark, I was enabled to overcome the difficulties by the following expedients:—Mr. Brebner suggested placing the induction coils with condensers close to the optical apparatus on the beacon and the battery and contact breaker on the shore, so as to pass only the primary current through the cable. Mr. Hart, electrician, also designed an improved break for the purpose, and Prof. Tait recommended the enlargement of the earth terminals. By these arrangements the current was passed successfully under the sea. The experiment was repeated at Granton, at the request of the Trinity House of London, in presence of Captains Fenwick and Nisbet, and Mr. Douglass, the engineer, accompanied by Mr. Farrer and Mr. Shaw Lefevre of the Board of Trade.[1] The distance between the battery and break on Granton Pier and the induction coils and optical apparatus on Newhaven Pier is

[1] "Proposals for the Illumination of Beacons and Buoys," by T. Stevenson, p. 14. (Edinburgh: A. and C. Black, 1870.)

upwards of half a mile, but the actual length of wire which was submerged, and through which the current passed, was upwards of a mile. The spark was about ¼ inch in length, bluish white in colour, and very striking and interesting in effect. It was placed in the focus of a holophotalised parabolic reflector.

At the same time in order to ascertain if by means of a single battery under the charge of one keeper a succession of flashes could be produced and a string of isolated dangers illuminated, the light was sub-divided first into two separate flashes and afterwards into six different sparks. The separate lights were quite satisfactory, though they were not as might have been expected of the same power as the original single one. But as the separate sparks were very close to each other this cannot, I presume, be held to be a proper sub-division of the light.

In 1867, at the British Association at Dundee, I suggested that the "effect of the light might be also increased without using additional cells if the same current could be again utilised so as to generate a second spark in the same focus. This was proposed to be done by 'using additional coils' for the same focus or separate sparks in the foci of separate reflectors.[1] I also added that the "time is perhaps not far distant when the beacons and buoys in such a navigation as the entrance to Liverpool may be lit up by submarine conduction from a central station on either shore, while the whole management may be trusted to the charge of one or two light-keepers."[2]

I may add that similar trials were made with Wilde's electromagnetic machine, which gave a light of much greater volume and power. The electrodes employed in all these experiments were made of platinum, but several other metals were experimented with, and of all that were tried bismuth was found to give the brightest light.[3]

A committee of the Scottish Society of Arts, consisting of Dr. Ferguson, convener, Dr. Lees, and the late Dr. Strethill Wright reported in the following terms:—"The peculiar character of the light, which is flickering, though continuous, is well marked and would be easily understood. So far as Mr. Stevenson's experiments go, they seem to prove the practicability of his proposal, and your committee do not anticipate any serious obstacle to its realisation." THOMAS STEVENSON

Edinburgh

UNDERGROUND TEMPERATURE[4]

DR. STAPFF has continued his observations of the temperature in the St. Gothard Tunnel, and has contributed to the Swiss Natural History Society a paper[5] of fifty-six quarto pages, embodying the results.

The following is his description (pp. 26, 27), of the mode of observing the temperature of the rocks in the tunnel:—

"The exact determination of the temperature of the rocks in the tunnel formerly occasioned a notable expenditure of time and money. At first thermometers about a metre long (made by J. Goldschmid, of Zurich) were employed for this purpose; their tubes being cemented into a wooden cylinder, so that only the bulb (surrounded by a perforated steel cap) projected below, and the scale (extending from 15° to 30° C.) above. Tallow was poured round the wooden cylinder, and the whole thermometer was then thrust into a bore-hole a metre deep, so that only the scale projected, from which readings were taken from time to time until the temperature became constant. The final reading had to be corrected not only for rise of zero but also for the temperature of the quicksilver in the thermometer tube which extends from the opening to the bottom of the bore-hole. Another very notable correction was required for the more or less oblique position of the thermometer; for the hydrostatic pressure of the quicksilver presses out the glass bulb so far that without change of temperature the long thermometer reads from 0°·4 to 1°·0 less in the vertical than in the horizontal position.

"After about from three to ten days, the reading of a thermometer luted into a bore-hole ceased to alter.

"Separate trials with thermometers of similar construction, but different length, showed, moreover, that, after months, the temperature of the rock at about a metre deep was still unchanged. This is obviously owing to the small difference of temperature between the rock and the surrounding air.

"From the observations at No. 8 and No. 15, in Table III., it is seen that the temperature at the bottom of the bore-hole was sometimes a little lower and sometimes a little higher than nearer its mouth.

"This mode of observing gave correct results, but was laborious and costly, not only on account of the necessity of making special bore-holes for the purpose, but because almost every experiment cost a thermometer. The projecting end was often maliciously broken off, and on account of the swelling of the wooden case it almost never happened that at the end of an experiment a thermometer was drawn out again uninjured.

"Hermann and Pfister remedied this latter evil by surrounding the thermometer-tube from the bulb to the scale, with a glass case, and this with a steel jacket. This arrangement, however, involves not only conduction through the steel, but also continual interchange of heat by currents of air in the glass case, from the mouth to the bottom of the hole. For these reasons the observations made with these thermometers could not be employed without intricate corrections.

"Later I tried a Thomson's maximum thermometer,[1] kindly placed at my disposal by Prof. Everett, which (after previous strong cooling) was left for several days at the bottom of the bore-hole, closed air-tight. The results agreed with those obtained by other methods; but who can guarantee that the higher temperature prevailing in a newly-bored hole is always just so much depressed by the cold mass of the thermometer and its copper case, that the rock-temperature alone determines the final indication of the maximum thermometer?

"This consideration induced me to employ for rock-temperature observations (and they also serve for air and water observations), the above-mentioned short thermometers with insulated bulbs, the first of which Prof. Everett caused to be made by Negretti and Zambra for this express purpose. These thermometers, inclosed in a metal box provided with a handle, are thrust to the bottom of the bore-hole, which is at least a metre deep. To the handle is fastened a strong cord reaching to the mouth of the hole, by which it can be drawn out again at the end of the trial. The bore-hole, from the thermometer to the mouth, is stopped with greased rag or other similar material, as air-tight as possible. After two or three days, the thermometers have usually assumed the temperature of the surrounding rock, that is to say, their reading has ceased to alter. The insulation of the quicksilver prevents alterations during the drawing-out and reading of the thermometer. The correctness of the result is in no way prejudiced by sediment from the boring which may yet remain in the hole. The pouring in of some water may even be useful in accelerating the experiment. Wet bore-holes with standing-water are, however, to be avoided, because rock-temperature and water-temperature are not identical.

"In the manner last described, at every available opportunity, that is to say, when the work of the tunnel is from any cause compelled to cease for a few days, rock-temperature observations are now instituted in bore-holes ready to our hand. The observations are simple, give exact results if taken with proper precaution and sufficient duration of the experiment, and cause no further expense, since the thermometers, being sunk in the rock, are secured against wanton injury, and there are always bore-holes available."

Dr. Stapff further states by letter that the two original thermometers supplied by Negretti and Zambra having been broken, he has had others made, in which he has introduced the improvement of hermetically sealing the outer glass case, instead of closing it with a waxed cork, which gradually admitted moisture.

In the Report for 1876 an account was given of the observations of Herr Dunker in a bore about 4,000 feet deep at Sperenberg, and allusion was made to the undue weight which had been attached by some writers to the empirical formula in which Herr Dunker sums up his observations; a formula which indicates a retarded rate of increase, and, if extended to greater depths, leads to the conclusion that the temperature reaches its maximum at the depth of about a mile.

A discussion has been carried on in Germany on this subject,[2]

[1] British Association Reports, 1867. [2] *Ibid.*

[3] "Proposals for the Illumination of Beacons and Buoys," pp. 14-15. (Edinburgh: A. and C. Black.)

[4] Eleventh Report of the British Association Underground Temperature Committee, by Prof. Everett.

[5] "Studien über die Wärmevertheilung im Gotthard," 1 Theil. "Der schweizerischen naturforschenden Gesellschaft zu ihrer sechzigsten Jahresversammlung in Bex gewidmet," von F. M. Stapff. Bern, 1877.

[1] It was one of the protected Negretti maximum thermometers constructed for the Committee.

[2] See papers by Mohr, Heinrich (two papers), Dunker, and Hottenroth, in the "Neues Jahrbuch" for 1875, 1876, and 1877; by Brauns, in the "Zeitschrift für die gesammten Naturwissenschaften," 1874, p. 483; and by Hann, in the "Zeitschrift der österreichischen Gesellschaft für Meteorologie," 1878, p. 17.

chiefly in the "Neues Jahrbuch für Mineralogie," &c., and the best authorities seem to be unanimous in rejecting the hypothesis of a retarded rate of increase in the earth's surface as unwarranted, either by the Sperenberg observations or any others. Herr Dunker himself concurs in this opinion. Dr. Stapff also, though some of his own empirical formulæ indicate a retarded rate of increase, writes to Prof. Everett in the following terms:—"As to my formulas, I beg you to remember that they are not constructed for expressing laws of Nature. They simply are made for facilitating the view over a heap of figures and data of observation. And generally I beg you to be sure that those formulas, in my mind, cannot express any law for the increase of warmth at greater depths than those in which the tunnel observations were made. The formulas give good means for eliminating empirically some of the influences of the shape of surface which occur in the profile of the mountain."

Mr. W. Galloway, one of H.M. Inspectors of Mines, has taken observations in Fowler's Colliery, Pontypridd, South Wales. The shaft is 846 feet deep, and the air current down it amounts to between 20,000 and 30,000 cubic feet per minute.

In order to determine the normal temperature of the coal, a hole $1\frac{1}{2}$ inch in diameter was bored in the side of a narrow place that was being rapidly driven in the solid coal. The hole was bored in the very face, to the depth of 4 feet. The thermometer (one of the committee's slow-action non-registering instruments) was placed at the inner end; then a wooden cylinder of nearly the same diameter as the bore-hole, and 9 inches long, was pushed in until it came in contact with the copper case of the thermometer; and lastly a wooden plug, wrapped round with cloth, was driven firmly into the mouth of the hole. The thermometer was at 58° F. when it was put into the hole, and after remaining there from 2 P.M. on August 25, 1876, to 3.45 P.M. on the following day, it stood at 62°·7. There was no water whatever in the hole, and the depth below the surface of the ground was 855 feet.

The circumstances of this observation seem to preclude any considerable disturbance of the normal temperature; and combining it with the mean annual temperature at the surface, which is said to be 51°·5, we have an increase of 11°·2 F. in 855 feet; which is at the rate of 1° F. for 76 feet.

Two other observations were taken in other parts of the mine. They are not directly available for the purposes of the Committee, but were intended to test the influence of air-currents on the temperature of the coal; and they show variations of 2° or 3°, according to the season of the year.

Observations are being taken for the Committee by Mr. G. F. Deacon, Borough Engineer of Liverpool, in a bore which has attained the depth of 1,004 feet, in connection with the Liverpool Waterworks at Bootle.

The temperature at this depth is 58°·1. The observation nearest the surface was at the depth of 226 feet, the temperature at this depth being 52°. We have here a difference of 6°·1. in 778 feet, which is at the rate of 1° for 128 feet, and the same rate is approximately maintained throughout the descent. For instance, at 750 feet, the temperature was 56°, which gives 1° for 131 feet by comparison with the depth of 226 feet, and 1° for 121 feet by comparison with the bottom.

The bore is 24 inches in diameter, and the observations were taken with a protected Phillips's maximum thermometer every Monday morning. The operation of boring was continued up to twelve o'clock on Saturday night, and was not resumed till the temperature had been taken on the following Monday. The time that the thermometer remained at the bottom was not less than a quarter of an hour, and was sometimes half an hour.

The rock-formation consists of the pebble beds of the Bunter or lower trias, and most of it is described as hard, close-grained, and compact. The speed of boring is indicated by the dates of the observations at 226 and 1,004 feet, the former being November 12, 1877, and the latter August 12, 1878. A month was lost by the jamming of the drilling tool, in May and June, 1878, when a depth of about 890 feet had been attained.

The depth from the surface of the ground to the surface of the water in the bore has gradually decreased from 66 feet, when the bore was at 318 feet, to 52 feet, when the bore was at 800 feet, and to 51·1 feet, at the present depth. It would thus appear that the inflow of water from below has increased with the depth attained. There is a slow percolation from the upper part of the water-column to an underground reservoir near at hand, the top of the water-column being considerably higher than the top of the water in the reservoir. Mr. Deacon remarks that the slow upward flow which supplies the water for this gradual discharge is favourable to the accuracy of the observations (which have always been taken at the bottom), by checking the tendency of the colder and heavier upper water to descend and mix with the lower. As bearing on the subject of the disturbance of temperature by the stirring of the water in boring, as well as by the generation of heat in the concussions of the tool, it may be mentioned that the last observation before the month's interruption by the jamming of the tool was 57°·5, at 886 feet, and the first observation after the extraction of the tool was 57°·0, at 898·6 feet, the former being on May 20, and the latter on July 1. The smallness of the difference between these two temperatures seems to indicate smallness of disturbance by the action of the tool.

It appears from these various circumstances that the observations are entitled to considerable weight, and that the rate of increase of temperature downwards at Liverpool is exceptionally slow. It will be remembered that the rate found by Mr. Fairbairn, at Dukinfield Colliery, in the adjacent county (Cheshire), was also very slow, though not nearly so slow as that indicated by these Liverpool observations.—(See our Report in the volume for 1870).

Mr. E. Wethered, of Weston, near Bath, has also commenced observations in a colliery in that neighbourhood. Mr. J. Merrivale, of Nedderton, near Morpeth, has received a thermometer for observations in a colliery. Mr. J. T. Boot, of Hucknall, near Mansfield, has received a second thermometer (in place of a broken one) for observations in a deep bore, and Mr. Rowland Gascoigne, of the same town, has received one for a similar purpose.

In the eleven years which have elapsed since the appointment of this Committee a large amount of useful work has been done, by methods of observation not requiring any elaborate or expensive appliances, or any special training on the part of the observers.

Two difficulties are encountered in investigating underground temperature. We have to contrive instruments which shall truly indicate the temperature at the point of observation, and we have further to insure that this temperature shall be the same at the time of observation as it was before the locality was artificially disturbed.

As regards the first of these difficulties the Committee have been completely successful, and have largely increased the resources at the command of observers.

But in regard to the second difficulty the same amount of success has not been attained. The circulation of water in bore-holes and of air in mines are disturbing elements difficult to deal with. Even such firm plugging as was employed to isolate portions of the water-column in the great bore at Sperenberg cannot altogether remove the error arising from convective disturbance; for the long-continued presence of water at a temperature different from that proper to the depth affects the temperature of the surrounding rocks, and the temporary isolation of a short column would not abolish this source of error, even if the plugs themselves were impervious to conduction and convection.

After the experience which has now been gained of rough and ready methods, it is time to consider the propriety of resorting to a more special method, which has been more than once suggested, but has hitherto been postponed on account of the additional labour and skill which would be requisite for carrying it out.

There can be no doubt that the surest way to bring any point of a boring to its original temperature is to fill up the bore, and reduce it as nearly as possible to its original condition. Several instruments have been contrived which, when buried in the earth, with wires coming from them to the surface, admit of having their temperature observed by electrical means.

One of these is Siemens' resistance thermometer, another is Wheatstone's telegraphic thermometer, of which a description will be found in the Report of the Dundee Meeting of the British Association; another is Becquerel's thermo-electric apparatus, which has been employed by its inventor and his son and grandson for some forty years. It is described in the following terms in the first Report of this Committee (1868):—

"The thermo-electric method might also be followed with great advantage. Two wires, one of iron and the other of copper, insulated by gutta-percha or some other covering, as in submarine cables, and connected at their ends, might be let down so as to bring their lower junction to the point where the temperature is to be taken, their upper junction being immersed

in a basin of water, and the circuit completed through a galvanometer. The temperature of the water in the basin might then be altered till the galvanometer gave zero indication."

Sir Wm. Thomson now adds the recommendation that, in carrying out this method, the two wires, each well covered with gutta-percha, should be twisted together; that the wires should be stout and as homogeneous as possible throughout, and that a piece of stout copper tube should be attached to the lower junction, this tube being uncovered and in close contact with the earth all round, its purpose being to insure that the junction takes the proper temperature.

It would probably be desirable, in filling up the bore, to mix clay with the original material to render it watertight, for it would be impossible to render the filling of the bore as compact as the surrounding rock.

Several pairs of wires would be buried in the same bore, with their lower junctions at different carefully-measured depths.

The upper junctions would be kept in a room provided with a steady table for a mirror-galvanometer.

THE RAINFALL OF THE WORLD[1]

1. THE pamphlet referred to below embodies the outline of an attempt to bring into harmony the disconnected, and in some cases apparently irreconcilable results that have hitherto attended comparisons of terrestrial rainfall and sun-spot variations. It relates, therefore, to the entire rainfall system of the globe.

2. The plan by which it is thought this object will be best attained is one which divides the world into a number of rainfall zones where either *à priori* considerations or actual experience would lead us to expect typical changes in the effects of a recurring secular variation in solar radiated heat upon the rainfall; it being immaterial as far as regards the practical advantages secured by this method of hyeto-graphical subdivision, whether the solar radiation be ultimately found to vary directly or inversely with the sun-spots.

3. The way in which typical changes may arise in different parts of the earth from the effects of an assumed recurring secular change in solar radiated heat, is shown by a reference to the general scheme of atmospheric circulation in conjunction with the two leading factors of variability, viz., season and latitude.

4. A consideration of these points leads the author to divide the world into five zones, which either theoretically might, or are actually known to, involve some typical change in the secular variation of the rainfall either of one season or the whole year.

5. Partly to illustrate this mode of subdivision by applying a reasonable working hypothesis, and partly in the absence of absolutely conclusive evidence in its favour, by exhibiting the harmony of existing facts with the conditions theoretically deduced from it, to promote its ultimate adoption, the theory of the inverse variation of solar radiated heat with the sun-spots is assumed throughout.

6. It is also shown in the Introduction that we have a good deal of evidence in favour of the same theory, both *à priori*, from a consideration of the principle of conservation of energy as applied to the sun, as well as indirect, from the results of thermometrical observations.

7. In applying this hypothesis to determine the rainfall variation, account is mainly taken of the direct relation between wind velocity and temperature, the secular changes in solar radiation being assumed to cause *similar* effective secular changes in the velocity of the larger atmospheric convection currents.

8. An induction from Messrs. Blanford and Eliot's theory of cyclone-generation is then made use of, in combination with the preceding hypothesis, from which it appears that while, owing to the diminished solar temperature, evaporation might be lessened in the tropics at the epoch of maximum sun-spot, the diminished carrying power of the wind (by which the prevalence of cyclones at this epoch would be accounted for, according to Blanford and Eliot's theory) might allow of greater precipitation near the place of evaporation, and therefore of a generally heavier rainfall in these regions. At the opposite epoch, on the other hand, the increased velocity of the wind would probably cause a wider distribution of tropical vapour, and therefore in combination with the direct effects of the assumed increase in solar radiation at the same epoch give rise to a deficiency of rain in parts, more especially those in which the local conditions normally tend to produce aridity.

9. These hypothetical results are then shown to approximately agree with the actual results of observations recorded in these regions.

10. It is next shown that the effects of the assumed secular change in the velocity of the anti-trade (the prevailing wind of the temperate zone) should differ considerably from those in the case of the monsoons and trades of the tropics, an increased velocity in the case of the anti-trade causing a greater quantity of tropical vapour to be conveyed to the temperate regions, and consequently a greater degree of humidity to ensue there. When, therefore, the direct effects of the assumed increase of solar heat at such an epoch are at a minimum, that is to say, in the *winter*, the relative humidity, and consequently the rainfall, should be *increased*. It is also evident that such an effect should be most conspicuously felt in those regions where rain falls *only* in the winter, and is due to the descent of the anti-trade.

11. The occurrence of this inverse variation in the zone of winter rains, which in the case of the Mediterranean stations (*Zeitschrift für Meteorologie*, Band viii. No. 6), had hitherto been deemed unfavourable to Messrs. Lockyer and Meldrum's generalisation regarding the direct variation of terrestrial rainfall with the sun-spots, is also shown to be visible in the winter rainfall of Northern India, and the rainfalls of Jerusalem and California, thereby affording some preliminary support to the notion that it holds over a still wider extent of the globe where the rain falls mostly during the winter.

12. The attempt is then made to show that while the direct effects of the secular change in the sun's heat over extra-tropical continents may, during the summer, operate so far as to destroy the indirect effects produced by the corresponding variations in the strength of the anti-trade, and as Dr. Hahn has shown in the case of the summer rainfalls of several stations in Central Europe, actually cause a *direct* variation with the sun-spots, there are, as there should be, in accordance with the hypothesis, some preliminary indications of an *inverse* variation of that proportion of the total which falls during the winter months alone, even in those places where the rain falls throughout the year. This fact, then, would imply that a change of season causes a change of type in the character of the variation, so that in order to render the variations distinctly apparent we should compare the winter and summer falls separately. It may also be inferred that the quality of the variation in the total annual fall will depend on the preponderance of the summer or winter falls respectively, which fact may help to account for the numerous anomalies noticed by those who have hitherto compared the total annual falls of places in the temperate zone with sun-spots.

13. It is finally inferred in the appendix, as a direct result of the hypothesis assumed throughout, that the winter gales of the temperate zone and the cyclones of the tropics should bear a complementary relation to each other, the former being most frequent about the time of minimum, and the latter about that of maximum, sun-spot. Some evidence in favour of this notion was recently communicated to NATURE by Mr. S. A. Hill (vol. xviii. p. 616).

14. The pamphlet is intended by the author to be considered as merely tentative, and not by any means conclusive. It is the method of division into zones and the separate comparison of seasonal falls, rather than the accordance of data with theoretical deductions, to which he desires to give prominence, and which he thinks may be of some assistance to other workers in the same field.

[1] The Rainfall of the World in Connection with the Eleven-Year Period of Sun-spots. With an Introduction and Appendix. By E. D. Archibald, Professor of Mathematics in the Patna College. (Calcutta and London: Thacker and Co. 1878.)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Cambridge Mathematical Tripos list was published on the 24th. This year the list contains 91 names. There are 28 classed as Wranglers, 33 as Senior Optimes, 29 as Junior Optimes, and 1 Ægrotat. In 1878 the list contained 94 names, 31 being Wranglers, 30 Senior Optimes, 29 Junior Optimes, and 4 Ægrotant. The first three Wranglers are Mr. A. J. Campbell Allen, of St. Peter's, Mr. George Walker, of Queen's, and Mr. Carl Pearson, King's. Mr. Campbell Allen, of St. Peter's College, the Senior Wrangler, is a native of Belfast, and was born in 1856. He received his elementary education at the

Royal Academical Institution, Belfast. In 1872 he became a student at Queen's College, Belfast, where he succeeded in winning several scholarships and also two Peel Exhibitions, one for general proficiency and the second for mathematics. In 1875 he was elected to an open scholarship at St. Peter's, and in October of that year he graduated B.A. at the Queen's University, subsequently proceeding M.A. On each occasion he obtained a first-class for mathematical science, and was awarded a gold medal. He has won several college prizes during his residence at Cambridge. Mr. Walker is a native of Durham, and was educated at Durham University, of which he is a Fellow, and proceeded to Queen's College in October, 1875. He has been a prizeman of the college for mathematics. Mr. Pearson was educated at University College School, and also under private tuition with the Rev. L. Hensley, of Hitchin. He gained an open scholarship at King's College in 1875, and has been each year college prizeman in mathematics.

A REPORT just published by the Swiss Statistical Board gives some information as to the state of primary instruction in the various cantons of Switzerland. Out of 21,875 recruits examined during the year 1877, 11·7 per cent. proved to have primary instruction quite insufficient, and were sent back to the primary military schools. The better educated cantons are those in which manufactures are more developed, namely, Basel (town), Geneva, and Zurich, Schaffhausen and Thurgau. The worst educated are those of Appenzell (land), Uri, Wallis, and Freiburg (Catholic). Primary education seems to have become worse during recent years, as the results for 1877 are far below those of 1876.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, December, 1878.—From experiments here described by Mr. Jacques, it appears that currents of air of varying density, as in Tyndall's well-known experiment, not only diminish the *intensity* of a sound, but affect its *distinctness*. This holds good especially for the human voice, and for musical instruments with few overtones (as the flute). The effect on the voice is that of a repetition of each syllable several times in close succession. Sound-waves were traced out in the space of an auditorium in Boston, and their confusion shown on introducing air-currents. The good acoustic properties of the Baltimore Academy of Music are proved to be due to arrangements by which a large volume of air is conducted, in gentle current, across the stage and diagonally towards the roof. When, by closing certain valves, ventilation was arrested and currents of circulation generated, the sound was noticed to be "dead," or "confused and indistinct."—Dr. Dudley investigates the chemical composition and physical properties of steel rails, deducing some rules for guidance of the Pennsylvania Railroad Company.—Mr. Dupuy writes on the direct process of making wrought iron and steel,—Mr. Dumont on tests of boiler iron,—and Prof. Haupt on the use of the heliotrope in geodetic surveys.

THE *Archives des Sciences physiques et naturelles* (parts 251 and 252, November and December) contain the following papers of interest:—On ytterbina, a new earth contained in gadolinite, by C. Marignac.—On a transformation of dibromethylene into an acetone with four atoms of carbon, brought about by the action of hypobromous acid, by E. Demole.—A note on Dr. Heine's work on the formation of mountains, by E. Renevier.—On the geography and archæology of forests, by Dr. Asa Gray.—Recent researches in solar chemistry, by J. Norman Lockyer.—Observation of a case of migration of carps, by A. Bartholoni.—On a general method of continuous integration of any numeric function, applied to several theorems furnished by the mathematical analysis of the calculation of the curves of a new thermograph, by Raoul Pictet and Gustave Cellérier.—On the limnograph of Secheron, near Geneva, by Ph. Plantamour.—A note on the useful effect of magneto-electric machines and the production of electric light, by A. Achard.—On the reappearance of Encke's comet of short period, with a history of this comet, by Alfred Gautier.—Some remarks on the migration of carps by G. Lunel.—On the ophite of Spain, by M. Calderon.

Bulletin de l'Académie Royale de Belgique, Nos. 9 and 10, 1878.—This contains an account, by M. Dupont, of a recent important "find" of fossils in the Sainte Barbe, one of the coal mines of Bernissart (a village near the French frontier), consisting of five skeletons of large adult iguanodons together with tortoises, numerous fishes, and plant-impressions, constituting a fauna and flora wholly new for the country. The bones are unfortunately impregnated with pyrites, so that they are readily disaggregated on contact with air, but they have been carefully removed in plaster to Brussels, after precise noting of position, &c. The fossils were found at several different levels separated by layers of sterile clay. There is no indication of molluscs of any kind. The deposit is thought to be of the Wealdian horizon, and is remarkable, both in itself, and in its relations to the subterranean orography of the valley of Mons, and the lower cretaceous strata of Hainaut.—M. Plateau writes on a law of the persistence of impressions in the eye. With two disc having the same number of sectors, and the white sectors of the one being equal in angular width to the black discs of the other, the "times of apparent constancy," of the two impressions are to each other in inverse ratio of the brightnesses of the two grey tints producing these impressions. A complete impression, whether intense or weak, has no appreciable time of apparent constancy; and the time is longer, the more incomplete the impression. The degree of illumination of the object has but a weak and indirect influence on the time of apparent constancy.—M. Longchamps contributes further additions to the synopsis of the Gomphines; and M. Renard lithological researches on the phtanites of the carboniferous limestone of Belgium.—M. Montigny describes an experimental arrangement for the study of coloured stars.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 19, 1878.—"On the Torsional Strain which remains in a Glass Fibre after Release from Twisting Stress," by J. Hopkinson, D.Sc., F.R.S.

It has long been known that if a wire of metal or fibre of glass be for a time twisted, and be then released, it will not at once return to its initial position, but will exhibit a gradually decreasing torsion in the direction of the impressed twist. The best method of approximating to an expression of the facts has been given by Boltzmann ("Akad. der Wissensch. zu Wien," 1874). He rests his theory upon the assumption that a stress acting for a short time will leave after it has ceased a strain which decreases in amount as time elapses, and that the principle of superposition is applicable to these strains, that is to say, that we may add the after-effects of stresses, whether simultaneous or successive. Boltzmann also finds that, if $\phi(t)\tau$ be the strain at time t resulting from a twist lasting a very short time τ, at time $t = 0$, $\phi(t) = \frac{A}{t}$, where A is constant for moderate values of t, but decreases when t is very large or very small.

The glass fibre I examined was about twenty inches in length. The glass from which it was drawn was composed of silica, soda, and lime; in fact, was glass No. 1 of my paper on "Residual Change of the Leyden Jar" (*Phil. Trans.*, 1877). In all cases the twist given was one complete revolution. The deflection at any time was determined by the position on a scale of the image of a wire before a lamp, formed by reflection from a light concave mirror, as in Sir W. Thomson's galvanometers and quadrant electrometer.

The first point to be ascertained from the results was whether or not the principle of superposition, assumed by Boltzmann, holds for torsions of the magnitude used.

The experiments indicate a large deviation from the principle of superposition, the actual effect being *less* than the sum of the separate effects of the periods of stress into which the actual period may be broken up.

They also appear to indicate the form $\phi(t) = \frac{A}{t^{\alpha}}$, α being less than, but near to, unity. If $\alpha = 0·95$ we have a fairly satisfactory formula for the case in which the fibre was twisted two hours.

In the author's paper on "Residual Change of the Leyden Jar" that subject is discussed in the same manner as Boltzmann discusses the after-effect of torsion on a fibre, and it is worth remarking that those results can be roughly expressed by a formula in which $\phi(t) = \frac{A}{t^{\alpha}}$. For glass No. 5 (soft crown) $\alpha = 0·65$, whilst for No. 7 (light flint) it is greater: but in the electrical experiment no sign of a definite deviation from the law of superposition was detected.

January 16.—"On the Effect of Strong Induction-Currents upon the Structure of the Spinal Cord," by William Miller Ord, M.D.

Conclusions.—1. That, in young dogs, the protoplasmic constituent of the grey matter contracts under the influence of strong faradaic currents.

2. That it contracts unequally and irregularly by reason of its unequal and irregular sectional area, causing thereby condensations at certain points—notably in the anterior horns and around the central canal—and rarefaction at others—notably in the middle of each crescent, such rarefaction going on sometimes to rupture of tissues.

3. That nerve-corpuscles contract in various degrees according to the strength and duration of currents, and that while they tend in contraction to become spherical, they also tend to become vacuolated.

4. That the vessels are in some places strongly contracted and empty; in others dilated and filled with blood-clot, having the appearance of embolus.

5. That the appearances correspond so decidedly with appearances in chorea and tetanus as to give ground for the supposition that contractions, such as are produced by electricity, do actually occur during life under the effect of nervous shock, and may be phenomena, casual, or associate, of such diseases.

"On some Points connected with the Anatomy of the Skin," by George Thin, M.D. Communicated by Prof. Huxley, Sec. R.S.

It is partly the object of this paper to describe some methods by which it can be demonstrated that the connective tissue-bundles of the cutis are, as has been long ago pointed out by Rollett, composed of subdivisions, which are again composed of minute fibrillæ. These subdivisions the author terms primary bundles to distinguish them more markedly from the fibrillæ, and also to describe some other points in the anatomy of the skin which were observed by means of these methods.

The primary bundles isolated by these methods were flattened, cylindrical elements, even contoured, homogeneous in appearance, and uniform in breadth over the whole length isolated. The difference in breadth between individual bundles was very slight. By measurement he found that they were from 0·004 to 0·005 millim. broad.

In gold preparations the following facts regarding the disposition of the elastic fibres were noted:—

If a portion of skin is hardened in bichromate of potash, and the sections moderately stained by eosin, all the large elastic fibres are stained much more intensely than the bundles, and it is then observed that they lie on the surface of the bundles, and run parallel to them. In the gold preparations, after maceration in formic acid, further details regarding the fibres can be detected. It is then seen that there is a close network of minute elastic fibres, of which no traces are observed in eosin-stained bichromate preparations, on the surface of the bundles, and that at certain points the larger fibres give off branches which join this network. At these points the network is so dense over a small defined space that the size of the meshes is nearly equalled by that of the fibres.

Elastic fibres which penetrate the bundles enter between the primary bundles, and the primary bundles are embraced by the fibres which entwine them very closely.

The dark very finely granular deposit produced by the reduction of the gold chloride had a special relation to the elastic fibres. Strictly defined narrow strips of this deposit were found investing the fibres, and this so closely that it was only at points where it had been disturbed in the preparation that the fibre itself could be observed.

The distinctly localised character of the deposit around the elastic fibres supports, according to the author, the idea that the larger ones are surrounded by an albuminous fluid, of a like nature to that shown by gold preparations, to be present between the laminæ of the cornea.

The "spiral" fibre, as observed on the bundles of the skin, is an elastic fibre that encircles the bundles like a ring, and is stained yellow by pikro-carminate of ammonia.

The cells seen in the preparations were in two positions. Some of them were found in a delicate tissue between the bundles; other cells were found in direct connection with the bundles. Of the latter cells the greater number seen were applied to the surface of the bundles, but others were found in the substance of the bundles between the primary bundles.

These cells were all of the endothelial type. In all of them the cell-contour was clearly marked, and in none of those observed was there a trace of a process, or of ridges and depressions similar to those described by some histologists in tendon.

"On Hyaline Cartilage and Deceptive Appearances produced by Reagents, as observed in the Examination of a Cartilaginous Tumour of the Lower Jaw." By George Thin, M.D. Communicated by Prof. Huxley, Sec. R.S.

This paper is written with a twofold object: firstly, as a contribution to the histology of hyaline cartilage; secondly, to illustrate how much the apparent structure of a tissue which is being examined microscopically depends on methods of preparation.

The author was able to isolate the cells from the cartilaginous substance of the tumour after the action of osmic acid. All the cells thus isolated were flattened, rounded, or somewhat polygonal bodies, with round nuclei. Their contours did not correspond exactly with those of the rounded cartilage "capsules."

The examination of this tumour showed that most delusive appearances as regards the nature of cartilage cells may be sometimes produced by staining and hardening agents. Carmine and eosin by staining an unformed substance that exists in the structure in a localised form, may simulate branched protoplasmic cells, and bichromate and logwood preparations, either in sections or teased out, may as closely simulate cells with fibre processes.

The facts adduced by the author justify, as he believes, serious doubts as to the correctness of interpretation in all cases in which histologists have described branched cells in hyaline cartilage, whether the latter existed as a normal structure or as a pathological growth. They further show that, taken alone, carmine or eosin staining should not be held as conclusive evidence of the existence or limits of cellular protoplasm in any animal tissue.

Meteorological Society, January 15.—Annual Meeting.—Mr. C. Greaves, president, in the chair.—The Report of the Council showed that the chief features of the proceedings during the year 1878 had been the final completion, on a comprehensive and well-organised basis, of the arrangements for systematic inspection of the Society's stations, an object which has engaged the sedulous attention of successive Councils for the last four years; and the delivery of a series of lectures on Meteorology by certain Members of the Council. The total number of Fellows now amounts to 425, forty-one having been elected during the year.—The President having delivered his address on Dryness *versus* Humidity, the following gentlemen were elected officers and Council for the ensuing year:—President: Charles Greaves, M. Inst. C.E., F.G.S. Vice-Presidents: Charles Brooke, M.A., F.R.S., F.R.C.S., Henry Storks Eaton, M.A., Rev. William Clement Ley, M.A., Capt. Henry Toynbee, F.R.A.S. Treasurer: Henry Perigal, F.R.A.S. Trustees: Sir Antonio Brady, F.G.S., Stephen William Silver, F.R.G.S. Secretaries: George James Symons, F.R.S., John W. Tripe, M.D. Foreign Secretary: Robert H. Scott, M.A., F.R.S. Council: Arthur Brewin, F.R.A.S., Edward Ernest Dymond, William Ellis, F.R.A.S., Rogers Field, B.A., M.Inst.C.E., Rev. Charles Higman Griffith, William John Harris, M.R.C.S., James Park Harrison, M.A., John Knox Laughton, M.A., F.R.A.S., Robert John Lecky, F.R.A.S., Hon. Francis A. Rollo Russell, Richard Strachan, Henry Samuel Tabor.

Royal Microscopical Society, January 8.—J. W. Stephenson, treasurer, in the chair.—Five gentlemen were proposed for election as Fellows, and the list of nominations for the Council was read.—The following papers were read:—Observations on *Dactylocalyx pumiceus* (Stuchbury), with description of a new variety, *D. Stuchburyi*, by Mr. W. J. Sollas.—Note on a revolver immersion prism for sub-stage illumination, by Dr. James Edmunds.—Immersion illuminators, by Mr. J. Mayall, jun.—Is not the genus Pedalion of Hudson synonymous with Hexarthra of Schmarda? by Mr. J. Deby.—The thallus of Diatoms, by Mr. F. Kitton.—Mr. Crisp (secretary) described the two new sense-organs in insects discovered by Prof. Graber, of Czernowitz.—The following were exhibited:—Specimens showing parasitism of a coral on a sponge (Dr. Matthews); the Sorby miniature micro-spectroscope; Recklinghausen and Meyer's pathological micro-photographs and specimens of microscopic printing (Mr. Crisp); sections of mistletoe on an apple tree double stained (Mr. Ward); Amici's original form of camera lucida referred to at the December meeting (Mr. Ingpen).

Entomological Society, January 15.—Anniversary meeting. H. W. Bates, F.L.S., F.Z.S., president, in the chair.—The following gentlemen were elected Members of the Council for the ensuing year, viz.:—H. W. Bates, F.L.S., F.Z.S., W. L. Distant, Rev. A. E. Eaton, M.A., E. A. Fitch, Ferd. Grut, F.L.S.,

R. Meldola, F.C.S., Edw. Saunders, F.L.S., J. Jenner Weir, F.L.S., J. W. Dunning, M.A., F.L.S., Sir Jno. Lubbock, Bart., V.P.R.S., Saml. Stevens, J. Wood Mason, F.G.S. The following officers were elected:—President: Sir Jno. Lubbock, Bart., V.P.R.S. Treasurer: J. Jenner Weir. Librarian: F. Grut. Secretaries: R. Meldola and W. L. Distant. The retiring president delivered an address which was immediately ordered to be printed.

WELLINGTON, N. Z.

Philosophical Society, November 9, 1878.—Mr. Carruthers, vice-president, in the chair.—Further contributions to the ornithology of New Zealand, by Dr. Buller, C.M.G. This paper consisted partly of technical matters and partly of observations on the habits and life-economy of a number of the more common species of native birds. The author gave the results of his examination of the group of *Platycerci* in the British Museum, and showed that many of the so-called species had no real existence, the same bird having been described under different names by different naturalists. He gave his reasons for considering *Platycercus Rowleyi*, described by himself from specimens in the Canterbury Museum, a good and valid species. He disputed Mr. Sharp's generic substitution of *Harpa* for *Hieracidea* in the British Museum catalogue of Accipitres, and his reduction of *H. ferox* to the rank of a "sub-species," as being unintelligible; either the two forms of sparrow-hawk represent distinct species, as the author and others believe; or they are one and the same, as contended for by Captain Hutton in the controversy which took place some time ago. Referring to that discussion and to Captain Hutton's emphatic denial that the New Zealand kingfisher ever caught fish, he proceeded to give some further facts in support of his own view to the contrary. In treating of the kaka (*Nestor meridionalis*) he mentioned the singular circumstance that at a certain season of the year, when these birds are migrating across the Strait at its widest part, numbers of them, owing to their fat condition, succumb to fatigue, and are washed up in Golden Bay and on the spit beyond, the set of the current being in that direction. The paper contained many other interesting notes, and a full account of the capture and subsequent history of a specimen of the plundering gull (*Stercorarius antarcticus*) still living in the author's garden.

PARIS

Academy of Sciences, January 20.—M. Daubrée in the chair.—The following papers were read:—On the development of the perturbative function in the case where, the eccentricities being small, the mutual inclination of the orbits is considerable, by M. Tisserand.—Observations on the second reply of M. Pasteur.—Reply by M. Pasteur, &c.—On the special apparatus of nutrition of phanerogamous parasite species, by M. Chatin. He distinguishes in the sucker a *cone de renforcement* (the central, mostly solid part), and a *cone perforant*, or parenchymatous cone continuing the other, and capable, notwithstanding its delicacy of tissue, of progressing through the hardest woods. (There are variations from this in some cases.) The suckers of parasites show great analogies to ordinary roots of plants.—On the temporary magnetic properties developed by induction in different specimens of nickel and cobalt, compared with those of iron, by M. Becquerel. The ratio of the temporary magnetic effects developed at ordinary temperature, by increasing magnetic inductions, in any of the nickel bars and in a bar of soft iron of the same length, weight, and section, is a number variable with the magnetic intensity to which the metals are submitted. This ratio, for very small intensities, first decreases, passes a minimum, then increases to a maximum, and lastly decreases to an inferior limit. Carburetted and forged nickels show the variations most. Pure cast or porous bars of nickel give results very like those of soft iron. Cobalt behaves similarly to nickel. The variation of the ratio considered is due to unequal saturation of the two metals.—On linear differential equations of the third order, by M. Laguerre.—On the classification of colours, and on the means of reproducing coloured appearances by three special photographic negatives, by M. Cros. Under the word colours he distinguishes lights and pigments. To get immediately the elementary tints of lights and pigments, look through a prism at a white bar on a dark ground, and a black bar on a white ground; in the first case you see a spectrum orange, green, violet; in the latter a spectrum red, yellow, blue. In the one case the orange, green, and violet are elementary lights; in the other, the blue, red, and yellow, are lights combined two and two. This he demonstrates with an apparatus he calls a *chromometer* (which distinguishes the colours by numerical data); and he makes this act on the positions from three negatives obtained through green, violet, and orange screens, ultimately reproducing coloured appearances.—Researches on the effects of induction through telephonic circuits, by means of the microphone and the telephone, by Prof. Hughes. A battery of three Daniells, a microphone, an inducing spiral, and a clock, were put in one circuit; another helix (to receive induction) and a telephone in another circuit. The sounds were still heard in the induced circuit when the spirals (containing 100 m. wire) were 30 ctm. apart. Conducting plates interposed weakened the effect, and spirals with closed circuit better. Flat helices gave more intense reproduction than long ones. Putting a telephone bobbin, in circuit with a microphone, to one ear, and the bobbinless telephone to the other, one can hear thus; and the arrangement is a sort of electric analyser revealing what passes in organs traversed by currents. (Other experiments are given.)—New voltaic element with constant current, by M. Heraud. The exciting liquid is chlorhydrate of ammonia, the depolarising body protochloride of mercury, or calomel. The former, in presence of zinc, gives chloride of zinc with ammonia and hydrogen. The hydrogen reduces the protochloride, giving metallic mercury, chlorhydric acid, and consequently, chlorhydrate of ammonia. To prevent deposition of ammoniacal oxychloride of zinc on the zinc, the solution of sal ammoniac used is diluted one-tenth with liquid ammonia. The zinc is suspended by a coated copper-plate about the middle of the liquid. The positive electrode is carbon in a canvas bag. One element, after 248 days' use, retained 0·66 of its original intensity.—On tetric acid and its homologues, by M. Demarçay. —Researches on the development of eggs and of the ovary in mammalia, after birth, by M. Rouget.—Description of the strata forming the ground in the department of Meurthe-et-Moselle, by M. Braconnier.

CONTENTS

THURSDAY, FEBRUARY 6, 1872

ENTOMOLOGY IN AMERICA

First Annual Report of the United States' Entomological Commission. (Washington: Government Printing Office, 1878.)

WE have received the first annual report of the United States Entomological Commission for the year 1877, "relating to the Rocky Mountain locust, and the best means of preventing its injuries and of guarding against its invasions." The title-page bears the heading, "Department of the Interior, United States Geological Survey, F. V. Hayden, U.S. Geologist in charge;" but the report is by C. V. Riley, A. S. Packard, Jun., and Cyrus Thomas, and it simply passes, *pro formâ*, through Dr. Hayden's hands to the Secretary of the Interior. The amount of entomological work previously done by the Survey is well known.[1] Before speaking of this report it may be well to refer to the circumstances which have led to its being made. During the years 1873–76 the injury done by the Rocky Mountain locust in states west of the Mississippi was so great as to create a feeling that steps should be taken by Congress towards mitigating the evil. A conference of the governors of various western states and territories was held at Omaha (Nebraska) in October, 1876, the result of which was a memorial to Congress for an appropriation of 25,000 dols. and the creation of a commission of five experts to thoroughly investigate the subject. Congress acceded partially to this and an act was passed "appropriating 18,000 dols. to pay the expenses of three skilled entomologists to be attached to Dr. F. V. Hayden's United States Geological and Geographical Survey of the Territories," and the Secretary of the Interior appointed the gentlemen above-named. As soon as the commissioners were appointed they met at Washington and agreed upon the division of labour. Their scheme was laid before the Minister of the Interior on March 22, 1877, was approved, and was immediately put in operation. Within eleven months from that date the report was prepared and ready for presentation. When it is considered how vast was the district to be examined, how numerous were the records of the movements of locusts during the year that were collected, and what a range of subjects the report includes, it seems astonishing that so much should have been accomplished in the time. The locust-area was divided into three districts for convenience. Mr. C. V. Riley took the region east of the Rocky Mountains and south of the 40° N., the western half of Iowa, and conjointly with Mr. Packard, British America west of 94° W. Mr. A. S. Packard took Western Wyoming, Montana, Utah, Idaho, and the Pacific coast. Mr. Cyrus Thomas took the portion north of Mr. Riley's region, including the eastern half of Wyoming, Northern Colorado, the southern and eastern part of Dakota, Nebraska, the eastern half of Iowa and Minnesota. Circulars asking for information were distributed among farmers and others, and every assistance seems to have been offered by the officials of the different states, of the Post Office, and of the railways. On the subject of the movements of locusts as many as 2,500 observations were thus obtained for the year.

The report is divided into nineteen chapters, and the scope of the work will be most conveniently indicated by giving a brief *résumé* of each.

Chapter I. is devoted to classification and nomenclature. It is pointed out that the words "locust" and "grasshopper" are often very loosely used, including diverse insects belonging even to different orders. The limitation of the use of the word "locust" in the report is explained at p. 33. "This family [speaking of the *Acrididæ*] contains the true locusts, such as those of Oriental countries and the Rocky Mountain locust; also such so-called grasshoppers as the common red-legged species of the States and those found hopping on the ground in open waste fields, along roadsides, &c. Therefore, in speaking hereafter of these species, we shall use the term *locust*." So that, insects belonging to the family *Locustidæ*, are not here included under the general term locust. The family *Acrididæ* is divided into three sub-families, the *Proscopinæ*, the *Acridinæ*, and the *Tattiginæ*. The first is an exotic family, and dismissed from further consideration. The *Tattiginæ* are comparatively few, quite small, and seldom noticed by unscientific observers. It is, therefore, only with the *Acridinæ*, which includes all the migratory locusts, that the classification deals. This sub-family contains several subordinate groups; but of these the writers remark, "no arrangement we have seen can be considered satisfactory." Reasons are given for excluding from consideration all genera except *Acridium* and *Caloptenus*. *Acridium* is limited in its permanent region to districts south of the latitude of St. Louis. *Caloptenus* has a wider region, and causes far greater losses by its ravages. After discussing Stål's *Calliptenus* the writers describe *Caloptenus* as they understand it (p. 40). There are twenty-nine species recognised in the United States, but of these there are but three which, for the purposes of the work, have to be considered, "as they are the only ones generally distributed, which are so closely allied to each other as to render it difficult to distinguish them." These are the *C. spretus*, Thomas, *C. atlanis*, Riley, *C. femur-rubrum*, De G. Three plates are devoted to illustrating the details of their appearance in different stages of growth. Throughout the report they are spoken of by their popular names: *atlanis* is called the lesser locust; *femur-rubrum*, the common red-legged locust; and from among the many names for *spretus*, such as "the hopper," "the army grasshopper," "hateful grasshopper," "Rocky Mountain locust," &c., the commission have adopted the last. It is with this Rocky Mountain locust and its depredations the report is mainly concerned, though the damages by others are not excluded.

Chapter II. (pp. 53 to 114) gives a chronological history of the ravages in past years, beginning with the imperfect records of as far back as 1818, and this is summarised in a tabular form (p. 113). With regard to this history it is pointed out that while the later years are recorded as years of wide-spread emigrations, it must be remembered that our means of obtaining statistics have improved. "There are no facts tending to show that the locusts themselves have been any more numerous of late years than previous to, for example, the years 1866-1867."

[1] See especially 4th, 5th, 6th, and 9th reports.

Chapter III. gives "Statistics of Losses." The estimate of losses is made by taking a "locust year" for comparison with one when there was no locust visitation. Dr. Hayden in his letter to the Secretary of the Interior, which accompanied the Report, has thus summarised it. The great practical importance of an exhaustive study of this destructive insect throughout all the immense extent of the locust area, which lies between the 94th and 120th meridian, embracing nearly two million square miles, may be realised from the fact that on a careful estimate from all the data obtainable, the States and territories lying west of the Mississippi and east of the great plains, suffered by the depredations of the locusts an aggregate loss in destruction of crops alone during the years 1874-77 of 100,000,000 dols., to say nothing of the indirect loss by stoppage of business and various enterprises which must have been as much more, thus making the direct and indirect losses at not less than 200,000,000 dols.

Chapter IV. (pp. 123 to 131) is occupied with considering the agricultural bearing of the locust problem, points out what crops suffer most, and discusses what is likely to be the effect of agricultural operations in the future. Such precautions as accurately knowing the dates of invading swarms and planting early or late accordingly are referred to.

Chapters V., VI., VII. (pp. 131 to 212) are occupied respectively with an account of observations on the "permanent breeding grounds," the "geographical distribution," and the "migrations" of the Rocky Mountain locust. Previous to 1877 very little was known of the breeding grounds. The Commission has been able to map the area and also map the districts subject to invasion, while the directions taken by invading and returning "armies" are also given. It is found that, as a rule, flight is undertaken only during a part of the day and in fair, clear weather, so that the desire for food, cloudy or rainy weather, and adverse winds, may keep them from rising and taking wing. In all flights it seems the locusts rely much on the wind to carry them, usually turning their heads towards the wind and drifting backwards. When the wind is slight, however, they use their wings and turn their heads forwards. Their flights can be continued for several days over a distance of several hundred miles. The rate at which they travel is variously estimated at from three to fifteen or twenty miles an hour, determined by the velocity of the wind. There are facts which show that they can fly two miles and a half above the general surface of Kansas and Nebraska, and far out of sight of the keenest vision. This will explain their often sudden and mysterious appearance in areas without anything having been seen of them on the line along which they travelled. Sometimes two swarms have been seen moving in opposite directions, one in an upper and one in a lower current. With regard to the return migrations, the Commissioners remark that they are led to the conclusion "that by some law governing them there is a tendency in the resulting broods hatched in this visited area to return to the native habitats from which their progenitors came." The connection of meteorological phenomena with migration is entered into at considerable length, and many pages of meteorological data are given.

Chapter VIII. (pp. 212 to 257) is devoted to habits and natural history. Various observations are collected as to the quantities of eggs laid and the conditions of hatching. The laying season is from six to eight weeks; the average interval of laying is two weeks, and the average number of egg-masses is three. The idea that locusts are led by kings or queens is unfounded. The reasons assigned for migrations are (1) hunger, (2) the desire to find fresh breeding-grounds, (3) to escape natural enemies, (4) "instinctive impulse." Though by choice their food is the various cereals, they will eat almost anything at a push, even "dry leaves, paper, cotton and woollen fabrics. . . . They do not even refuse dead animals, and have been seen feeding on dead bats and birds." They often strip fruit trees of their leaves. "Forest and shade trees suffer in different degrees, and some, when young, are not unfrequently killed outright." At the end of this chapter reference is made to unnecessary alarm often caused by comparatively harmless locusts.

Chapter IX. is on "Anatomy and Embryology," and this, according to a statement in the introduction, is by Mr. Packard. Two diagrammatic drawings and several figures illustrate this part of the work; and Mr. C. S. Minot has contributed a few pages on the "fine anatomy," illustrated by plates.

Chapter X. is on "Metamorphoses." The Rocky Mountain locust requires about seven weeks from hatching to attain full growth, and during that time it passes through six stages. Plate 1 illustrates these. Though in European migratory species there is a difference of opinion as to whether there are four or five moults, the writers say they have "thousands of mounted and alcoholic specimens of all ages" showing the six stages. "The number of moults may vary according to the amount of nutrition and rapidity of development."

In Chapter XI. on "Invertebrate Enemies," the life-histories of many insects are given, and this part of the work occupies fifty pages.

Chapter XII., on "Vertebrate Enemies," gives a *résumé* of what is known of the usefulness of birds. Blackbirds, prairie-hens, and quail, are found to be good locust destroyers, while a special section is given to stating reasons why the English sparrow should not be introduced.

Chapter XIII., seventy pages in length, is largely of interest to mechanicians, and deals with "remedies and devices for destruction." Many of the remedies are agricultural operations to be performed at particular times, according to varying circumstances, but the special devices, both protective and for "catching or bagging" eggs and insects are numerous and are illustrated by woodcuts. The three succeeding Chapters are on "influence of prairie fires on locust increase," "influence of weather on the species," "Effects that generally follow severe locust injury." Then follows a Chapter (XVII.) on the uses to which locusts can be put, in which it is urged they form an abundant and nutritious article of food. "Why should the people of the West, when rendered destitute and foodless by these insects, not make the best of the circumstances, and guard against famine by utilising them as food?"

The different methods of cooking locusts are entered into, and an account is also given of the use of them by different nations. They were counted as clean animals

by the Jews [Levit. xi. 22], and Herodotus mentions a tribe of Ethiopians which fed on locusts, which came in swarms from the southern and unknown districts.

Mr. Riley speaks of good broth being made "by boiling the unfledged *Calopteni* for two hours in a proper quantity of water, and seasoned with nothing but pepper and salt; the broth is hardly to be distinguished from beef broth." Boiled, fried, or roasted the full-grown are said to make pleasant food, and ground and compressed they will keep a long time. The other uses suggested are as fish bait, as manure, and as a source of formic acid.

There are altogether twenty-seven appendices occupying 279 pages, the last appendix giving the bibliography of the subject.

GUTHRIE'S PHYSICS

Practical Physics, Molecular Physics, and Sound. By Frederick Guthrie, Ph.D., F.R.SS. L. and E., Professor of Physics in the Royal School of Mines, London. (London: Longmans, Green, and Co., 1878.) [London Science Class-Books, edited by G. Carey Foster, F.R.S., and Philip Magnus, B.Sc., B.A.]

"THE works comprised in this series," the editors tell us, "will all be composed with special reference to their use in school-teaching; *but*, at the same time, particular attention will be given to making the information contained in them trustworthy and accurate, and to presenting it in such a way that it may serve as a basis for more advanced study."

The little word *but*, which we have taken the liberty to emphasise, seems to hint at some opposition between accurate statements and school-teaching, which, if not a fundamental necessity, is at least a universally existing phenomenon in the present order of things. This series of class-books is by no means the first attempt to procure books for children from writers of scientific reputation; and Prof. Guthrie, the author of this little book on practical physics, has himself invented several experimental methods at once interesting, ingenious, and simple.

If a child has any latent capacity for the study of nature, a visit to a real man of science at work in his laboratory may be the turning-point of his life. He may not understand a word of what the man of science says to explain his operations, but he sees the operations themselves, and the pains and patience which are bestowed on them; and when they fail he sees how the man of science, instead of getting angry, searches for the cause of the failure among the conditions of the operation.

Accordingly, in this little book the parts which are most interesting, whether to young or old, are those in which Prof. Guthrie describes his own beautiful experiments on the size of drops and bubbles, or teaches us how to blow glass. But if he once opens his ears to the siren song of the scientific imagination, floating down from heights unprofaned by experiment, through the window of the laboratory, and makes three paces through the room from the blowpipe to the lecture-table, we know that the curse has come upon him, and that for him it will never more be possible to reconcile the claims of accuracy with those of school-teaching.

What but some vile enchantment could have induced an intelligent man to begin his discourse to the poor little children in this style:—

"§ 1. **Hardness. Form-elasticity.**—The pressure required to alter the relative positions of two contiguous parts of a body measures its hardness. As this pressure is greater with greater surface of contact, some unit of surface must be fixed upon. The term hardness is generally applied loosely to difficulty of fracture. The following remarks may show that our speech and ideas in regard to hardness are deficient in precision. Glass is said to be harder than lead, yet a glass cup is more easily broken than a leaden one—more easily broken, though not so easily bent. Hard bodies are always elastic; elastic bodies are not necessarily hard, nor are they necessarily brittle, nor are soft bodies necessarily plastic. Toughness seems to imply a resistance to change of form, which resistance increases more rapidly than the displacement; thus, while a band of vulcanised caoutchouc will be extended to a degree proportional to the weight hung at one end, a leathern strap will not be extended twice as far if the weight on it is doubled. Toughness is generally associated with texture, and stretching causes partial fibration in the line of pull."

Here is a teacher who, with all the stores of science to choose from, selects, as the first lesson to a child, the necessity of fixing on a unit of surface, which, however, he makes no attempt to do, but goes on to harangue him on the deficiency in precision of our ideas and language in regard to hardness.

The poor child is not responsible for this want of precision; his first duty is not to reform his language, or even to criticise it, but to learn it, and if there is any part of human knowledge about which our speech and ideas have become tolerably precise, let us teach him that first, so that he may have some hope that knowledge is attainable before we let him see, as we must at last, how confused our own notions are.

Whether a child receives any special instruction in science or not, it is of unspeakable advantage to him if he is not put in the way of explaining things by false hypotheses. The difficulty which we have in recognising the paradoxical character of some of the most celebrated paradoxes shows how much has been done by the teachers of the last two centuries in causing false principles to be forgotten. The paradoxes are no longer paradoxical, because the dogmas which made them so are now known only to the owls and the bats.

We have selected a few statements in this book which we do not remember to have seen before.

(When a wire is stretched by a weight) "it may be assumed that the volume of the metal remains approximately unchanged, so that if the elongation is such that the length m becomes n, the original diameter d becomes $d\sqrt{\frac{m}{n}}$" (p. 4).

"A drop of water on a board strewed with powdered resin is nearly spherical." "The spherical is the form in which the mean distance of all parts from the centre of mass is the least. It is the most compact form for a given mass. This shows that cohesion moulds the drop to the spherical form" (p. 8).

Does Prof. Guthrie take his science from Rogers' verses on a tear? We refer him to Shakespeare ("King John," Act iii. Sc. 4) as a better authority on Capillary Attraction:—

"Bind up those tresses: O, what love I note
In the fair multitude of those her hairs!
Where but by chance a silver drop hath fallen,
Even to that drop ten thousand wiry friends
Do glue themselves in sociable grief,
Like true, inseparable, faithful loves,
Sticking together in calamity."

"A more exact method" (of measuring the viscosity of gases) "is to place timed chronometers under bell-jars containing various gases, and also *in vacuo*" (p. 19).

"**Diffusion of Gases into Gases.**—The unhampered diffusion of gases into gases has been little studied" (p. 39).

"Experiments show that the more a gas is soluble in a liquid the more is the liquid volatile in the gas" (p. 45).

"Whether the feeling called pitch depends upon the appreciation of the rapidity of sequence or upon the duration of each distortion of the ear-drum is not easy to decide, for the one is the inverse of the other. Perhaps the fact that a single long wave produces a different impression from that produced by a single short wave, and that this difference reminds one of the differences between grave and shrill notes, may be regarded as evidence that duration of individual impression rather than rate of sequence is to be considered as the origin of pitch" (p. 77).

After reading these statements, we have come to regard it as a decided merit, that in this book on Molecular Physics we are not told anything about molecules. The value of the book would be increased by cutting out "Molecular Physics" from the title, together with everything in the book included under that heading, and devoting the whole book to Practical Physics as adapted to the capacities and opportunities of young students.

J. CLERK MAXWELL

OUR BOOK SHELF

The Journal of the Royal Agricultural Society of England. Part II. (London: John Murray, 1878.)

THIS is no ordinary number of an agricultural journal. It is, in fact, a memoir on the agriculture of England and Wales, prepared under the direction of the Royal Agricultural Society, and presented by them at the International Congress held in Paris during the present summer. The memoir is now issued as the second part of the Society's *Journal* for the current year.

The memoir is well worthy of the Society under whose auspices it has appeared. It forms a large volume of over six hundred pages, and contains ten treatises on different aspects of English agriculture, each the work of a distinct author.

The first article in the memoir is a "General View of British Agriculture," by Mr. Caird. He commences with statistics as to the home and foreign supply of food, then glances at the changes introduced in agriculture during recent years by the increased use of machinery, and of artificial foods and manures, and last, though not least, by free trade. After a very brief notice of the differences of climate and soil in England he comes to his main subject—"the landed property of England," its character, distribution, ownership, improvement, value, and relation to Government. The whole essay is written with remarkable ability, and is full of important information.

The second article is on "English Land-law," by F. Clifford and J. A. Foote. It treats of succession, tenancies, agreements, leases, and recent legislation on the subject of unexhausted improvements. The third article is by Capt. Craigie; it deals with "Taxation," and describes the various kinds of taxation, and their incidence on the various classes connected with agriculture. This is followed by a short paper on "Farm Capital," by E. P. Squarey.

The fifth article is by much the longest in the volume; it has for its scope the whole subject of "Practical Agriculture"; it is written by Mr. J. A. Clarke. The article commences with a sketch of the climate and geology of England, and then proceeds to give statistics as to the crops produced, the number of live-stock maintained, the imports of manure and food, and the prices of agricultural products. Then follow chapters on the management of cattle, sheep, and pigs, with a description of the various breeds of live-stock, including horses. Crops and manures are then discussed, the practice of good farms in different parts of England being indicated. The concluding chapter is on machinery, and cultivation by steam. The whole article covers nearly two hundred pages; it is full of practical information, condensed into a small compass.

The sixth article treats of "Dairy Farming," and is written by Mr. J. C. Morton; to this is added an appendix on "Pastoral Husbandry," by W. T. Carrington. Mr. Morton, after reviewing the statistics of the subject, proceeds to describe the various breeds of cattle employed for dairy purposes in England, illustrating this part of his paper by wood-cuts. He then treats of the rearing of calves, the sale of milk, and the production of cheese and butter, describing in each case the practice of different parts of the country.

The seventh article treats of "The Cultivation of Hops, Fruit, and Vegetables," and is the work of Mr. C. Whitehead. It is naturally divided into three chapters. The subjects are treated statistically and practically, and much important information is given.

The next paper is of special interest at the present time, it is on "The Agricultural Labourer," and is written by Mr. H. J. Little. The past history of the British labourer is sketched, and his condition in various parts of the country described. His earnings, expenses, domestic life, education, and provident societies are treated of, and evidently by one who can speak from personal knowledge of the subject.

The ninth paper is the only one of a strictly scientific character; it deals with the "Influence of Chemical Discoveries on the Progress of English Agriculture," and is the work of Dr. A. Voelcker. The scope of this paper is hardly so large as its title, as the author generally limits his remarks to investigations made since 1860, and has nothing to say of Continental discoveries. The paper is for the most part an account of the investigations made by Messrs. Lawes and Gilbert at Rothamsted, and of the work done by the author himself. The subjects treated of are—The Soil, Continuous Cropping, Manures, Improvement of Permanent Pastures, Feeding and Rearing of Stock, Industries attached to the Farm, Experimental Stations.

The volume fitly concludes with an article on the history and work of "The Royal Agricultural Society," written by Mr. J. M. Jenkins, the Secretary of the Society.

It will be seen that the memoir issued by the Royal Agricultural Society includes a wide range of subjects; it would be hard indeed to find another volume containing as much information on English agriculture in the same compass. The whole is published at the extremely low price of six shillings. We trust that it will find a large circulation

R. W.

A Visit to South America; with Notes and Observations on the Moral and Physical Features of the Country, and the Incidents of the Voyage. By Edwin Clark, C.E. (London: Dean and Son, 1878.)

MR. CLARK'S modest little narrative is considerably superior to the ordinary run of modern books of travel, which have become as plentiful, nearly, as novels, though we are glad to say, on the average, much more worth reading. Mr. Clark has certain scientific qualifications

which give him an advantage as an observer of phenomena both on sea and land, and the results of which are apparent in the volume before us. Mr. Clark's narrative relates to the years 1876-77, during which he resided for nearly two years in Buenos Ayres, Paraguay, and Uruguay. The two latter regions are yet sufficiently unknown to make any contribution from a competent observer who has visited them, welcome. The information, especially, which he gives us on Paraguay, is of much importance, and is a valuable addition to that obtained by Mr. Keith Johnston, in his visit two or three years ago. From a scientific point of view, perhaps the most valuable portion of Mr. Clarke's book are the numerous meteorological notes which he made both during his voyage out and his stay in South America. His knowledge of meteorology in its widest sense seems to us both extensive and accurate, and his observations on the instruments he used, on doldrums, tropical evaporation, and other such topics, are really interesting. But Mr. Clark knows something also of botany, as is evident from the frequent observations in this direction to be found throughout his volume. A whole chapter is devoted to the climate and meteorology of Buenos Ayres, important both from a scientific and practical point of view, as it is one of the great centres of emigration for South America. Many interesting sketches are given of the people and their mode of life in the various districts visited by Mr. Clark, and altogether his work is one of substantial value and real interest, and we trust it will find many readers.

Our Railways: Sketches Historical and Descriptive, with Practical Information as to Fares, &c., and a Chapter on Railway Reform. By Joseph Parsloe. (London: Kegan Paul and Co., 1878.)

MR. PARSLOE'S volume contains a large amount of very varied information on railways, their origin, their working; its object, he tells us, being to present a sketch of our railway system in its general details. The contents are so varied it would be difficult to give any idea of their nature without a lengthened notice. Mr. Parsloe goes back to the old days of stage-coaches, coming down to the origin of railways, then speaks of their construction, of navvies, working expenditure, signals, gauges, tickets, and a multitude of other topics all of much interest to the travelling public. The book is certainly both interesting and instructive.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Formation of Mountains and the Secular Cooling of the Earth

THE letters of Mr. Wallace and Mr. Fisher in NATURE, vol. xix. pp. 121, 172, 244, 267, raise the question as to whether or not it is possible that the interior of the earth can be cooling more rapidly than the exterior. The following is an attempt to answer the query as to where the loss of temperature per unit time is greatest.

Sir W. Thomson (see Thomson and Tait, "Nat. Phil.," App. D) considers the cooling of "a solid extending to infinity in all directions, on the supposition that at an initial epoch the temperature has had two different constant values on the two sides of a certain infinite plane." The solution given is—

$$v = v_0 + \frac{2V}{\sqrt{\pi}} \int_0^{\frac{x}{2\sqrt{kt}}} e^{-z^2} dz$$

where k denotes the conductivity of the solid, measured in terms of the thermal capacity of the unit of bulk;

V, half the difference of the two initial temperatures;

v_0, their arithmetical mean;

t, the time;

x, the distance of any point from the middle plane;

v, the temperature of the point x at time t.

The above solution shows that for all values of the time when $x = 0$, $v = v_0$, so that the temperature at the medial plane is constant.

Then differentiating v with regard to the time we have—

$$-\frac{dv}{dt} = \frac{V}{2\sqrt{\pi k}} \frac{x}{t^{\frac{3}{2}}} e^{\frac{-x^2}{4kt}}.$$

This expression is that required for the rate of cooling. We now wish to find where it is a maximum. Consider the function ze^{-z^2}; this is clearly a maximum when $\log z - z^2$ is a maximum, and by the ordinary rules this is a maximum when $\frac{1}{z} = 2z$, or when $z^2 = \frac{1}{2}$.

Hence it follows that $-\frac{dv}{dt}$ has its maximum value where $x^2 = 2kt$.

Now when the unit of length is a foot and of time a year, $k = 400$; hence $x = \sqrt{800t}$.

This formula shows that the seat of the maximum rate of cooling moves inwards as the time increases. If the time which has elapsed from the initial state be two hundred million years, or $t = 2 \times 10^8$, we have $x = 400{,}000$ feet, or a little less than eighty miles.

Sir W. Thomson shows, in his paper on the Secular Cooling of the Earth, that the solution of his ideal problem will be very nearly correct for the case of the earth, which is supposed to be a hot sphere cooling by radiation.

It follows, therefore, from the numerical result which is given above that the seat of the maximum rate of cooling must probably be something like 100 miles below the earth's surface.

It does not, of course, necessarily follow that the seat of the maximum rate of contraction of volume should be identical with that of the maximum rate of cooling; yet it seems probable that it would not be very far removed from it.

The Rev. O. Fisher very justly remarks that the more rapid contraction of the internal than the external strata would cause a wrinkling of the surface, although he does not admit that this can be the sole cause of geological distortion. The fact that the region of maximum rate of cooling is so near to the surface recalls the interesting series of experiments recently made by M. Favre (of which an account appeared in NATURE, vol. xix. p. 103), where all the phenomena of geological contortion were reproduced in a layer of clay placed on a stretched india-rubber membrane, which was afterwards allowed to contract. Does it not seem possible that Mr. Fisher may have under-estimated the contractibility of rock in cooling, and that this is the sole cause of geological contortion?

G. H. DARWIN

Storm Warnings

A NEW YORK telegram occasionally announces that a cyclonic storm will probably reach the coast of Europe in a few days.

Such warnings are often of great value; but many storms are deflected in the Atlantic, while others—without having touched the American coast—come unannounced with destructive violence.

A floating buoy might be constructed to serve the purpose of a marine observatory, when placed in the usual track of storms at a sufficient distance from exposed coasts to be useful for *warnings* for ships in and near harbours.

The chief meteorological "elements" which are of essential significance in such a case are the *height* and changes of the barometer, and the varying force and direction of the wind.

If an experimental buoy were fixed by means of a slightly elastic cable about eighty miles off Valencia Observatory, and connected therewith by submarine telegraph wire, a slight modification of the aneroid lodged therein would enable the observer on shore to determine to about a tenth of an inch the height and changes in its *readings*.

A wind-vane in connection with a magnetic bar, and presenting a disk to the air-current, might be made the means of registering approximately the force and direction of the wind.

If the plan were successful, other meteorological facts might be determined by passing a current through mechanical indicators attached to each piece of apparatus.

Changes of temperature, electric conditions, and rainfall, might ultimately be brought within the scope of such a plan of telegraphic registration, and three or four floating observatories might be arranged at considerable distances apart.

The problem thus presented to the mechanician is the construction of apparatus such that in passing an electric current successively through indicators specially devised for each instrument, *readings* could be made and announced to all concerned.

By such means all coasts liable to be visited by progressive storms might have timely warning of danger.

The cost of such work would be very small in comparison with the saving of life and property concerned.

The Board of Trade might be induced to offer a substantial reward for the most efficient models of such floating stations.

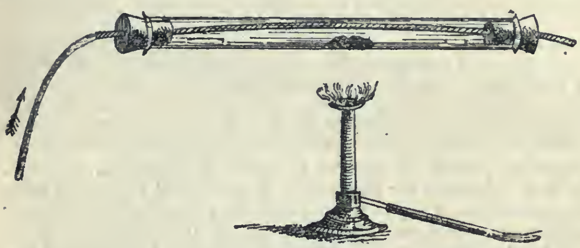
The essential feature of this proposal is, that new instruments should be devised as entirely different in form from those in use as the aneroid is from the old barometer.

There is no reason to doubt that whenever instruments are devised in which the passage of an electric current can be made through the indicators, it will be as easy to take readings of meteorological instruments at the distance of a thousand miles as when in sight, and with sufficient accuracy for the purposes in view. A HUTTON BURGESS

The Dissociation of Sal-Ammoniac—An Experiment

ALL chemists admit that when sal-ammoniac is volatilised the vapour consists, if not wholly, at least in great part, of hydrochloric acid and ammonia gases in the free state. But this fact, so far as I am aware, is very seldom, if ever, demonstrated experimentally by teachers. The following modification of Pébal's original experiment renders this proof very easy and available for lecture purposes:—

The stem of a long clay tobacco-pipe is passed loosely through a couple of perforated corks fitted into the two extremities of a piece of ordinary combustion tubing about a foot long. The tube contains in the middle a small lump of sal-ammoniac, and



near each end a strip of blue litmus paper. When the middle of the tube is heated the vapour of the sal-ammoniac surrounds a portion of the pipe-stem. If, now, a rapid stream of air or any other indifferent gas is sent through the pipe, it is found to be strongly charged with ammonia, so that it answers freely to all the usual tests. At the same time the litmus papers contained in the glass tube become red owing to the accumulation of hydrochloric acid in the residue. This experiment, of course, depends upon the diffusion of the lighter ammonia through the clay more rapidly than the hydrochloric acid also present.

WILLIAM A. TILDEN

The Sting of the Bee

IN "The Origin of Species," p. 242, fourth edition, Mr. Darwin says, "If we look at the sting of the bee as having originally existed in a remote progenitor as a boring and serrated instrument, like that in so many members of the same great order, and which has been modified, but not perfected, for its present purpose, with the poison, originally adapted for some purpose such as to produce galls, subsequently intensified, we can, perhaps, understand how it is that the use of the sting should so often cause the insect's own death; for if, on the whole, the power of stinging be useful to the social community, it will fulfil all the requirements of natural selection, though it may cause the death of some few members." In a lecture given as it happens, this day ten years ago, I ventured to suggest that bees may have derived advantage, not in spite of the fatal condition annexed to the use of their sting, but from that condition itself, since "it may have proved expedient for a creature to be armed with a weapon capable of inspiring terror, yet so contrived, that its possessor should of necessity be peaceful towards its neighbours." It is very certain that many gentle-hearted human beings wage remorseless war upon wasps, who would never think of harming a bee or a bluebottle. On the other hand there are many mischievous persons ready enough to trifle with the feelings of a bluebottle, who keep at a respectful distance from a bee, simply because they know it possesses a certain power of revenge. In this way the sting is not, as your correspondent "R.A." is inclined to think, worse than useless to the individual bee, but an effective protection, albeit rather as a shield than a sword. What is needed for its efficacy is not so much intelligence in the bee as in those who would otherwise attack the bee, and though to the individual bee a single experience ending in its own death could be of no avail, yet the other animal, the wounded survivor in the fray, would have its understanding wonderfully quickened to the advantage of all bees it might meet in the future.

If, then, the bee is actually better off with its imperfect sting than it would be with one theoretically more perfect, it may be scarcely worth while to inquire whether a more effective weapon could or could not be developed on the principles of natural selection. But assuming that under given circumstances bees would derive advantage if the sterile workers had stings which they could use without sacrificing their own lives, the very statement of the hypothesis implies that a swarm, in which such workers were developed, would have an improved chance of surviving in the struggle for existence. Enemies would be more certainly vanquished; food would be more securely stored or in greater abundance; and thus the particular strain which had produced the improved variety would be more likely than others less favoured to be transmitted to future generations. The power of producing the better-armed warriors would be transmitted just as the power of producing the worse-armed warriors is transmitted, neither in the one case nor the other through the warriors themselves. THOMAS R. R. STEBBING

Tunbridge Wells, February 1

Fossil Forests and Silicified Trunks

IN NATURE, vol. xix. p. 257, the discovery of fossil forests in the spring region of the Yellowstone River is referred to. I have visited the United States National Park, and its geysers, and observed exactly how silicified trunks *in situ* originate. All geologists suppose that this must have happened beneath water, and consequently Mr. Holmes supposes a constant alternation of land and sea throughout a long period of subsidence. My observations show the contrary, as silicified trunks originate only in air, never in water. The siliceous hot water of the geyser basins runs off periodically in another direction; if it comes to a forest, then all green leaves, all bark, and most of the branches fall off, but the trunks remain erect. Now the siliceous water rises by capillary attraction in the stem, but only on the outside of the trunk does the siliceous acid become solid by drying in the air; from the outside the silicification of the wood cells enters very slowly to the inner part; the trunks are mostly struck down by the wind before the inner part gets petrified, and then the inner part shows no ligneous structure, is only filled with foreign matter, or sometimes with other minerals, or it is hollow, for the inner wood decays. The white silicified wood is for a long time soft, less coherent than common wood, and if such trunks fall down into water, as I observed, they never get hard. Those white forests without leaves, bark, and branches, are not rare around the geysers. With my observations accord all characteristics of silicified trunks, *i.e.* such carbonaceous trunks excepted, that consist only of outfilled matter, stone kernels, for all real silicified trunks are barkless, leafless, branchless, often with inside hollow or partly filled, and always found along with common opal derived also from geysers.

Is silicification of trunks with well-preserved structure possible beneath water? No proof has yet been given. And further, would it be possible for stems, which are lighter than water, to remain *in situ* and erect by sinking under water? Scarcely—only if previously silicified and heavy.

Besides, most statements of travellers on fossil forests relate to the tropics. I saw several on my voyage round the world, which consisted only of stems lying together.

Leipzig-Eutritzsch, January 28 OTTO KUNTZE

Force and Energy

YOUR correspondent, Mr. R. H. Smith (NATURE, vol. xix. p. 194), speaks of "the fine old crusty Newtonian maxim . . . 'force is any CAUSE which,' &c." Now Newton's words are these: "Definitio IV.—Vis impressa est ACTIO IN CORPUS EXERCITA, ad mutandum ejus statum vel quiescendi vel movendi uniformiter in directum." It will be observed that Newton avoids the use of the obnoxious word CAUSE. I suppose that some translator, or commentator on Newton, adopted the word "cause" (in the sense, probably, not of an efficient cause in itself, but, by a common figure of speech, of the *action* of some cause), and that other writers transcribed the expression.

Prof. Tait, who is specially referred to by your correspondent, seems to have overlooked the above definition when he wrote ("Recent Advances," ed. i. p. 16): "the definition of force in physical science is implicitly contained in Newton's 'First Law of Motion,' and may thus be given: *Force is any cause*," &c. Newton, in that law, speaks of "vires impressæ," but forbears there to define, or explain, "vis." Clearly he refers back to Def. IV., where, as I have shown, he defines "vis impressa" by "actio," not by "causa."

In justice to Prof. Tait, however, it should be pointed out that in the passage referred to he proceeds at once to discuss the difficulty introduced by the word "cause." He has, in fact, anticipated your correspondent in the idea of his definition of force. Prof. Tait writes thus: "In every case in which force is said to act, what is really observed . . . is either a transference or a tendency to transference of what is called energy from one portion of matter to another. Whenever such a transference takes place there is relative motion of the portions of matter concerned, and the so-called force in any direction is merely the rate of transference of energy per unit of length for displacement in that direction."

J. G. H.

Electrical Phenomena

MR. GREEN, in his letter to NATURE, vol. xix. p. 220, omits to state the route by which he ascended Monte Rosa "not long since." This is a detail of interest, because the rocks of that range are decidedly magnetic, and much hidden on the north side by ice.

In 1875, much out of sorts, I was training by short climbs, and at the Kiffleberg, well known for its effect upon the magnet, strolling up the Gorner Grat in company with three other members of the Alpine Club, and several more, the sky quickly clouded over, it thundered, and the axes of the Alpine men fizzed in most orthodox fashion, especially when held up, and the long sticks of the non-climbing men also crackled. A transitory but vivid lightning storm followed.

Several days later, during an attempt to ascend the Stockhorn, in company with a young Englishman, from the north side, by the Triftje glacier, the same fizzing, concurrently with snow, thunder, and lightning, took place, and half up the last glacier a violent storm came upon us, and throwing caution to the winds, we both skeltered down the snow and ice slopes with scant respect for crevasses seen and hidden. But for the mountaineer's axiom, "never part with your axe," we were much inclined to throw ours on one side. Soon we got below this critically charged stratum of air and earth, and the fizzing ceased. I shall never forget that terrible half-hour, only to be imagined by mountaineers or seafarers. Forbes, in his splendid work on glaciers, relates a similar incident somewhere in this same range.

Positive and negative changes of earth and air, conductibility of these and of axes, and involuntary experimenters suggest themselves. In our latter case all were more than *damp!*

I have not Forbes' book here, and can therefore quote no details. Thunderstorms are characteristic. In 1849 (I think it was) I made a new pass, called the Neue Weiss Thor. Overhead it was fine. A mile below was a thunderstorm, and during our descent on the Italian side, we came into it, and were refreshed first by snow, then by rain, till we reached Macuguaga.

MARSHALL HALL

Vernex-Montreux, Canton Vaud, Switzerland, January 27

Ear Affection

SEVERAL years ago, during an attack of whooping-cough, I found that one of my ears was so affected as to cause sounds heard by that ear to seem flatter than their true pitch as heard by the other ear. The difference was about a semitone, as I ascertained by holding a tuning-fork to each ear alternately; and when I whistled I heard two notes in discord. The affection lasted about ten days.

Will one of your readers kindly render me an explanation.

Adelaide, November, 1878 P.

RELATION OF METEORITES TO COMETS[1]

I HOLD in my hand a stone that weighs about two and a half pounds. Over a part of its surface is a thin black crust. A part of its corners are cracked off, showing a gray interior, and on looking closer you see small points of iron all through it. It is heavy—about one half heavier than granite, or marble, or sandstone. Altogether it is a very curious stone, totally unlike any of our rocks.

That stone was once a part of a comet.

Do you want my reasons for saying it? Or, does any one doubt it? I propose to-night to give those reasons; to set in order, as clearly and simply as I can, the facts and lines of thought that lead me to say as I did—*that stone was once a part of a comet.*

It came to us from Iowa. Three years ago, on February 12, about ten o'clock in the evening, the light of a bright meteor was seen by nearly everybody then in the open air in the south-east part of that state. I will quote from a vivid description of the meteor given by Mr. Irish, a civil engineer of Iowa City, who has collected and published many facts about it: "The observers," he says, "who stood near to the line of the meteor's flight, were quite overcome with fear, as it seemed to come down upon them with a rapid increase of size and brilliancy, many of them wishing for a place of safety, but not having the time to seek one. In this fright the animals took a part, horses shying, rearing, and plunging to get away, and dogs retreating and barking with signs of fear. The meteor gave out several marked flashes in its course, one more noticeable than the rest. . . . Thin clouds of smoke and vapour followed in the track of the meteor. . . . From one and a half to two minutes after the dazzling, terrifying, and swiftly moving mass of light had extinguished itself in five sharp flashes, five quickly recurring reports were heard. The volume of sound was so great that the reverberations seemed to shake the earth to its foundations; buildings quaked and rattled, and the furniture that they contained jarred about as if shaken by an earthquake; in fact, many believed that an earthquake was in progress. Quickly succeeding, and blended with the explosions, came hollow bellowings and rattling sounds, mingled with clang, and clash, and roar, that rolled away southward, as if a tornado of fearful power was retreating upon the meteor's path."

From accounts collected from eye-witnesses by Prof. Leonard and Mr. Irish, I conclude that the meteor when first seen was not less than sixty miles high over Northern Missouri; that it descended at an angle of about 25° with the horizon, in a right line, and disappeared at a height of five or ten miles. Those in the east, as at Kiokuk, saw it low in the west. From St. Louis it was seen in the north-west. In the western part of Iowa it was seen to pass north across the eastern sky. To persons in the north it passed straight down on the southern sky, while to those under the path named it passed nearly overhead, rising in the south and south-west and descending in the north north-east. The path thus determined is at least 120 miles long, and was passed over in a few seconds, probably not over ten. The country near the explosion was prairie or alluvial, where stones on the surface are rarities, and about 800 lbs. of stones like this one, nearly 200 in number, have been picked up in a region seven miles by four, a little east of the end of the

[1] A lecture delivered in the Mechanics' Course at the Sheffield Scientific School of Yale College, U.S., by Prof. H. A. Newton.

meteor's path. These are all supposed to come from the meteor. Some were picked up on the surface of the frozen ground. One was found on the top of a snow-bank, and about forty feet away were marks of a place where it had first struck the ground. Some were ploughed up in the spring. The two largest found, of 74 lbs. and 48 lbs., fell by the roadside, and a law-suit to settle whether they were the property of the finder as being wild game, or of the owner of the lands adjacent as being real estate, was decided in favour of the owner of the land.

No one saw this stone come from that meteor. But in many cases peculiar stones very like to this one have been seen to fall from meteors, and this is one of a group of about twenty stones belonging to Yale College which were gathered at the places and directly after the time of the fall. They are in the Peabody Museum in a case by themselves, and are about one-tenth of all that has been found.

But though we have no eye-witnesses to speak of its fall and finding, the stone as we look at it tells its own story. This rounded side is not waterworn. From your seats you cannot see them, but over these rounded hills and down these valleys run streaks showing that melted matter has flowed over them. On two of the smaller sides is collected a real lava deposit, giving in smallest miniature the twisted gnarled forms that some of you have possibly seen in the large lava beds at the foot of the cone of Vesuvius. This other surface had just begun to be melted, as though the fracture that formed it had been made late in the meteor's flight. This larger face is only smoked, and we might even doubt whether the stone had not been broken here after its fall. But the rounded edges of the thin black crust at the angles of the stone show clearly that, except perhaps at some of the corners, the stone was in its present shape when it struck the ground.

Now what caused that brilliant light, that terrific explosion which was heard for forty miles around, that rain of stones? The only explanation which we can admit is that a stone weighing not less than 800 lbs.—how much more we know not—perhaps two, perhaps fifty times as much, came into the air from without.

What ought to happen upon the passage of such a stone through the air? At the height of thirty to eighty miles, the region where the meteor-tracks are most frequently seen, the air is very rare, rarer than in the so-called vacuum of an air-pump. Yet the rapid velocity of the stone condenses the air in front of it. If we admit the truth of the kinetic theory of gases we must regard the molecules of air as in rapid motion, each molecule driven this way or that, coming in contact with and bounding back from other molecules. The average velocity of these dancing molecules of air at usual temperatures is a fraction of a mile per second. They therefore bound back from any heavy body that moves only a few hundred feet per second, only slightly checking its velocity. But the air is here met by a stone moving, say, fifty times as fast as the average molecule. The molecules are driven together beyond the possibility of getting away, until the temperature of the air is raised enormously. Probably the air is liquefied by the pressure, and then pushed aside by main force till the meteor has passed, when it is driven back again into the vacuum behind, giving us a flame shaped like that of a candle.

What effect has all this on the stone? *It* is solid and firm, as you see, and can withstand not a little pressure. It is not, therefore, heated within; but on the outside it is in contact with, or rather rubbed hard against, an intensely hot stratum of air. It is therefore melted off just as a piece of tallow would be melted if drawn across a white hot iron. There is no time for the heat to pass by conduction deep into the stone. The melted matter is wiped off by the air. A part clings to these hinder faces of the stone, but the far greater portion helps to make up the meteor's train. It is scattered in eddying currents in a long, narrow, whitish cloud, at first straight, then twisted. That cloud broadens and floats away in contorted forms, remaining visible sometimes a second, sometimes an hour even. The pressure and the heat generally keep cracking the stone, just as any stone is cracked by pressure or when thrown into a hot fire. Parts may survive this treatment and reach the ground. Those who have picked them up as they fell have generally said they were hot, as they must be on the outside. But some have been found, it is said, that were very cold. This, too, we may well believe, for they should retain in their interior the intense cold of space.

This stone in my hand shows the breaking up, one fracture being very clearly more recent than another, and if you were near me you might even see fractures that were begun but not ended when the stone reached the ground.

We often *see* this breaking up. On the wall is a picture of the principal explosion of the Iowa meteor, as given by one who saw it, representing, it may be, the cracking when this fragment was broken off from the main mass. There is also one of a meteor seen in Greece in 1863 by Dr. Schmidt. He was standing on the roof of his house in Athens when he caught sight of a magnificent fireball, moving so slowly that he was able to turn his telescope upon it. The head had two main parts, which were chased by a motley troop of followers, each drawing a bright line on the sky, all of which, at a distance of three or four degrees, melted into a reddish cloud of light. Often a meteor is to the naked eye made up of a group of smaller ones, the whole being like a flock of birds.

I have traced back the history of this stone to its entrance into the air on February 12, 1875, when it was part of a mass not less than two feet, and I suspect not more than ten feet each way. It looked larger, but men saw the flame around the stone, not the stone itself. By itself, and strictly taken, this history has gaps; but taken along with the history of like stones and meteors that are numerous in the records of science, the story is easily filled out as I have given it above. No scientific man today would question it.

The next step in my argument, though admitted by most, is not admitted by all of those whose opinions in this matter are entitled to special respect. I am not aware, however, that anybody has given any formal reasoning against it. I claim that between this stone-producing meteor of Iowa and the faintest shooting-star which you can see on a clear night in a telescope there is no essential difference as to astronomical character. In all their characteristic phenomena there is a regular gradation in the meteors from one end of the line to the other. They differ in bigness, but in their astronomical relations we cannot divide them into groups. They are all similar members of the solar system.

To prove this we must of necessity rehearse the points in which the large and small meteors are alike and unlike.

First. They are all solid bodies. The Iowa meteor sent down these stones, and we know that they are solid. This other stone which I show you is one of about 4,000 which fell from a meteor in Poland in 1868, and this you can see is solid. In the Peabody Museum is a goodly collection of such stones from other meteors.

A year ago last December, early on the evening of the 21st, a meteor entered the air sixty miles or more in height over the north-west corner of the Indian Territory, or it may be still farther west. It crossed at a height of between sixty and thirty miles the states of Kansas, Missouri, Illinois, Indiana, and Ohio and passed on over Lake Erie and the state of New York. No sound was heard, so far as I know, in the state of Kansas; but in Missouri, and still more, in Illinois, the explosions were

fearful, and multitudes of fragments were seen to fly off by every one who saw the meteor. In Indiana it was thought that the explosions were heard at Bloomington 150 miles from the nearest point of the path. In New York State the sky was wholly overcast, so that of course nothing was seen. But at many places the people thought there was an earthquake. Was this a solid body? As if to remove this from the class of detonating into that of stone-producing meteors, one single small fragment three-fourths of a pound in weight, was heard to fall and was picked up the next morning on the snow in Indiana. A piece of this is in the Peabody Museum.

In 1860 a meteor went north-west across Georgia and Tennessee and exploded, disappearing nearly over the southern boundary of Kentucky at a height of about twenty-eight miles. There was the same terrific explosion heard, the same scattering of fragments seen. The meteor was seen over all the region from Pittsburgh to New Orleans, and from Savannah to St. Louis. But from this meteor no stone was found, but you cannot doubt for all that that it was a solid body.

So, a few weeks ago a meteor fell in broad daylight in Southern Virginia, the sound of which, over a limited region, seemed like an earthquake. It, too, must have been solid.

In July, 1860, some of you, I presume, saw a meteor cross from the west to the east. It came from over Northern Michigan, and was seen until it had passed at least 200 miles east of us, passing between us and New York City at a height of a little more than forty miles. One pear-shaped ball chased a second and a third across the sky. People listened for the sound to come, and one or two thought that they heard it but would not affirm that it was sound from the meteor. I cannot doubt that that too was solid. It was seen to break in two, and the parts passed on one after the other for hundreds of miles. To be sure no stone was found from it, and perhaps no sound heard, yet that it was solid seems to me almost as sure as if I had a piece of it in my hands.

Again, going one step farther, how can we avoid calling all the meteors solid which are seen to break into pieces, and all those which glance, describing a curved course, or a course having an angle? The number of such cases is large, and often they are very faint shooting-stars. But it is doubtful whether a small gaseous mass could exist permanently as a separate body in the solar system. Its repulsion would keep the parts so far asunder that the sun's unequal attraction would scatter the substance beyond all its own power of recovery. A liquid would probably freeze and become solid. In any case neither a gas nor a liquid could for an instant sustain the resisting pressure which a meteor is subjected to in the air, much less could it travel against it ten, or forty, or a hundred miles. In short, every shooting-star *must be* a solid body.

Second. The large meteors and the small ones are seen at about the same height from the earth's surface. The larger meteors may become visible a little higher than shooting-stars, though that is doubtful; they come down in general a little lower, some of them even come to the ground, but that is due rather to the size of the body. The air is a shield to protect us from an otherwise intolerable bombarding. Some of the larger balls come through that shield, or, at least, are not all melted before their final explosion, when the fragments, their original velocity all gone, fall quietly to the ground. The small ones burn up altogether, or are scattered into dust.

In the *third* place, the velocities of the large and small meteors agree. These velocities are never very exactly measured directly; but we are sure that in general they are more than two and less than forty miles per second. This is true both for small and for large meteors. The average velocities for each class are not widely different.

We sometimes need a name for the small body that will, if it should come into the air, make a shooting-star or larger fireball. We call such a body a meteoroid. Now, velocities of from ten to forty miles a second imply that the meteoroids are bodies that move about the sun as centre, or else move through space. These velocities, as well as other facts, are utterly inconsistent with a permanent motion of the meteoroids about the earth, or with a terrestrial origin, or with a lunar origin.

Fourth. The motions of the large and small meteors, as we see them cross the sky, have no special relations to the ecliptic. If either the one or the other kind had special relations to the planets in their origin or in their motions we should have reason to expect them, if not always, at least in general, to move across the sky *away from* the ecliptic. But the fact is otherwise. We see both small and large meteors move *towards* the ecliptic as often as *from* it. Neither class seem, therefore, to have any relation to the planets.

Again, in general character the two classes are alike. They have like varieties of colour, they have similar luminous trains behind them; in short, we cannot draw any line dividing the stone-producing meteor from the shooting star, at least in their astronomical relations. We cannot say that the Iowa meteor is different from the Georgia meteor of 1860, on the ground that stones were found in one case and not in the other; or that the meteor of December, 1876, was different from that of July, 1860, on the ground that one had a series of terrific explosions and the other was only seen to break into parts; or that the meteor that is seen to break into parts differs from one evidently solid, that burns up without any appearance of explosion. They all are astronomically alike. They differ in bigness; but this has nothing to do with their motion about the sun or in space.

When, therefore, we learn something about the origin and motions of the smaller meteoroids, we can infer like facts about the larger ones. I propose, then, to show that shooting stars were once pieces of comets.

(*To be continued.*)

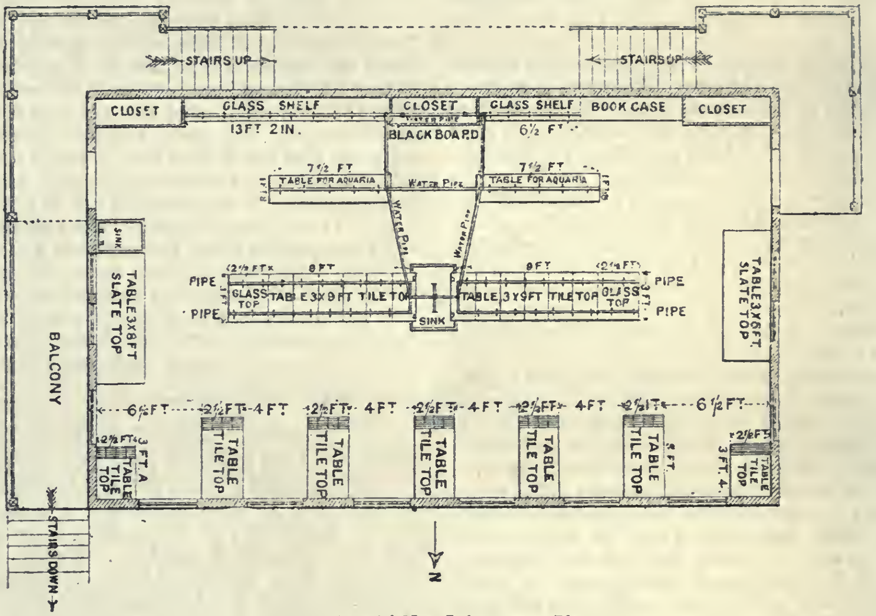
A ZOOLOGICAL LABORATORY

PROF. ALEXANDER AGASSIZ, in his Report to the President and Fellows of the Harvard College Museum for 1877 and 1878, to September 1, gives an account of his new laboratory at Newport, a plan of which he has been good enough to send us. This is the first report which has been presented since the Museum has come under the care of the President and Fellows of Harvard College, and the description given by Prof. Agassiz sufficiently indicates that the Museum is a model of its kind. During the past eighteen months increased funds have been placed at the disposal of the Museum, and excellent use has been made of them. Not only is the Museum arranged so as to make it of the greatest service to students, but in such a way that the portion thrown open to the public must have an excellent educational effect. Everything has been done to make visitors clearly understand what they see, and evidently this attention is appreciated and is answering its purpose.

The new laboratory, erected by Prof. Agassiz at his own cost, and which is a model of what such a place should be, is described by Prof. Agassiz as follows:—

The new laboratory erected by me at Newport is twenty-five feet by forty-five. The six windows for work are on the north side, and extend from the ceiling to within eighteen inches of the floor. In the spaces between the windows and the corners of the building are eight work-tables, three feet by five, covered with white tiles, one foot of the outer edge being covered, however, with black tiles for greater facility in detecting minute animals on a black background. Between the windows, movable brackets with glass shelves are placed; while similar

brackets extend across the windows and between the tables, thus providing a shelf at any desired height. The tables for microscope work are three-legged stands of varying height, adapted to the different kinds of microscopes in use. The whole of the northern side of the floor upon which the work-tables and microscope stands are placed is supported upon brick piers and arches independent of the main brick walls of the building, which form at the same time the basement of the building. The rest of the floor is supported entirely upon the inside walls and upon columns with stretchers extending under the crown of the arches reaching to the northern wall. This gives to the microscopic work the great advantage of complete isolation from all disturbance caused by walking over the floor. This will be duly appreciated by those who have worked in a building with a wooden floor, where every step caused a cessation of work, and was sure to disturb any object just at the most interesting moment. The floor is cemented and covered by a heavy oil-cloth. The centre of the large room is occupied by a sink, on each side of which extend two long tables, three feet by twelve. These are covered with different coloured tiles, imitating mud, sand, gravel, sea-weed, black and white tiles, as well as red, yellow, blue, green, violet, to get all possible variety of background. A space at each end is covered by a glass plate, allowing the light to come from underneath, thus enabling the observer to examine larger specimens from the under-side, without disturbing them when fully expanded. Two shorter and narrower tables, eighteen inches by seven feet, are placed halfway between these central tables and the southern face of the building. These tables are intended for larger aquaria or dishes, and are covered with common marble slabs. There is a blank wall on the south side, the whole of which is occupied by closets and shelves for storing glass jars, reagents, bottles, dishes, and so forth. A space is devoted to books. The open shelves for jars and dishes are of heavy rolled glass, supported upon iron brackets. The basement is used for the storage of alcoholic specimens, dredges, trawls, and other similar appliances. In the attic there is a large tank for salt water and another for fresh: the rest of the attic space will be eventually devoted to photographic rooms and room for an artist. The laboratory is supplied with salt water by a small steam-pump



Prof. Agassiz' New Laboratory: Plan.

driven by a vertical boiler of five-horse power: this is kept going the whole time day and night, the overflow of the tank being carried off by a large pipe. The water is taken some distance from the laboratory, and drawn up at a horizontal distance of sixty feet from the shore in a depth of some four fathoms, the end of the suction pipe standing up vertically from the ground a height of five feet, and terminating in an elbow to prevent its becoming choked. The water is led through iron pipes coated inside with enamel. From the tank the salt water is distributed in pipes extending in a double row over the central tables, over the long narrow tables for aquaria, and along the whole length of the glass shelves on the south wall. Large faucets to draw off salt water are placed at each sink, and by a proper arrangement of valves it is possible to lead fresh water to a part of the pipes, in case it is needed. The pipes leading over the tables and shelves are provided with globe valves and nozzles, to which rubber pipe can be attached and the water led to a vessel below: there are fifty such taps, each of which can supply water or air to at least three or four jars. The overflow runs into gutters laid alongside the tables, leading into the main drain-pipe. To aerate the salt water I use an injector invented by Prof. Richards, of the Institute of Technology. This can be used to supply aerated water directly to the jar by providing it with a siphon overflow, or the aerated water can be kept in a receiver, from which air alone is then led to the jar. This latter course is the only practical one for delicate specimens and for the bulk of the work of raising embryos. The east and west sides have large windows and doors provided with blinds; they always remain open with the blinds closed to keep out sunlight, and serve to ventilate the laboratory thoroughly. Large tables for dissection, covered with slate and adjoining a sink provided with fresh and salt water, are placed across the windows of these sides. Ever since the closing of the school at Penikese it has been my hope to replace, at least in a somewhat different direction, the work which might have been carried on there. It was impossible for me to establish a school on so large a scale, but I hope by giving facilities each year to a few advanced students from the Museum and teachers in our public schools, to prepare, little by little, a small number of teachers who

will have had opportunities for pursuing their studies hitherto unattainable. The material to be obtained at Newport is abundant. The dredging is fair and not difficult, as the depth in the immediate neighbourhood does not exceed twenty to thirty fathoms. The pelagic fauna, however, is the most abundant. During the course of each summer, by the use of the dip-net, representatives of all the more interesting marine forms are sure to be found. With my small steam launch a large space can always be traversed any evening and advantage taken of the condition of the wind and tide, the launch being amply large for easy dredging in the moderate depths of the entrance of Narragansett Bay. The laboratory is placed on a point at the entrance of Newport Harbour, past which sweeps the body of water brought by each tide into Narragansett Bay and carrying with it everything which the prevailing southwesterly winds drive before it. Newport Island and the neighbouring shores form the only rocky district in the long stretch of sandy beaches extending southward from Cape Cod—an oasis, as it were, for the abundant development of marine life along its shores.

BIOLOGICAL NOTES

Caspian Sea Algæ.—Herr A. Grunow has quite recently published a detailed catalogue of a collection of algæ, made by himself at Baku and Krasnowodsk, on the Caspian, and also of some collections made by his friend Czermak in Baku Bay and by Thieme in Krasnowodsk Gulf, in addition some specimens preserved in spirits were given him by Dr. Schneider. Excluding the diatoms only eleven species are alluded to, and but two (Cladophora) appear as new. Of the diatoms there is a goodly list. Many of the species of these diatoms appear to occur everywhere. Go where one will, they are to be found, and what a marvellous geographical distribution!—Baku on the Caspian, St. Paul's Island in the Southern Ocean, and then the Frith of Clyde, or the mouth of the Thames. Two beautiful plates representing the new species of diatoms accompany the paper. Many of the species are marine forms.

Natural History of the Caucasus.—A very important contribution to the natural history of this region has been made by Dr. Oscar Schneider based on collections made by himself during a summer spent there in 1875. The series of memoirs before us, edited by Dr. O. Schneider, has been reprinted from the *Journal* of the "Isis" Society of Dresden, and consists of an account of the mollusca, by the editor; the arachnoids, by Dr. L. Koch, many new species are figured; the hemiptera, by Dr. G. v. Horvath; the algæ, by Dr. A. Grunow, a memoir we have already noticed; the minerals, by Dr. A. Frenzel; the rocks, by Dr. Moehl; the fossils, by Dr. Geinitz. These reprints form a small volume of 160 pages with five plates.

On Sprouting in Isoetes.—K. Goebel records in some detail and with illustrative figures the fact that he has found buds developed from the base of the leaves below the lingule in *Isoetes lacustris*. The specimens were collected in Longemer Lake in the Vosges, and the discovery was made during an investigation into the embryology of both *I. lacustris* and *I. echinospora*. The examples in question showed neither macro- nor microsporangia, but in their place were found on the leaves little Isoetes plants. The first appearance of the buds was under the lingule in the furrow of the still young leaves. A pretty compact swelling made its appearance on the under half of the glossopodium. This swelling was the commencement of a conical protrusion of the cellular tissue, in which a side cell did not take any leading part; later on this swelling appeared to be more rounded off; the stages between this and that in which one to two leaves were found, was not specially observed. A section through the young bud shows that the median plane of the young leaves is precisely that of the mother leaves, and they lie so tightly packed together that the lingule of the first new leaf is parallel with the surface of the mother leaf. The root formation of these buds appears to be quite normal. Some of the leaves only gave rise to these buds. The author thinks this is an instance of De Bary's apogamy. Interesting and novel as these observations of Goebel are, they yet leave a good deal to be desired (*Bot. Zeitung*, i., 1879).

The Brittle Stars of the *Challenger*.—In order that persons who are interested in echinoderms may get early information, and to secure a just priority of discovery to the *Challenger* expedition, Mr. Th. Lyman has just published, as No. 7, vol. v. of the *Bulletin* of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass., a Part I. of a catalogue of the new species found, which contains brief diagnoses, with figures, of the more essential parts of no less than thirteen new genera, and ninety-six new species of Ophiuroids. Part II. will contain some remaining species of the family Ophiuridæ, and those of Astrophytidæ. All matter beyond the mere necessary description is reserved for the volume to be devoted to this group, and which is to be brought out by the British government under the general superintendence of Prof. Sir Wyville Thomson.

Spines of Echini.—The last published part of the *Transactions* of the Royal Irish Academy (vol. xxvi., Science, part 17) contains a memoir by H. W. Mackintosh on the structure of the spines in the sub-order of the Desmosticha (Hæckel). In indicating four series into which, judging from the structure of the spines, this suborder may be divided, the author expresses his opinion that the characters derived from the spines are just as useful as any other characters drawn from the comparison of individual parts. He finds it just as easy and as certain to recognise a Diadema, an Echinus, or an Arbacia by the structure of its spines as by the arrangement of its pores, or the disposition of its anal or genital plates. The memoir is accompanied by three plates containing twenty-seven figures, all drawn by the author with the assistance of a Wollaston's camera lucida. The figures represent transverse sections of primary inter-ambulacral spines of some twenty-six species, and have been drawn on stone by Tuffen West with great care and accuracy.

The Food of Fishes.—Mr. S. A. Forbes publishes a very interesting paper on the food of fishes in the *Bulletin*, No. 2, of the Illinois State Laboratory. The importance of the subject to the scientific student and to the practical fish-breeder cannot be doubted. Some valuable fishes are found dependent on food too liable to injury or destruction by man or nature to make it worth while to cultivate them, while others, equally valuable, may subsist on food absolutely indestructible. The contents of the stomachs of some fifty-four species of Illinois fish were carefully examined, and the details of the food found are in each case given. In some instances the enormous quantity of food devoured, especially in insect-feeders, is noteworthy, and much of the food consisted of land-insects which had fallen into the water, thus bringing fish and land birds into competition for food. Some of the species were herbivorous, others carnivorous, and several, such as the cat-fishes, were quite omnivorous; the dog-fish (*Amia calva*) was herbivorous, but only one small specimen was examined. The shovel-fish (*Polyodon folium*), supposed by the fishermen to live on the slime and mud of the river-bottom, was found to feed to an enormous extent on Entomostraca, and fully one-fourth of the entire food was made up of vegetable matter, algæ being largely eaten, and there was very little mud found mixed with the food. The interlacing of the gill-rakers of this species, which are very numerous and fine, and arranged in a double row on each gill arch, doubtless form a strainer which allows the passage of the fine silt of the river out with the water, but arrests everything as large as a cyclops.

THE ELECTRICITY OF THE TORPEDO[1]

II.

4. *CURRENTS induced by a torpedo discharge are all produced at the beginning of each wave. There are currents induced on the completion of a circuit,* i.e., *the inverse of the inducting currents, as is shown by the electrometer.*

Fig. 7 will show the arrangement of the experiment to prove that the torpedo's discharge in the inductive coil

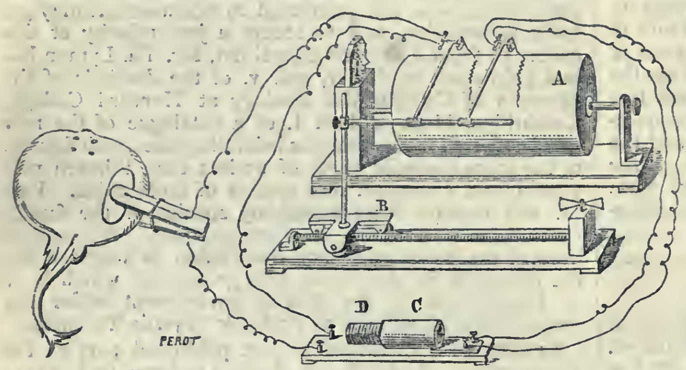


FIG. 7.

(D) produces, in the secondary coil (C) currents of sufficient energy to cause movement in the second signal placed in the circuit of the inducted coil. It must be remarked, however, that the electric apparatus which furnishes the indications of the passage of the discharge is not, as shown in the diagram, that which produces the inducted currents. Currents of sufficient intensity would not have been obtained to act upon the electro-magnetic signal, if we had opposed to the passage of the discharge a resistance so considerable as that of the coils of the electro-magnet. The difficulty has been averted by using the opposing apparatus for signalling; and this we are authorised in doing, since the discharges are absolutely symmetrical to the right and left when the nervous centres are excited by the magnet.

That exception made, let us examine the results of the experiment. The traces were placed one over another; one (as 1 in Fig. 8) indicating the successive waves of the discharge, and the other (as 2 in Fig. 8) the currents inducted by the waves. This figure already shows an important fact: that the number of inducted currents is equal to that of the inducting waves; and that each inducted current is produced at the commencement of each wave, just as in a galvanic current an inducted current is produced at each completion. But here we only find currents inducted by the completion or, more exactly, at the commencement of the waves; none are produced during the decreasing phase of each wave, or at least if they are produced they do not act upon the electro-magnetic signal.

From the preceding it should be inferred that the currents, inducted by the torpedo-waves and produced at the commencement of these waves, must in that be analogous to the inducted currents resulting from the completion of a galvanic circuit.

No instrument could be better than Lippmann's electrometer for giving us information as to the direction of the currents inducted by the torpedo waves. Its instantaneous action enables it to indicate, by a sudden displacement in a determinate direction, the direction of each inducted current.

If a weak current derived from the main discharge is passed through the electrometer, we see the column after moving to one side of the reticule always oscillate to the same side, thus showing that the successive waves are

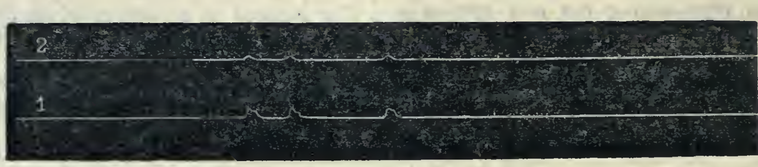


FIG. 8.

joined one to another, so that there is never an absolute break in the current. In Fig. 9 the arrows show the oscillation of the column towards the right side.

If the induced currents are directed through an electrometer by the discharge which causes the column of the instrument to deviate to the right, the direction of oscillation is immediately reversed (Fig. 10).

The comparison of those two diagrams showing that the inducted current has a contrary effect to that of the inducting current, brings together the inducted currents of the torpedo and those obtained by completing a galvanic circuit. To be more exact, since the circuit which comprises the torpedo remains always complete, we shall say that the inducted currents are produced at the beginning of each electric wave of the animal. Thus the torpedo calls forth in each of its electric waves an initial inducted current and does not give a terminal inducted current. This conclusion goes to support what we learned from the wave-writing of the electro-dynamograph, viz., that the initial phase of each wave has a suddenly increasing intensity, whereas the terminal phase presents a gradual decrease.

5. *The discharge of the torpedo is analogous to muscular tetanus; every electric wave in the discharge corresponds to a muscular shock.*

In what precedes we have endeavoured to give an idea of the torpedo discharge from the nature of the successive acts which constitute it, insisting only upon points relating to experimental science and to the direct results of M. Marey's investigations.

We are now enabled to meet the question on higher

FIG. 9.

FIG. 10.

ground and consider the electric function from a philosophical point of view, by classing it with the muscular function.

Let us first compare the elementary action of electric discharge to the elementary action of muscular contraction, *i.e.*, the electric wave to the muscular shock.

[1] Continued from p. 297.

The simple excitation of the remote end of an electric nerve produces a single wave, as the simple excitation of a motor nerve produces a single shock. In both cases, at the moment of the nerve-excitation produced in the neighbourhood of the electric apparatus or of the muscular apparatus, the amount of delay is sensibly the same, about seven-hundredths of a second. The electric wave, like the muscular shock, has a phase of increase and a phase of decrease; the former, as we have seen, is abrupt or sudden from one part to another; the decreasing phase is much more gentle. The same agents modify the wave and shock in the same manner; heat renders both these actions more speedy and more energetic up to a certain point at which both electric reaction and muscular reaction disappear; cold acts equally upon movement and electric action, rendering both more slow, more feeble, more extended, and at last extinguishes them when the temperature is lowered to about zero C.

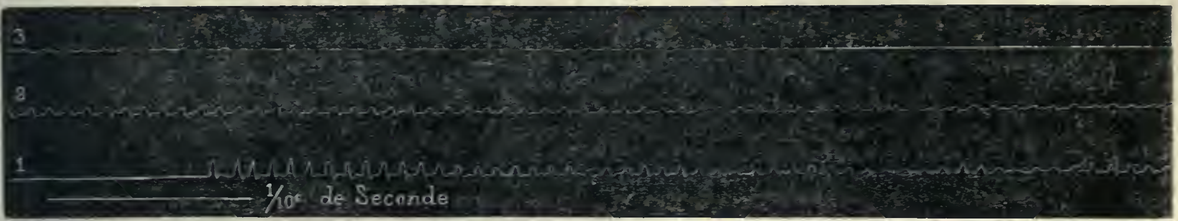


FIG. 11.

In the complex muscular act which is called contraction, as in the electric act which constitutes the discharge, the elementary phenomena which we have just been comparing, the waves and the shocks are added on one to another in the same manner; they succeed each other with a rapidity so great that each has not time to complete all the phases before its successor is produced. The floor and the shock are interrupted during their decreasing period by a new wave or shock coming to join on its effect to what remained of the preceding act. But, as there are imperfect muscular contractions, cases of tetanus where the shocks are not completely fused together, not being rapid enough in their succession, so in the same way certain electric discharges present a remarkable discontinuity, such that the elements of the perfect act are seen arranging themselves, the waves following each other with less rapidity, the shocks separated from each other by a larger interval of time.

FIG. 12.

Let us now compare the effects of fatigue upon muscular contraction and upon the discharge, as we have compared them upon the muscular shock and upon the electric wave; we shall see the two acts modified in the same direction. It is even possible to see the torpedo-wave and discharge gradually becoming extinguished, just as muscular shocks disappear under the influence of exhaustion. This gradual extinction of the electric waves is very evident in Fig. 11, obtained by means of the electro-dynamograph.

Poisons which act directly or indirectly upon the muscular function modify in the same manner the electric function. Thus strychnine, for example, which in a very special manner exaggerates the excito-motor power and that which might be termed the excito-electric power of nervous centres; a complex reaction, a muscular tetanus on one side and a real electric tetanus on the other, is produced in reply to a simple excitation, the mere touch of the skin, or a slight noise.

Fig. 12 shows a type of muscular strychnine contraction in the frog. Here we observe a diminution of intensity produced in the middle of the muscular tetanus between two maxima, one at the beginning and the other at the close.

In agreement with this characteristic phenomenon, the cause of which is unknown, we observe in torpedo poisoning by strychnine a weakening or interruption towards the middle of the discharge. Fig. 13 shows on line A a type of this species of interruption which must be compared with that which we have just seen on the tracing of muscular tetanus.

We might still further extend the comparison of those two functions, the electric and the muscular, by studying the action of other fishes, such as the *Gymnotus*, the electric ray, &c.; and by showing that the results are identical when heat and cold act upon muscular contraction and upon the discharge of the torpedo. The preceding paragraphs are sufficient to justify the functional assimilation which, let us hasten to say, is in accordance with the anatomical assimilation.

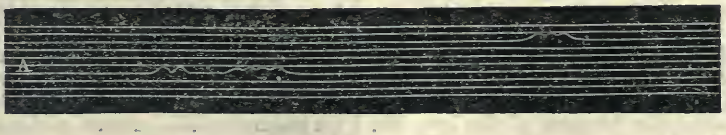


FIG. 13.

We shall only add that these identifications are of higher interest than curiosity; the more our knowledge of muscular phenomena and electric phenomena becomes perfected, the more enlightened is our knowledge of the motor nerves. Does the fact that a voluntary discharge of the torpedo is a complex act not prove that the voluntary contraction of the muscles is also a complex act? Very certainly, the comparison of the voluntary contraction of the muscles with the tetanic phenomena produced by electricity or by strychnine, the existence of a muscular sound during the contraction, the quivering or

dissociation of the shocks which are produced under the influence of cold, all these seem arguments in favour of the theory which considers muscular contraction as the result of very frequent shocks; but the complexity of the voluntary discharge of the torpedo, the manner in which the waves composing it succeed each other and are added together, forms a very important confirmation of the numerous presumptions already made.

FRANCOIS FRANCK

SCIENCE IN LANCASHIRE AND CHESHIRE

AT Liverpool the annual associated *soirée* of the Literary, Scientific, and Art Societies, eighteen in number, of which twelve are scientific, held at St. George's Hall on the 31st, was a marked success, and will tend much not only to foster scientific tastes in this district, but inculcate an element of scientific co-operation, in the various institutions of the town, that will be of the highest practical value. The fourteenth Winter Course of Free Lectures for the People, given at the Free Library and Museum, by order of the Corporation, commenced on the 6th of last month. Amongst the forty-one lectures announced, thirteen are on scientific subjects, given by the Rev. W. H. Dallinger, Mr. Moore (Curator), Mr. De Rance, Rev. H. H. Higgins, and others. The Liverpool Geological Society is also doing good work; a valuable paper on the carboniferous limestone of Denbighshire was lately read by Mr. Morton, and a short but important mineralogical paper was given by the President, Mr. Semmons. Geological knowledge has been increased by a boring at Bootle, sunk to determine the water-bearing properties of the new red sandstone at great depths, by Messrs. Mather and Platt, for the Liverpool Corporation, who were urged to this course by Alderman Bennett. The boring has reached a depth of 1,300 feet, is 25 inches in diameter, is filled with water up to a height of 50 feet from the surface, and, according to Messrs. De Rance and Morton, has proved the pebble beds of the Bunter to reach the extraordinary thickness of 1,200 feet, and the existence of the lower mottled sandstone beneath. The pumps not yet being fixed, it is impossible to judge how far the well will add to the supply of 6,000,000 gallons a day at present pumped from the corporation wells.

At Wigan, in addition to the ordinary course of lectures given at the Mining and Mechanical School, a special course has been arranged for candidates for colliery managers' certificates, and gives to the teaching of the school a special technical direction. The extension scheme for turning the very numerously attended evening classes of this school into a Mining Collegiate Centre for Lancashire, has necessarily languished under the unexampled and continued depression in the coal trade, though from the number and extent of the promised subscriptions and donations to the building-fund, there can be little doubt that, when better times again visit this country, this school will develop into an important centre of technical education. The town has lately had the good fortune to have presented to it a magnificent library, stored in a handsome building erected for the purpose, the former being the bequest of the late Mr. Winnard, the latter the gift of Mr. Thomas Taylor. The reference library is well stored with standard scientific works in all branches, and the selection reflects great credit on the industry and acumen of Mr. Gerrard Finch, barrister-at-law, who selected them, under the terms of the will. Some important works are of course conspicuous by their absence, but doubtless this will soon be remedied. The reference portion of this library will henceforth be opened on a Sunday to readers who have asked for special tickets, the extra cost of assistants being defrayed for three years by Mr. Taylor, the donor of the building. This town has also now a flourishing Literary and Scientific Society, with Lord Lindsay as president; it is divided into botanical, microscopical, and other sections, at which papers are read by the members, and discussed, and, in addition, special lectures are given to the united sections; amongst those delivered have been "Spectrum Analysis," by Lord Lindsay, and "Local Geology," by Mr. de Rance: others are announced by Prof. McKenny Hughes and Prof. Rudler.

At Southport there is at present little done for fostering a taste for either technical or scientific education, but the very fine aquarium is maintained in great efficiency, the contents of the table tanks, to which we have previously referred, being especially beautiful.

At Preston, meetings of the Scientific Society have been numerously attended, and the president, Dr. Arminson, and others, do good microscopical work. The meetings are held at the Avenham Institution, which is well filled with scientific works, including the natural history library of a defunct Naturalists' Society, and it is a matter of regret that the town, in adopting the Free Libraries Act, should not have carried out an amalgamation scheme, instead of running a new and inefficient library in opposition to the existing useful and self-supporting institution. The Gilchrist Trust lecturers here and at Burnley have been listened to with much interest by large audiences; and at the latter place Prof. Boyd Dawkins has inoculated his hearers with his taste for cave-hunting investigations, and searches have been organised into the wild hills which fringe the county boundary of Lancashire and Yorkshire, and form the backbone of England, a district which for the most part appears to be above the level of the glacial sea deposits.

At Chester the flourishing Natural Science Society that looks back with pride to Kingsley, its founder, and forward with hope to its president, Prof. McKenny Hughes, is divided into several sections like a small British Association. The most noticeable paper read was one by Mr. Shrubsole, on the Fenestellæ of the carboniferous limestone, which was an important and valuable contribution to our knowledge of this group, proving from perfect specimens that several supposed species are, in fact, portions of the same organism. General lectures have been given to the united sections by Prof. Judd and Mr. De Rance, who opened the winter session. The Society possesses a very good local museum, but unfortunately it is exhibited in the disused ball-room of an ancient hotel in an out-of-the-way part of the city, and is known to few of the scientific visitors of Chester, and is but seldom visited by the inhabitants. The collection would, however, form an admirable nucleus of a museum for teaching purposes, should the Corporation ever recognise the need of technical education in this town, and erect a building to hold a library, museum, and science and art schools by the side of their fine town hall. Towards filling museums of this class, great advantage would accrue if the duplicate specimens at the British and Jermyn Street Museums were either given, or allowed to circulate, in the same manner as the art treasures from South Kensington Museum. For valuable as are *local* collections for the scientific *specialists*, no one can doubt the importance of giving wide and varied knowledge to the general public, such as, perhaps, can only be imparted by the inspection of typical specimens of the natural and artificial products of all countries. Such a collection may be seen on a small scale in the admirable little museum at Castleton, in Derbyshire, formed by Mr. Rooke Pennington, which at once furnishes the visitor with all that can be collected for forming a mental local picture of the past, and affords the inhabitants of the district an opportunity of knowing something of the world around them.

At Manchester the museum at Owens College now includes the entire collections of the Manchester Natural

History Society and Manchester Geological Society, as well as a small typical collection originally belonging to the College and the complete mineralogical and metallurgical collections of the late David Forbes; the series are admirably arranged for the purposes of study. Prof. Boyd Dawkins is curator.

A REAL TELEGRAPH

A NEW invention of a real practical character, not a mere "paulo post futurum" invention like many we have heard of lately, has just been made by Mr. E. A. Cowper, the well-known mechanical engineer. It is a real telegraphic writing machine. The writer in London moves his pen, and simultaneously at Brighton another pen is moved, as though by a phantom hand, in precisely similar curves and motions. The writer writes in London, the ink marks in Brighton. We have seen this instrument at work, and its marvels are quite as startling as those of the telephone. The pen at the receiving end has all the appearance of being guided by a spirit hand. The apparatus is shortly to be made public before the Society of Telegraph Engineers. We give a facsimile of the writing produced by this telegraphic writing machine.

GEOGRAPHICAL NOTES

THE Spanish Ministry of Public Instruction has just issued a very important publication, being a collection of letters of Christopher Columbus, and of his contemporaries, as well as of reports sent in, during the sixteenth century by governors of the new American provinces, the originals of these letters and reports being now in the State Archives of Spain. The work, which bears the title "Cartas de India" (Letters from India), and forms a large volume of 877 folio pages, contains the following highly interesting documents: (1) Two autograph letters from Columbus, written in 1502 to King Ferdinand and Queen Isabella, the first letter dwelling upon the necessity of measures for increasing the population of the island "Española" (San Domingo), and the second being a discussion on the art of navigation; (2) a letter from Amerigo Vespuchi to the Cardinal Gimenes di Cuneros, Archbishop of Toledo, dated Sevilla, 1508, and dealing with the merchandise to be sent to the Antilles; (3) two letters from Fra Bartholomeo de las Casas, Archbishop of Nicaragua, to the Infanta Don Filippo, dated Gracias a Dios, in Guatemala; (4) two letters from Bernaldo Diaz del Castillo, one of the warriors of the small army of Cortes, and author of a history of Mexico, to Charles the Fifth (1552), and to Philip the Second (1558); (5) letters from the baccalaureates Don Pedro de Gasca and Don Christophor Vaca de Castro, dated Quito, 1541 and Cusco (1542), announcing to Charles the Fifth the death of the Marchese Don Pizaro and the insurrection of Don Diego de Almagro; both letters are very interesting, being accounts of eye-witnesses; (6) a very interesting letter of Donna Isabella Quivara to the Regent, Donna Huana, about the remarkable courage displayed by women during the expedition of Cortes, when all male members of the expedition were exhausted by disease. The work contains 652 pages of text and 225 pages of appendix, in which we find twenty-nine autograph letters and reports of various important historical persons; twenty-one sheets of autographs of Columbus, Vespuchi, Las Casas, Diaz del Castillo, Gimenez, &c.; a map of the fortifications where the gems of the Incas were found; and maps of Australia, of the River Amazon, the Antilles Archipelago, and Magellan Strait, drawn in the sixteenth century. We are sure that all friends of historical geography will feel grateful to the Spanish Government for this valuable publication.

COL. PRJVALSKY has left St. Petersburg to make another trip in Central Asia. He will proceed by Orenburg, Omsk, and Semipalatinsk to the Chinese frontier, thence to Hami, Hansu and Lassa. From Lassa he intends to reach the Himalaya by the Brahmapootra. Returning then to Lassa, he will visit Khotan, Kashgar, and cross the intervening plateaux to Russian Khokand. The journey is to occupy two years.

PÈRE HORNER has addressed a letter, dated Zanzibar, December 12, to *Les Missions Catholiques*, announcing that the members of the German scientific expedition have returned in bad health. This expedition, under the command of Herr C. Denhardt, started from Melinda, and explored the course of the River Dana, which has hitherto been supposed to take its rise in the slopes of Mount Kenia. They experienced many difficulties, and did not succeed in getting more than about sixty miles into the interior. According to Père Horner's report, in the place of Mount Kenia, covered with snow, they found only plains, and though they questioned more than 200 persons on the subject, they could find no one who had heard of the mountain. Père Horner thinks that the German travellers have not been far enough into the interior, and he says that they are going back again shortly to solve the problem. The truth, however, we believe is that Herr Denhardt has found that the Dana has a widely different course from that generally assigned to it, and that it does not flow anywhere near Mount Kenia. The party obtained a very complete series of meteorological observations, and they report that at some distance from the coast they met with a quiet and industrious tribe called the Vakopomo, who, it is thought, would welcome missionaries among them, if the fanatic Somalis of the coast region would allow them to pass.

IN the February number of the Geographical Society's new periodical we find a curious and learned paper by Sir Henry Rawlinson, entitled "Rough Notes on Prehistoric Cyprus, and another on the "Upper Basin of the Kabul River," from Mr. Markham's versatile pen, accompanied by a carefully drawn map of the Hindu Kush. Some of the geographical notes are of considerable interest. A St. Petersburg correspondent reports finding Prjvalsky restored to his habitual vigorous state of health and busy preparing for his next expedition, in which he hopes to reach Lhassa by the Hami and Sha-chau road, accompanied by young Eklon. Prjvalsky's Lob-Nor plant-collection turns out to be not very rich, the number of species being exceedingly few. Under the heading "Explorations North of India," we have the leading features of an unpublished report by Capt. Woodthorpe, R.E., and Lieut. Harman, of their recent work in the unexplored Miri and Mishmi Hills. In the latter region Capt. Woodthorpe obtained a fairly accurate knowledge of the sources of the Dihong River and the course of its main stream in the hills. The result of these explorations proves that the volum of the Subansiri is only one-fourth of that of the Dihong, which tends more than ever to identify the latter with the great river of Thibet. Some particulars are also furnished respecting the Dar-es-Salaam road in East Africa.

AT the last meeting of the Berlin Geographical Society several interesting communications were read. Gerhard Rohlfs had left Tripolis about Christmas, and proceeded southwards for twelve days' journey, there awaiting the presents intended for the Sultan of Wadai, sent by the German Emperor. The Society has also received news from Count Szechenyi, who has undertaken the task of investigating the Lo-floa. He arrived at Pekin in October last, and succeeded in obtaining passes for his journey to Thibet, a favour which has never before been bestowed upon any European by the Celestials.

HERR J. M. HILDEBRANDT is about to start on a scientific tour through Madagascar, at the request of the Berlin Academy of Sciences. He will direct his principal attentions to the botany, zoology, and topography of the island. A report has reached Bremen through the French Secretary for the Navy, that the young Bremen traveller, Dr. Rutenberg, who is also investigating Madagascar, has been murdered there. Direct news from Nossi-Bé, however, states that Dr. Rutenberg stayed there till November 29 last. He was then about to start on a tour through the interior in a southerly direction, and hoped eventually to reach Fort Dauphin, near the southern extremity of the island.

THE Paris Geographical Society intend holding a meeting in commemoration of Capt. Cook on the 14th inst., that being the hundredth anniversary of his death. M. W. Hüber, Dr. E. H. T. Hamy, and M. Crosnier de Varigny have agreed to address the meeting respectively on Capt. Cook's life, the ethnographical results of his voyages, and the present state of the countries discovered by him. Mr. J. Jackson has taken charge of the bibliographical researches, and Mr. W. Blakeney, R.N., secretary to the hydrographer, has been requested to lend the assistance of his knowledge concerning the great voyager. The Royal Geographical Society have been invited to be represented at the meeting. It seems strange that it should be left to a foreign society to commemorate the death of England's greatest scientific voyager.

A PHENOMENON causing much anxiety amongst the inhabitants of the shores of the Amazon is the continued rapid decrease of that generally colossal river. It appears that navigation above Manaos has become an impossibility. The cause of the continued decrease of the quantity of water is entirely unknown at present, and it is most desirable that men of science should thoroughly investigate the matter.

THE "Berlin Central Union for Commercial Geography and for the Furthering of German Interests Abroad" has just issued its first publication under the title "Geographische Nachrichten für Welthandel and Volkswirthschaft;" the editor is Dr. O. Kersten, and the publishers Herren Puttkammer and Mühlbrecht of Berlin.

A SINGAPORE paper states that the suitability of the soil of Perak for planting enterprise is exciting more and more attention. Five or six more planters from Ceylon are now there engaged in examining the soil. A similar remark applies to Johore, where the Maharajah has invited an ex-planter in Ceylon to become a sort of Minister of Lands; he is expected to arrive early this year, accompanied by several planters, who intend to explore Johore.

THE position of the missionary settlement at Blantyre which lies to the east of the Murchison Falls on the Shiré, East Africa, has never hitherto been known within several miles. It will, therefore, be interesting to record that as the result of a series of observations taken some five months since, Mr. Louis Carr has determined it to be in S. lat. 15° 45′ 25″, and E. long. 35° 14′ 11″.

ENGINEERING RESEARCH

THE Institution of Mechanical Engineers have decided to take a step which must meet with the approval of all who have the progress of engineering at heart. Like all other arts, that of the engineer, to be fully efficient, must be based on the laws which govern matter; and it is evident that the better we know these laws the more efficiently will our engineers be able to do their work. The only method of discovering these laws and their action under all the conditions with which engineering has to deal, is by systematic and thorough research; and since in this country our Government are so slow to see its true interests, the work, as far as possible, must be undertaken by individuals and bodies such as the Society of Mechanical Engineers. That Society is to be congratulated on its enterprise and the clear perception possessed by its members of the foundation on which their all-important art should be established. At a recent meeting of the Society it was resolved that the Council should be empowered to expend during the present year a sum of not more than 300*l.* "for the purpose of promoting practical research in mechanical subjects." What the nature and aims of this research are likely to be we learn from a circular which has been distributed among the members for the purpose of eliciting suggestions.

It is proposed that a Research Committee, consisting of five Members of Council, be appointed by the Council annually, and that a sum be voted at each annual general meeting to be expended by the Committee. The first duty of this Committee, when appointed, will be to prepare a list of subjects on which further research is desirable, and present it to the Council, recommending certain subjects to be first investigated. The Research Committee will then appoint a Sub-Committee for each of the selected subjects, and invite gentlemen (not necessarily members of the Institution), to give assistance to such Sub-Committees. This proposed condition we think exceedingly praiseworthy and liberal, and augurs well for the comprehensiveness and thoroughness of any research that may be undertaken.

The circular referred to proposes that each Sub-Committee be instructed that its first duty is to collect and collate all the records of experiments and other information already existing on the subject; then to determine what further experiments, if any, are needed, and ascertain their probable cost; and to present a report to the Research Committee, embodying a summary of the information so obtained, a description of the experiments proposed to be made, and an application for the requisite funds. Upon the approval of any report of a Sub-Committee by the Research Committee, the latter, it is proposed, will apply to the Council for a suitable sum for the use of the Sub-Committee in carrying out their investigations.

Then it is proposed that ample provision be made for the publication of the results of any research, and for the continuation of investigations as far as circumstances seem to demand. The whole scheme seems to us to be conceived in a thoroughly liberal spirit, and with a true idea of the value of scientific research, and of the conditions under which it can be carried out with efficiency. That the scheme is likely to be carried out in as intelligent a manner as it has been devised will be evident from the names of those who have been appointed in the first instance as a Research Committee. These are: Dr. Siemens, Mr. Wm. Anderson of Erith, Mr. E. A. Cowper, Mr. A. Paget, and Mr. F. W. Webb. "The names of these gentlemen," as the *Engineer* rightly remarks, 'are a sufficient guarantee that the work will be carried out both with energy and discretion; and we can only conclude by wishing them success in their labours."

NOTES

THE Council of the Royal Society of Edinburgh has recommended for the four vacancies in their list of Foreign Honorary Members the names of Donders of Utrecht, Asa Gray of the United States, Janssen of Paris, and Listing of Göttingen.

AT the meeting of the Royal Society on Monday last the Keith Prize was presented to Prof. Heddle of St. Andrew's for his papers on the Rhombohedral Carbonates and the Felspars of Scotland.

OUR readers will be glad to hear the latest news from Madeira, that Prof. Clifford is certainly better, and able to be carried out in the sunshine.

THE following are the lecture-arrangements at the Royal College of Surgeons for the present season:—Prof. Parker commences a series on Monday, "On the Evolution of the Vertebrata," to be continued on Mondays, Wednesdays, and Fridays, to March 3. On the same days of the week, from March 5 to 24, Prof. Flower will lecture "On the Comparative Anatomy of Man," in continuation of his course of last year. In June, Prof. Jonathan Hutchinson will give six lectures: "On Certain Diseases of the Eye, Skin, and Joints which are produced through the Influence of the Nervous System;" and in the same month Mr. B. T. Lowne, F.R.C.S., will give three lectures "On the Physiology of the Nervous System," in continuation of his course of last year.

AT the General Monthly Meeting of the Royal Institution of Great Britain on Monday, Dr. Warren De La Rue, F.R.S., was elected Secretary of the Institution, and Dr. William Spottiswoode, Pres. R.S., was elected Manager.

WE learn from the *Journal de St. Pétersbourg* that the epidemic in Astrakan was discussed before the Russian Medical Society at a gathering where 800 were present. It seems that the people call it the plague, though it is not officially so known. M. Botkine mentioned that at the time of the last plague at Moscow in 1770, the question was discussed whether it was the true plague or a marked form of typhus, and he added that the diagnosis of the various forms of typhoid infection in Russia is very difficult. He believes that the spots on the body and the quickness with which death follows indicate that the present epidemic of Vetlianka is not a European malady. Dr. Nicolaïew, describing the symptomatology of the plague, said that its action is both physical and moral, and that to impose quarantine often helps rather than retards the spread of the disease by the fear it awakens.

ADMIRAL MOUCHEZ will soon resume, at the Paris Observatory, the series of *soirées scientifiques* which had been commenced by Leverrier. The first will be given at the end of the present month or the beginning of March. M. Wolf will lecture on astronomy.

THE second International Meteorological Congress will be opened at Rome on April 14 next. At the same time an exhibition of meteorological instruments will take place, and the Italian Government invites home and foreign institutions and private men of science to participate in the Congress.

WE regret to announce the death of Herr Georg Peter Winther, of Copenhagen, an eminent Danish naturalist, well known through his excellent treatises on the fishes of Denmark. He died on January 14 at the early age of thirty-five years.

A CELEBRATION of the fourth centenary of the introduction of the art of printing into Leipzig will take place during this year and will be coupled with an exhibition comprising all branches of the graphic arts.

THE little town of Hohenstein in the Erzgebirge will celebrate the centenary of one of its most celebrated sons on April 26, 1880. The eminent naturalist and philosopher, Gotthilf Heinrich von Schubert was born at Hohenstein, in 1780, and died at Munich on July 1, 1860. It is intended to erect a monument to his memory and to establish a school under the name of Schubert Institution.

A BOTANICAL society is in course of formation at Strassburg. Its object, apart from a special study of the botany of the Reichsland, is to provide all the higher schools of the country with complete herbaria.

WE hear that the coal-mining experiments at Kaiping in the north of the Chinese province of Chihli are proceeding successfully. The boring has reached a depth of nearly 500 feet, passing through six seams of good coal, one of which is three feet and another eight feet thick. It is proposed to bore to a depth of 550 feet.

A CORRESPONDENT asks us whether the "microphone electromagnetique," said to be invented by Dr. Frank, rue St. Honoré, Paris, is really a useful invention for deaf persons, or not? We have not yet heard of any microphone which in any way assists the deaf.

ON January 30, when the National Assembly of Versailles, was voting on a successor to Marshal MacMahon, M. Paul Bert, a representative of Yonne, was lecturing on Claude Bernard and his works, in the large hall of the Sorbonne, before more than 2,000 persons, belonging mostly to the high schools and learned professions. The only reference made to political matters by the lecturer related to the funeral of Claude Bernard, which took place at the expense of the Government. M. Paul Bert reminded his hearers that it was the first time such an honour had been paid to a man of science. Up to that time they had been exclusively reserved for men who had earned their reputation on the battle-field, or who belonged by blood to the reigning family.

IN his lecture on Claude Bernard, M. Paul Bert narrated a singular stratagem which was invented by Bernard during the last Franco-German war, and might be utilised without difficulty, under similar circumstances. It was proposed to re-victual Paris, which was strictly blockaded by German forces. A large number of cattle had been collected, waiting for an opportunity to cross the German lines. But a difficulty was to silence these animals, as their cries would attract the attention of the enemy. Claude Bernard proposed to practise upon them the section of the nerve which enables them to emit their usual cries. The operation is so easy that it could be executed in a few seconds by an ordinary butcher. None of the animals appeared to suffer in any way by the mutilation which had made them mute. But the military movement proved a failure, and for other causes the re-victualling could not take place.

SOME of our readers may be interested to know that there exists in Berlin an exceedingly efficient and comprehensive scientific agency, that of Friedländer und Sohn. Not only do they issue, at short intervals, catalogues of works and papers in all departments of science, published all over the world, but they undertake to assist individuals and associations in carrying out almost any scheme of a scientific kind. To any one, *e.g.*, anxious to pursue a particular line of research, they will furnish a methodical list of all the best researches that have been published on the subject; they assist museums, libraries, &c., in

forming collections of scientific specimens and books, and are, in short, the guides, philosophers, and friends of all desirous of accomplishing almost any purpose connected with science. Their "Bücher-Verzeichniss," No. 293 (Physics and Chemistry), is marvellously complete; nothing of any value, published in any country in any form, seems to have escaped the compiler. Friedländer und Sohn have been at this work for twenty-eight years, and their catalogues issued during that time must be of great interest and value to the student of science.

THE *Times* Geneva correspondent, under date February 3, telegraphs that a singular and almost unprecedented meteorological phenomenon has been observable during the past ten or fifteen days in many parts of Switzerland. While the temperature in the valleys and plains has been low, the waters covered with ice, and snow resting on the ground, a warm south wind has prevailed in the uplands and among the higher Alps, where the streams remain unfrozen and the snow has almost disappeared. This has been especially the case in Uri, Schwytz, the Grisons, Neuchatel, and the Bernese Oberland. Mr. Coolidge, an Englishman, with four guides, made the ascent of the great Schreckhorn last week at four o'clock in the afternoon, when the thermometer on the summit of the mountain marked several degrees above freezing-point. The Oberland Alpine Club propose to buy some of the ibex forming part of the collection of the late King Victor Emmanuel, for the purpose of re-stocking the mountains of Switzerland.

A SHOCK of earthquake was felt at Foochow and Amoy on December 17.

A FINE meteor was observed at Prague and many other towns and villages of Bohemia on January 11, at 7.30 P.M. It appeared in the north-western part of the sky and moved towards the south-west, disappearing with a loud report, and leaving a long luminous train behind. The colour of the meteor was white at first and reddish violet at the end; the duration of the phenomenon was ten seconds.

THE project of a canal between the Rhine and the Maas seems at last to approach realisation. The city of Crefeld has declared its readiness to pay the sum of 500,000 marks (25,000*l.*) towards it, and it is confidently hoped that now both the Prussian as well as the Dutch Government will grant the necessary additional funds.

WE believe that the changes in the Government of the French Republic will be favourable to the development of education all over the land. The extension of public instruction is to be a part of the programme of the Ministry, which will not be published before our present impression will be in the hands of our readers.

ANOTHER of the London gas companies has been trying to show what gas-lighting can be made if only the public are willing to go to the necessary expense. On Friday last the Gas Light and Coke Company lit up part of Regent Street in much the same way that the Phœnix Company recently did the Waterloo Road. The result is described as admirable. By the use of Sugg's improved form of burner, a light framework, and the proper adjustment of suitable reflectors, a light was obtained very much brighter than that to which we have been so long accustomed. We believe if some enterprising company undertook to light one of our principal thoroughfares for some months at their own expense by this method, they would most likely be rewarded by a demand on the part of the public that the new form of light should be made general and permanent. Some comparative experiments which have been made at Westgate-on-Sea with the Jablochkoff candles have led the experimenters to the conclusion that this form of electric lighting is much more expensive than gas, and is surrounded with so many difficulties that no amount of improvement is likely to fit it for adoption. It is rumoured that an experiment is likely to be made in lighting the reading-room of the British Museum with the electric light.

THE Austrian Tourist Club has offered two prizes of 100 and 50 florins respectively for the best and next-best monograph of a mountain group or single mountain from the district of the Austrian Alps. Particulars respecting the competition can be learnt upon application to the Committee of the Club, Gusshausstrasse, Vienna.

CONTINUING his researches on the scintillation of stars, M. Montigny has examined the influence of atmospheric temperature and pressure, moisture in the air, fogs, snow, different winds, &c. His observations are detailed in a recent number (11, of 1878) of the Belgian Academy's *Bulletin*. The general conclusion to which the various facts point is thus stated:—It is the presence of water in greater or less quantity in the atmosphere, that exerts the most marked influence on scintillation, and which most modifies the character of it, either when the water is dissolved as vapour in the air, or when it falls to the surface of the ground in the liquid state, or in the solid state, in the form of snow."

IN spectacles designed purely for amusement there occur from time to time exhibitions of muscular dexterity and strength which are highly interesting to the physiologist. *La Nature* mentions that there was lately to be seen at the Hippodrome, in Paris, a gymnast, named Joignerey, who discharged a piece of cannon, not supporting it on the shoulder, as others have done, but like a rifle. The same man, suspended by his legs from a trapeze, raised with his teeth a horse and its rider. About the same time visitors to the skating theatre were astounded by the feats of the juggler Treniz, who entwined himself in a long streamer wound as an aerial helix, a feat which has been peculiar to the Japanese; and, with cubes of wood thrown into the air and caught, sketched the rudiments of unstable architectural forms, modifying their arrangement with unfailing dexterity and certainty.

AT a recent meeting of the French Physical Society M. Benoit showed a thermo-regulator of his invention, based on the increase of maximum tension of a saturated vapour with the temperature. A small vessel, containing methylic ether, is placed in the stove whose temperature is to be kept constant; it communicates with a mercury manometer, the movements of which, again, serve to regulate the flow of the coal-gas which heats the stove. M. Benoit has thus been able to maintain a temperature of 85° C. constant to within one-tenth of a degree. The apparatus owes this rare precision to the smallness of its mass and the rapidity with which the tension of the vapour increases with the temperature. The author showed that after having regulated it for the surrounding temperature, one had merely to blow rapidly on the small vessel of liquid in order to produce the extinction of the gas-burners governed by the apparatus.

SEVERAL Parisian photographers have tried to use electric light for obtaining *clichés*, and have been wonderfully successful. MM. Pierre Petit and Lebert are the most prominent amongst them.

SCARCELY a month passes but we receive the first number of a new journal devoted to science. Last week we referred to a new Italian *Nature*, and we have before us several other journals which are at least new to us. *L'Athénæum Belge*, which has entered on its second year, devotes a portion of its space to science, as well as to literature and art; it seems to us to be well conducted. The first number of the second year of *Le Monde*

de la Science et de l'Industrie is extremely satisfactory, containing much and varied information both in pure and applied science. The *Telephone Journal*, of which also No. 1 of vol. ii. lies before us, we have seen for the first time. It seems to be an organ of the Chicago branch of the Bell Telephone Company, and contains mostly a list of persons and firms telephonically connected with each other through the Central Office in Chicago. The list of names is a long one, and as the "calls" of the Company are stated to average 5,500 daily, we infer they are doing a paying business. We have already referred to the Spanish *Crónica Cientifica*; Nos. 25 and 26 are exceedingly creditable, containing a fair selection from the scientific work being done both in Spain and in other countries. Altogether science has taken a prominent and influential place in the journalism of the day.

"THE Magic Lantern Manual," by Mr. W. J. Chadwick, is a plentifully illustrated little volume likely to be of great service to those, and they are many, who work with this useful apparatus in one or other of its many forms. Warne and Co. are the publishers. Equally useful in its own department is Dr. Sylvester Marsh's little manual on Section Cutting, a practical guide to the preparing and mounting of sections for the microscope, special prominence being given to the subject of animal sections. Messrs. Churchill are the publishers.

SOME excavations made at Merten, near Bolchen, in German Lorraine, have given remarkable results. The remains of a gigantic equestrian statue were found, of which the figure of the rider is particularly well preserved. Investigation of other remains tend to show that the origin of the statue is Roman; parts of mosaic floors, &c., have also been discovered.

A NEW agricultural school is about to be established at Meissen, Saxony.

THE cultivation and consumption of opium continues to increase largely in China, but notwitstanding this extended cultivation, the Persian drug is extensively consumed on account of its comparative cheapness. In a report from Amoy it is stated that the poppy is cultivated in the neighbourhood with the knowledge and sanction of the mandarins; but so far the production of opium appears in no way to affect the foreign produce, as, from the imperfect system of manufacture practised by the natives, they are unable to produce a drug in any way approaching the foreign article, either in quality or flavour. As regards the habit of using opium, Mr. Alabaster says: "It is now so general that I assume there is little probability of much increase in the demand unless the population of Formosa increase, where, as the use of opium is almost a necessity of life in the plantations there, to counteract the malarious influences of the climate there must be a larger export thither. Nor is it to be desired that the consumption should become greater, for although I cannot agree with those who so vigorously denounce the trade as a source of every evil, and am inclined to think from observation that many more lives are annually saved by its moderate use than are sacrificed to inordinate indulgence in it, an increase would now rather mark the spread of the abuse of the drug, than of its employment as a stimulant to counteract the lowering effects of climate, and damp and ill-drained houses."

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented respectively by Mr. E. E. Barclay and Mr. Eardley Holt; a Weeper Capuchin (*Cebus capucinus*) from South America, presented by Mr. W. Fridrick; two White-Fronted Capuchins (*Cebus hypoleucus*) from South America, presented by Mr. Geo. Backhouse; a Short-Tailed Wallaby (*Halmaturus brachyurus*) from West Australia, presented by Mr. G. Bowen; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. B. Baverstock; a Golden-Naped Amazon (*Chrysotis auripalliata*) from South America, presented by Mrs. H. A. Hopkins; three Canada Geese (*Bernicla canadensis*) from North America, presented by Mr. W. Bonorton; a Black-Winged Pea-fowl (*Pavo nigripennis*) from Cochin China, presented by the Hon. A. S. G. Canning, F.Z.S.; a Giraffe (*Camelopardalis giraffa*) from Nubia, deposited; a Golden-Fronted Parrakeet (*Brotogerys tuipara*), an American Tantalus (*Tantalus loculator*), a Rough Terrapin (*Clemmys punctularia*) from South America, four River Jack Vipers (*Vipera rhinoceros*) from West Africa, received in exchange.

FOREST GEOGRAPHY

SOME months ago Prof. Asa Gray delivered to the Harvard University Natural History Society a lecture on Forest Geography and Archæology, which has been published in two recent numbers of the *American Journal of Science*. The lecture referred mainly to the forests of North America, and in speaking of these, Prof. Gray referred to them not exactly as they are to-day, but as they were before civilised man had materially interfered with them. In the first part of the lecture Prof. Gray showed how the distribution of forests is mostly dependent on the distribution of moisture, and thus explained the great difference which exists in this feature between the eastern and western States. The Atlantic "forest primeval," he stated, a few generations ago covered essentially the whole country from the Gulf of St. Lawrence and Canada to Florida and Texas, and from the Atlantic to beyond the Mississippi. This Atlantic forest of the United States is one of the largest and almost the richest of the temperate forests of the world. Then going westwards from the Mississippi come prairies and open plains; beyond these is the Rocky Mountains, forest again, but only in narrow lines and patches; but after passing the Sierra Nevada, the western rim of the basin, we come to what is in some respects the noblest and most remarkable forest in the world. In the long valley of California it almost disappears again, to resume its sway in the Coast Ranges, with altered features, some of them not less magnificent and of greater beauty. Thus there are two forest-regions in North America—an Atlantic and a Pacific, each dependent on the oceans which they respectively border. Prof. Gray then goes on to show how the distribution and nature of these forests are dependent mainly on moisture and temperature, proceeding to prove that the difference in the composition of the Atlantic and Pacific forests is not less marked than that of the climate and geographical configuration to which the two are respectively adapted.

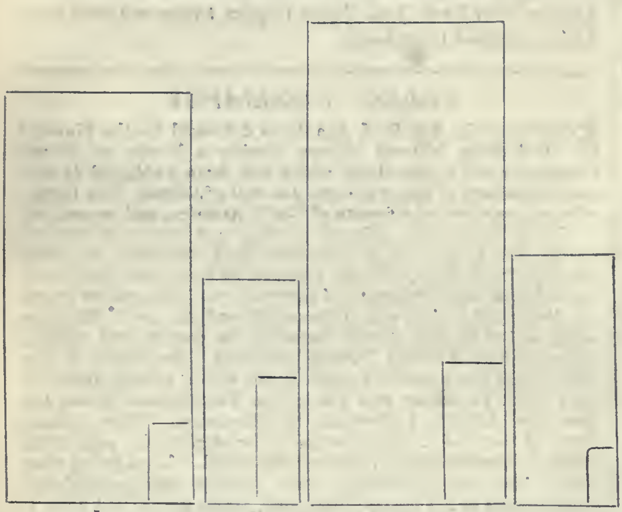
"With some very notable exceptions, the forests of the whole northern hemisphere in the temperate zone (those that we are concerned with) are mainly made up of the same or similar *kinds*. Not of the same species; for rarely do identical trees occur in any two or more widely separated regions. But all round the world in our zone, the woods contain *pines* and *firs* and *larches*, *cypresses* and *junipers*, *oaks* and *birches*, *willows* and *poplars*, *maples* and *ashes*, and the like. Yet with all these family likenesses throughout, each region has some peculiar features, some trees by which the country may at once be distinguished."

With regard to the Pacific forests the greater part of the Atlantic trees are conspicuous by their absence.

"For example, it has no magnolias, no tulip-tree, no papaw, no linden or basswood, and is very poor in maples; no locust-trees—neither flowering locust nor honey locust—nor any leguminous tree; no cherry large enough for a timber-tree, like our wild black cherry; no gum-trees (*Nyssa* nor *Liquidambar*), no sorrel-tree, nor kalmia; no persimmon, or bumelia; not a holly; only one ash that may be called a timber-tree; no catalpa, or sassafras; not a single elm, nor hackberry; not a mulberry, nor planer-tree, nor maclura; not a hickory, nor a beech, nor a true chestnut, nor a hornbeam; barely one birch tree, and that only far north, where the differences are less striking. But as to coniferous trees, the only missing type is our bald cypress, the so-called cypress of our southern swamps, and that deficiency is made up by other things. But as to ordinary trees, if you ask what takes the place in Oregon and California of all these missing kinds, which are familiar on our side of the continent, I must answer, nothing, or nearly nothing. There is the *Madroña* (arbutus) instead of our kalmia (both really trees in some places); and there is the California laurel

instead of our southern red bay tree. Nor in any of the genera common to the two does the Pacific forest equal the Atlantic in species. It has not half as many maples, nor ashes, nor poplars, nor walnuts, nor birches, and those it has are of smaller size and inferior quality; it has not half as many oaks, and these and the ashes are of so inferior economical value, that (as we are told) a passable waggon-wheel cannot be made of California wood, nor a really good one in Oregon."

Prof. Gray then illustrates graphically by diagrams, which we here reproduce, this poverty of the western forest in species in type (of timber-trees); it has only 31 genera and 78 species to 66 genera and 155 species on the Atlantic side. In the appended diagrams the short side of the rectangle is proportionate to the number of genera and the long side to the number of species. The geographical areas of the two forests are not very different, the length of the Pacific forest making up to some extent for its comparative narrowness.



1 2 3 4

1. Atlantic American Forest. 2. Pacific American Forest. 3. Japan-Manchurian Forest. 4. European Forest.

"How can so meagre a forest make so imposing a show? Surely not by the greater number and size of its individuals, so far as deciduous (or more correctly non-coniferous) trees are concerned; for on the whole they are inferior to their eastern brethren in size if not in number of individuals. The reason is, that a larger proportion of the genera and species are coniferous trees; and these, being evergreen (except the larches), of aspiring port and eminently gregarious habit, usually dominate where they occur. While the east has almost three times as many genera and four times as many species of non-coniferous trees as the west, it has slightly fewer genera and almost one-half fewer species of coniferous trees than the west. That is, the Atlantic coniferous forest is represented by eleven genera and twenty-five species; the Pacific by twelve genera and forty-four species. This relative preponderance may also be expressed by the diagrams, in which the smaller inclosed rectangles, drawn on the same scale, represent the coniferous portions of these forests.

"Indeed, the Pacific forest is made up of conifers, with non-coniferous trees as occasional undergrowth or as scattered individuals, and conspicuous only in valleys or in the sparse tree-growth of plains, on which the oaks at most reproduce the features of the 'oak openings' here and there bordering the Mississippi prairie region. Perhaps the most striking contrast between the west and the east, along the latitude usually traversed, is that between the spiry evergreens which the traveller leaves when he quits California, and the familiar woods of various-hued round-headed trees which give him the feeling of home when he reaches the Mississippi. The Atlantic forest is particularly rich in these, and is not meagre in coniferous trees. All the glory of the Pacific forest is in its coniferous trees: its desperate poverty in other trees appears in the annexed diagram. These diagrams are made more instructive, and the relative richness of the forests round the world in our latitude is most simply exhibited, by adding two or three similar ones. Two will serve, one for Europe, the other for North-East Asia . . . .

"Keeping as nearly as possible to the same scale, we may count the indigenous forest trees of all Europe at 33 genera and 85 species. And those of the Japan-Manchurian region, of very much smaller geographical area, at 66 genera and 168 species. I here include in it only Japan, Eastern Manchuria, and the adjacent borders of China. The known species of trees must be rather roughly determined, but the numbers here given are not exaggerated, and are much more likely to be sensibly increased by further knowledge than are those of any of the other regions. Properly to estimate the surpassing richness of this Japan-Manchurian forest, the comparative smallness of geographical area must come in as an important consideration.

"To complete the view, let it be noted that the division of these forests into coniferous and non-coniferous is, for the

European non-coniferous,	26 genera,	68 species.
,, coniferous	7 ,,	17 ,,
	33 ,,	85 ,,
Japan-Manchurian non-coniferous	47 genera,	123 species.
,, ,, coniferous	19 ,,	45 ,,
	66 ,,	168 ,,

In other words, a narrow region in Eastern Asia contains twice as many genera and about twice as many species of indigenous trees as are possessed by all Europe; and as to coniferous trees, the former has more genera than the latter has species, and over twice and a half as many species.

"The only question about the relation of these four forest regions, as to their component species, which we can here pause to answer, is to what extent they contain trees of identical species. If we took the shrubs, there would be a small number, if the herbs a very considerable number, of species common to the two New World and to the two Old World areas respectively, at least to their northern portions, even after excluding arctic-alpine plants. The same may be said, in its degree, of the North European flora compared with the Atlantic North American, of the North-East Asiatic compared with the northern part of the Pacific North American, and also in a peculiar way (which I have formerly pointed out and shall have soon to mention) of the North-Eastern Asiatic flora in its relations to the Atlantic North American. But as to the forest trees there is very little community of species. Yet this is not absolutely wanting. The Red Cedar (*Juniperus Virginiana*) among coniferous trees, and *Populus tremuloides* among the deciduous, extend across the American continent specifically unchanged, though hardly developed as forest trees on the Pacific side. There are probably, but not certainly, one or two instances on the northern verge of these two forests. There are as many in which eastern and western species are suggestively similar. The hemlock-spruce of the Northern Atlantic States, and the yew of Florida are extremely like corresponding trees of the Pacific forest; indeed the yew-trees of all four regions may come to be regarded as forms of one polymorphous species. The white birch of Europe and that of Canada and New England are in similar case; and so is the common chestnut (in America confined to the Atlantic States), which on the other side of the world is also represented in Japan. A link in the other direction is seen in one spruce tree (called in Oregon Menzies spruce) which inhabits north-east Asia, while a peculiar form of it represents the species in the Rocky Mountains."

Prof. Gray then asks why the Pacific forest region, which is rich and in some respects unique in coniferous, should be so poor in deciduous trees. And how came California to have the monopoly of the two *Big-trees*, Sequoias, which have no near relatives anywhere? "Such relatives," he goes on to say, "as the Sequoias have are also local, peculiar, and chiefly of one species to each genus. Only one of them is American, and that solely eastern, the taxodium of our Atlantic States and the plateau of Mexico. The others are Japanese and Chinese. Why should trees of six related genera, which will all thrive in Europe, be restricted naturally, one to the eastern side of the American continent, one genus to the western side and very locally, the rest to a small portion of the eastern border of Asia? Why should coniferous trees most affect and preserve the greatest number of types in these parts of the world? And why should the north-east Asian region have, in a comparatively small area,

not only most coniferous trees, but a notably larger number of trees altogether than any other part of the northern temperate zone? Why should its only and near rival be in the antipodes, namely, here in Atlantic North America? In other words, why should the Pacific and the European forests be so poor in comparison, and why the Pacific poorest of all in deciduous, yet rich in coniferous trees?"

Prof. Gray ventures to conclude that this richness is normal, and that what we really have to explain is the absence of so many forms from Europe on the one hand, from Oregon and California on the other. He shows that most of the forms, even of shrubs and herbs, which are peculiar to the Atlantic forest, have their close counterparts in Japan and North China. Prof. Gray noticed the feature long ago, and evidences of the remarkable relationship have multiplied year after year.

"The result, as to the trees, is seen in these four diagrams. As to number of species generally, it cannot be said that Europe and Pacific North America are at all in arrears. But as to trees, either the contrasted regions have been exceptionally favoured, or these have been hardly dealt with. There is, as I have intimated, some reason to adopt the latter alternative.

"We may take it for granted that the indigenous plants of any country, particularly the trees, have been selected by climate. Whatever other influences or circumstances have been brought to bear upon them, or the trees have brought to bear on each other, no tree could hold its place as a member of any forest or flora which is not adapted to endure even the extremes of the climate of the region or station. But the character of the climate will not explain the remarkable paucity of the trees which compose the indigenous European forest. That is proved by experiment, sufficiently prolonged in certain cases to justify the inference. Probably there is no tree of the northern temperate zone which will not flourish in some part of Europe. Great Britain alone can grow double or treble the number of trees that the Atlantic States can. In all the latter we can grow hardly one tree of the Pacific coast. England supports all of them, and all our Atlantic trees also, and likewise the Japanese and North Siberian species, which do thrive here remarkably in some part of the Atlantic coast, especially the cooler-temperate ones. The poverty of the European sylva is attributable to the absence of our Atlantic American types, to its having no magnolia, liriodendron, asimina, negundo, no æsculus, none of that rich assemblage of leguminous trees represented by locusts, honey-locusts, gymnocladus, and cladrastis (even its cercis, which is hardly European, is like the Californian one, mainly a shrub); no nyssa, nor liquidambar; no ericaceæ rising to a tree; no bumelia, catalpa, sassafras, osage orange, hickory, or walnut; and as to conifers, no hemlock spruce, arbor-vitæ, taxodium, nor torreya. As compared with north-eastern Asia, Europe wants most of these same types, also the ailantus, gingko, and a goodly number of coniferous genera. I cannot point to any types tending to make up the deficiency, that is, to any not either in east-north America or in north-east Asia, or in both. Cedrus, the true cedar, which comes near to it, is only north African and Asian. I need not say that Europe has no sequoia, and shares no special type with California.

"Now the capital fact is, that many and perhaps almost all of these genera of trees were well represented in Europe throughout the later tertiary times. It had not only the same generic types, but in some cases even the same species, or what must pass as such, in the lack of recognisable distinctions between fossil remains and living analogues. Probably the European miocene forest was about as rich and various as is ours of the present day, and very like it. The glacial period came and passed, and these types have not survived there, nor returned. Hence the comparative poverty of the existing European sylva, or at least, the probable explanation of the absence of those kinds of trees which make the characteristic difference."

Before answering the question as to why these trees perished out of Europe but survive in America and Asia Prof. Gray inquires how these American trees came to be in Europe. From certain considerations he is led to the inference that all species closely related to each other have had a common birthplace and origin. So that when we find individuals of a species or of a group widely out of range of their fellows, we wonder how they got there. When we find the same species all round the hemisphere—and a very considerable number of species of herbs and shrubs, and a few trees are so found—we ask how this dispersion came to pass. Prof. Gray goes on to say:—

"I take it that the true explanation of the whole problem comes from a just general view, and not through piecemeal suppositions of chances. And I am clear that it is to be found by looking to the north, to the state of things at the arctic zone first, as it now is, and then as it has been. North of our forest-regions comes the zone unwooded from cold, the zone of arctic vegetation. In this, as a rule, the species are the same round the world; as exceptions, some are restricted to a part of the circle. The polar projection of the earth down to the northern tropic, as here exhibited, shows to the eye—as our maps do not—how all the lands come together into one region, and how natural it may be for the same species, under homogeneous conditions, to spread over it. When we know, moreover, that sea and land have varied greatly since these species existed, we may well believe that any ocean-gaps, now in the way of equable distribution, may have been bridged over. There is now only one considerable gap.

"What would happen if a cold period were to come on from the north, and were very slowly to carry the present arctic climate, or something like it, down far into the temperate zone? Why, just what has happened in the glacial period, when the refrigeration somehow pushed all these plants before it down to southern Europe, to middle Asia, to the middle and southern part of the United States; and at length receding, left some parts of them stranded on the Pyrenees, the Alps, the Apennines, the Caucasus, on our White and Rocky Mountains, or wherever they could escape the increasing warmth as well by ascending mountains as by receding northward at lower levels. Those that kept together at a low level, and made good their retreat, form the main body of present arctic vegetation. Those that took to the mountains had their line of retreat cut off, and hold their positions on the mountain-tops under cover of the frigid climate due to elevation. The conditions of these on different continents or different mountains are similar, but not wholly alike. Some species proved better adapted to one, some to another, part of the world; where less adapted, or less adaptable, they have perished; where better adapted, they continue—with or without some change;—and hence the diversification of alpine plants, as well as the general likeness through all the northern hemisphere.

"All this exactly applies to the temperate zone vegetation, and to the trees that we are concerned with. The clue was seized when the fossil botany of the high arctic regions came to light; when it was demonstrated that in the times next preceding the glacial period—in the latest tertiary—from Spitzbergen and Iceland, to Greenland and Kamtschatka, a climate like that we now enjoy prevailed, and forests like those of New England, and Virginia, and of California, clothed the land. We infer the climate from the trees; and the trees give sure indications of the climate.

"I had divined and published the explanation long before I knew of the fossil plants. These, since made known, render the inference sure, and give us a clear idea of just what the climate was. At the time we speak of, Greenland, Spitzbergen, and our arctic sea-shore had the climate of Pennsylvania and Virginia now. It would take too much time to enumerate the sorts of trees that have been identified by their leaves and fruits in the arctic later tertiary deposits.

". . . . Long genealogies always deal more or less in conjecture; but we appear to be within the limits of scientific inference when we announce that our existing temperate trees came from the north, and within the bounds of high probability when we claim not a few of them as the originals of present species. Remains of the same plants have been found fossil in our temperate region, as well as in Europe.

"Here, then, we have reached a fair answer to the question how the same or similar species of our trees came to be so dispersed over such widely separated continents."

Prof. Gray then shows what would naturally follow from a gradual pushing of the Arctic vegetation southwards, and that the modifications resulting from differences of climate in the divergent continents, and on their different sides, might well account for the present diversification. The siftings and resiftings which have since taken place from changes of climate, submergence, and re-emergence, and other causes, have left their impress on the actual vegetation, especially on the trees. They furnish probable reason for the loss of American types sustained by Europe.

"I conceive that three things have conspired to this loss. First, Europe, hardly extending south of latitude 40°, is all within the limits generally assigned to severe glacial action.

Second, its mountains trend east and west, from the Pyrenees to the Carpathians and the Caucasus beyond, near its southern border; and they had glaciers of their own, which must have begun their operations, and poured down the northward flanks, while the plains were still covered with forest on the retreat from the great ice-wave coming from the north. Attacked both on front and rear, much of the forest must have perished then and there. Third, across the line of retreat of those which may have flanked the mountain-ranges, or were stationed south of them, stretched the Mediterranean, an impassable barrier. Some hardy trees may have eked out their existence on the northern shore of the Mediterranean and the Atlantic coast. But we doubt not, taxodium and sequoias, magnolias and liquidambars, and even hickories and the like were among the missing. Escape by the east, and rehabilitation from that quarter until a very late period, was apparently prevented by the prolongation of the Mediterranean to the Caspian, and thence to the Siberian ocean."

Prof. Gray shows that on the American continent on the other hand the trees, when touched in the north by the incoming refrigeration, had only to move their southern border southward, along an open way, as far as the exigency required; and there was no impediment to their due return. The still greater richness of north-east Asia in arboreal vegetation may find an explanation in the prevalence of particularly favourable conditions, both anteglacial and recent.

"The case of the Pacific forest is remarkable and paradoxical. It is, as we know, the sole refuge of the most characteristic and wide-spread type of miocene coniferæ, the sequoias; it is rich in coniferous types beyond any country except Japan; in its gold-bearing gravels are indications that it possessed, seemingly down to the very beginning of the glacial period, magnolias and beeches, a true chestnut, liquidambar, elms, and other trees now wholly wanting to that side of the continent, though common both to Japan and to Atlantic North America. Any attempted explanation of this extreme paucity of the usually major constituents of forest, along with a great development of the minor, or coniferous, element, would take us quite too far, and would bring us to mere conjectures."

Prof. Gray concludes his interesting lecture by saying:—

"I have done all that I could hope to do in one lecture if I have distinctly shown that the races of trees, like the races of men, have come down to us through a prehistoric (or prenatural-historic) period; and that the explanation of the present condition is to be sought in the past, and traced in vestiges, and remains, and survivals; that for the vegetable kingdom also there is a veritable archæology."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Cambridge Smith's Prizes have been awarded to Micaiah John Muller Hill, B.A., St. Peter's College, and Arnold Joseph Wallis, B.A., Trinity College, bracketed equal. These gentlemen were also bracketed equal as fourth wranglers in 1879.

A UNIVERSITY for ladies will be opened shortly in Odessa. It will have three faculties—History and Literature, Mathematics, and Natural Science. The programme will be the same as in the other Russian Universities for male students, with a few changes. Greek will not be obligatory in the Historico-Literary Faculty; there will be in the same faculty a Chair of Political Economy and Statistics. Pedagogy and hygiene will be obligatory in all faculties.

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 12, 1878.—In an inaugural dissertation here given, Herr Nahrwold studies atmospheric electricity; his method of experiment having been to electrify air in a cylindrical vessel fitted with a (mercury) dropping collector. His first attempts, with points, convinced him that only the dust, not the air, could be thus electrified; he then successfully used a fine platinum wire kept glowing with a battery (the air having been first freed from dust), and a condenser or galvanic element connected with the circuit. Interesting data are furnished with regard to the charge of the air, the ratio of this to the source of electricity used, and the decrease of the charge. Some of the observations seem to throw doubt on Thomson's conclusions as to the distribution of electricity in the upper regions of the atmosphere.—Herr Wiedemann offers a theory on the nature of spectra, deduced from the kinetic theory of gases. Line-spectra are attributed to oscillatory motions of atoms, isolated at high temperatures; band spectra of elements and spectra of compounds to vibrations of atoms in the molecule, or of the ether-envelopes. —A quantitative verification of the electrodynamic law, regarding the reciprocal action of closed circuits, for the case in which the circuit suffers deformation, is furnished by Herr Niemöller.—Herr Korteweg discusses the velocity of propagation of sound in elastic tubes, and Herr Rühlmann gives formulæ for measurement of ocean depths with the manometer. —There are several notes on crystallography.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 23.—"Researches on Lactin," by Edmund J. Mills, D.Sc., F.R.S., "Young" Professor of Technical Chemistry in Anderson's College, Glasgow, and James Hogarth.

The authors have investigated lactin with the aid of polarised light, their object being to gain further insight into the chemical nature of that compound. Their conclusions are as follows:—

1. The initial specific rotation of lactin is $92°\cdot63$.
2. The permanent specific rotation of lactin is $59°\cdot17$.
3. The change of rotation of a solution of lactin can be expressed by a mathematical equation.
4. When the specific rotation $64°\cdot8$ is reached, the law of change must be expressed by a different equation.
5. The initial solubility of lactin is one part lactin in 10·64 parts water.
6. The permanent solubility is one part lactin in 3·23 parts water.

"Researches on Chemical Equivalence." Part II. Hydric Chloride and Sulphate. By Edmund J. Mills, D.Sc., F.R.S., and James Hogarth.

While carrying out their researches on lactin, it struck the authors that use might be made of it to compare the dynamical equivalents of acid bodies. They accordingly selected hydric chloride and hydric sulphate for the measurements in question.

The results show that though $2HCl$ may be the "equivalent" of H_2SO_4 in weight for saturation (*i.e.*, in the ordinary sense), it certainly is not the equivalent in the dynamical sense. They also render it highly probable that HCl is equal dynamically to H_2SO_4. Ostwald, by a method based on the alteration of the specific volume of solutions, has shown that the ratio $\frac{2HCl}{H_2SO_4} = 1\cdot93$, a result which their numbers, though not as perfect as the authors could wish, nevertheless strongly confirm.

"Limestone as an Index of Geological Time," by T. Mellard Reade, C.E.

January 30.—"On Certain Means of Measuring and Regulating Electric Currents." By C. William Siemens, D.C.L., F.R.S.

The dynamo-electro machine furnishes us with a means of producing electric currents of great magnitude, and it has become a matter of importance to measure and regulate the proportionate amount of current that shall be permitted to flow through any branch circuit, especially in such applications as the distribution of light and mechanical force.

On June 19 last, upon the occasion of the *soirée* of the President of the Royal Society, was exhibited a first conception of an arrangement for regulating such currents, which the author has since worked out into a practical form. At the same time a method has been realised by which currents passing through a circuit, or branch circuit, are measured, and graphically recorded.

It is well known that when an electric current passes through a conductor heat is generated, which, according to Joule, is proportionate in amount to the resistance of the conductor, and to the square of the current which passes through it in a unit of time, and advantage has been taken of this well-established law of electro-dynamics, in order to limit and determine the amount of current passing through a circuit.

The paper refers to three instruments, in the first of which one end of a thin strip of metal is attached to a screw, by which its tension can be regulated; it then passes upwards over an elevated insulated pulley, and down again to the end of a short

lever working on an axis, armed with a counter-weight and with a lever whose angular position will be materially affected by any small elongation of the strip that may take place from any cause. The apparatus further consists of a number of prisms of metal, supported by means of metallic springs, so regulated by movable weights as to insure the equidistant position of each prism from its neighbour.

A series of comparatively thin coils of wire of German silver or other resisting metal, connect the alternate ends of each two adjoining springs, the first and last spring being also connected to the lever and terminal respectively.

The strip is put under a glass shade, and the instrument itself should be placed in a room where a tolerably uniform temperature of say 15° C. is maintained.

When the minimum current is passing, the thin metallic strip is at its minimum working temperature, and all the metallic prisms are in contact, this being the position of least resistance. As soon as the current passing through the apparatus shall increase in amount, the thin metallic strip will immediately rise in temperature, which will cause it to elongate, and will allow the lever to recede from its extreme position, liberating one contact piece after another. Each such liberation will call into action the resistance coil connecting the spring ends, and an immediate corresponding diminution of the current through the additional resistance thus thrown into the circuit.

Suppose that the current intended to be passed through the instrument is capable of maintaining the sensitive strip at a temperature of say 60° C., and that a sudden increase of current takes place in consequence either of an augmentation of the supply of electricity or of a change in the extraneous resistance to be overcome, the result will be an augmentation of temperature, which will continue until a new equilibrium between the heat supplied and that lost by radiation is effected. If the strip is made of metal of high conductivity, such as copper or silver, and is rolled down to a thickness not exceeding 0·05 millim., its capacity for heat is exceedingly small, and its surface being relatively very great, the new equilibrium between the supply of heat and its loss by radiation is effected almost instantaneously. But, with the increase of temperature, the position of the regulating lever is simultaneously affected, causing one or more contacts to be liberated, and as many additional resistance coils to be thrown into circuit: the result being that the temperature of the strip varies only between very narrow limits, and that the current itself is rendered very uniform, notwithstanding considerable variation in its force, or in the resistance of the lamp, or other extraneous resistance which it is intended to regulate.

The resistance coils, by which adjoining contact-springs are connected, may be readily changed, so as to suit particular cases; they are made by preference of naked wire, in order to expose the entire surface to the cooling action of the atmosphere.

For feeble currents, disks of carbon are substituted for the wire rheostat, the electrical resistance of carbon varying inversely with the pressure to which it is subjected. A steel wire of say 0·3 millim. diameter is drawn tight between the end of a bell-crank lever and an adjusting screw, the pressure of the lever being resisted by a pile of carbon disks placed in a vertical glass tube. The current, passing through the steel wire, through the bell-crank lever, and through the carbon disks, encounters the minimum resistance in the latter so long as the tension of the wire is at its maximum; whereas the least increase in temperature of the steel wire by the passage of the current causes a decrease of pressure upon the pile of carbon disks, and an increase in their electrical resistance; it will thus be readily seen that, by means of this simple apparatus, the strength of small currents may be regulated so as to vary only within certain narrow limits.

The apparatus first described may be adapted also for the *measurement* of powerful electric currents. The variable rheostat is in this case dispensed with, and the lever carries at its end a pencil pressing with its point upon a strip of paper drawn under it in a parallel direction with the lever by means of clockwork. A second fixed pencil draws a second or datum line upon the strip, so adjusted that the lines drawn by the two pencils coincide when no current is passing through the sensitive strip. The passage of a current through the strip immediately causes the pencil attached to the lever to move away from the datum line, and the distance between the two lines represents the temperature of the strip. This temperature depends, in the first place, upon the amount of current passing through the strip, and, in the second place, upon the loss of heat by radiation from the strip; which two quantities balance one another during any interval that the current remains constant.

In order to facilitate the process of determining the value of the diagram produced by motion of pencil in Weber's or other units of current, it is only necessary, if the variations are not excessive, to average the ordinates, and to determine their value from a table prepared for that purpose. The error committed in taking the average ordinate instead of the absolute ordinates, when the current varies between small limits, is evidently small, the variation of the ordinates above their mean value averaging the variations below the same.

The thin sensitive conductor may thus be utilised either to restrict the amount of electricity flowing through a branch circuit, within certain narrow limits, or to produce a record of the amount of current passed through a circuit in any given time.

Physical Society, January 25.—Prof. G. C. Foster, vice-president, in the chair.—Prof. E. Ray Lankester and Mr. Alex. Macdonald, B.A., were elected Members.—Dr. Erck exhibited a constant bichromate of potash battery. The ordinary bichromate battery soon loses power when in use, and in order to secure a powerful constant battery to drive a small astronomical clock, Dr. Erck devised the modified form shown. It consists of a narrow lead trough 12 inches long by 3 inches wide and 1 inch deep, lined along both sides with two carbon plates. The zinc plate 10 inches long is immersed in the solution to the depth of an inch midway between the two carbons. A continual circulation of the bichromate solution is kept up by allowing fresh solution to drop into the cell at one end, and the exhausted solution to drop away by a tap at the other end. As the space between the two carbons is only about half an inch wide, there is merely a thin layer of solution between the positive and negative poles. The internal resistance of the cell is, therefore, very low, when short circuited only about $\frac{1}{4}$ ohm. To obtain the maximum current about 8 oz. of solution per hour should be applied. Dr. Erck also showed a battery formed of zinc and carbon circular plates mounted on an axle which is rotated by wheelwork, thus mechanically stirring the bichromate solution.—Dr. F. Guthrie, F.R.S., described some of the results he had obtained from experiments on the vibration of metal rods or lathes fixed in a vice at one end and free to vibrate at the other. The experiments were carried on by dusting sand on the rod and observing the nodal lines formed by it when the rod was vibrated, so as to give out notes determined by a monochord. Dr. Guthrie's results show that the two final segments at the free end are together equal in length to the inner segment at the fixed end. It appears from these experiments that if a free lathe vibrating with a node in the middle, but having an even number of segments, be clamped at where there is a node, we alter its conditions of vibration. When the lathe is half free, the end segment breaks up into two parts together equal to the segment at the fixed end. In the case of a torsional vibration of the lathe, the position of the longitudinal nodal lines depended to some extent on the clamping of the lathe in the vice. Prof. Foster pointed out that in a natural node the direction of the tangent is varying, whereas in an artificial node it is always horizontal. Prof. Unwin explained that the sand accumulated at nodes because the particles, when thrown off the lathe, make certain horizontal excursions which tend to move them nearer the points of repose of the lathe.—Messrs. Elliot Brothers exhibited sundry electric commutators and resistance boxes.

Anthropological Institute, January 21.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—The Director read a communication from Dr. Paul Topinard on resemblances between a Galtcha and a Savoyard skull. The similarity between these skulls is such that the author is inclined to regard the Galtchas of Eastern Turkistan and the Celts of Western Europe as branches of one common stock, of which the Slavs of Eastern Europe are also members.—M. Elie Reclus read a paper on circumcision, its significance, its origin, and its kindred rites. The practice of this custom was traced over a large portion of the inhabited globe, including Australia and South America, though among the nations of antiquity the Egyptians and Jews are those among whom it is best known to have prevaile·.

Photographic Society, January 14.—James Glaisher, F.R.S., in the chair.—Capt. Abney, R.E., F.R.S., read a paper on the fading of the undeveloped photographic image, and on soluble bromide in emulsions; and Col. Wortley explained a new instantaneous shutter, designed by him. Capt. Abney, in his paper, stated that one cause of the fading arose from impure

pyroxyline, which, during decomposition, liberated nitrous acid; and that this acid would destroy (before development) a photographic image on collodion, made from such pyroxyline.—Capt. Abney then discovered that as an excess of alkaline bromide would not diminish sensitiveness, or rather not prevent an image being formed, consequently, if a nitrite of some alkali, in conjunction with an excess of bromide, were added, the formation of any acid would be prevented, so that by the application of an alkaline carbonate to the film, all nitrous acid liberated from pyroxyline is absorbed to form an alkaline nitrite, and the destruction of the photographic image avoided.

CAMBRIDGE

Philosophical Society, December 2, 1878.—Prof. Liveing, president, in the chair.—Dr. G. W. Royston-Pigott made a communication to the Society on a new method of determining the limits of microscopic vision. The author referred to his method of forming miniatures by reversing an object-glass which has been described in the *Philosophical Transactions*, and showed how he had applied it to determine the limit of smallness of objects that could be detected by the microscope. The process employed was to form a miniature of an object such as a spider-line, and then examine the miniature with the microscope. In this manner Dr. Royston-Pigott had found that objects even as small as the millionth of an inch in diameter could be seen, contrary to the generally received view of opticians that it was useless to attempt to perfect the microscope further, as it could not show objects smaller than the hundred-thousandth of an inch in diameter. Dr. Jurin found that with the naked eye he could discover a pin fixed in a window forty feet away from him, subtending an angle of two or three seconds, but if he placed two pins together he could not distinguish them as separated except they were so far apart as to make an angle of forty seconds. Thus a bright interval could not be discovered unless it were ten or fifteen times larger than the objects forming it; but Dr. Royston-Pigott found that the excellence of modern objectives was such as to enable the eye to discern an interval only four times larger than the diameter of the web miniature, reckoned at one millionth of an inch. The microscopes and apparatus employed to produce the miniature were exhibited to the Society, and the precautions to be taken in the use of the method were explained.—Mr. W. M. Hicks communicated some results of an investigation on the motion of two cylinders surrounded by fluid.

PARIS

Academy of Sciences, January 27.—M. Daubrée in the chair.—The following papers were read:—Third reply to M. Berthelot, by M. Pasteur.—On the development of the perturbative function where, the excentricities being small, the mutual inclination of the orbits is considerable, by M. Tisserand.—On a formula giving approximately the moment of torsion, by M. de Saint-Venant.—Researches on the relations of spectrum analysis to the spectrum of the sun, by Mr. J. Norman Lockyer.—On the electric clutch-gear on board ships, by MM. Trève and Achard. An arrangement for managing marine engines or helms at a distance electrically.—On the composition of banana and attempts at utilisation of this fruit, by MM. Marcano and Muntz. The banana keeps the ground moist round it, and this property is utilised in Venezuela to help the coffee-plant in dry weather. But only a little of the banana's fruit is there used, and the authors call attention to the flour and the alcohol obtainable from it, recommending exportation. The flour is a food essentially feculent.—On the application of his atomic theory to various minerals, by M. Gaudin.—On the diameters of the sun and of Mercury, deduced from the transit of May, 1878, by M. Cruls (Rio de Janeiro). The solar semi-diameter is found 15′ 59″·982, agreeing nearly with Leverrier's 16′ 0″·0 (deduced from previous transits). The value for the planets' diameter, got from observations of ingress, was 10″·78, by another method 10″·74.—Hydro-electricity and hydro-magnetism; analytical results, by M. Bjerknes.—On a development in series, by M. Picard.—Displacement of spectral lines due to motion of rotation of the sun, by M. Thollon. His experiments on this with his new prism convince him that with a suitable arrangement the displacement may be produced in an incontestable manner. The telluric lines did not show any change, and some of them, near those of nickel, enabled one to see very distinctly how the latter were displaced.—On the radiation of incandescent platina, by M. Violle. He measured this from 900 to 1,775 degrees. The intensity of a given radiation does not increase indefinitely with the temperature, but passes through a maximum and then decreases to a point, where it becomes insensible. The luminous heat of fused platinum transmitted through alum is $\frac{1}{4·5}$ of the total heat transmitted through rock-salt.—On the illumination of lines of molecular pressure, and on the trajectory of molecules, by Mr. W. Crookes. M. Du Moncel said the dark space is not only manifested in vacuo; it is distinctly seen round the negative electrode, on sending an induction spark between two plates of glass, and examining it with a microscope. (A figure is given.)—On electrodynamic phenomena, and especially on induction, by M. de Meaux. In a closed circuit, you do not change the intensity of the current produced by induction of an indefinite cylindrical conductor on another of the same form, by surrounding one or other, or even both, of these conductors with a concentric metallic envelope, communicating with the ground throughout its length.—On a new Bell telephone speaking with loud voice, by M. Gower. The two magnet poles are placed opposite each other; the diaphragm is thick, large, and tense; the inclosing case is metallic and sonorous, and a speaking trumpet is added.—On the amalgams of chromium, manganese, iron, cobalt, and nickel, and on a new process of preparation of metallic chromium, by M. Moissan. This method is, stirring a concentrated solution of protochloride of chromium in water with pasty sodium-amalgam; the amalgam of chromium obtained is then heated to 350° in a current of hydrogen. Amalgams of the other metals named may be had similarly.—On a preparation of methylformic ether and of pure methylic alcohol, by MM. Bardy and Bordet.—On the principles which give *Sarracenia purpurea* its therapeutic properties, by M. Hetet.—On the termination of the visceral arterioles of *Arion rufus*, by M. Jourdain.—Researches on the action of *grenat*, or the residue of the manufacture of fuchsine, by M. Jousset de Bellesme. This is used to colour wines. It may be taken in large quantities without causing death; but it is hurtful, producing uræmia, &c.—On the quantity of light lost in actuating the visual apparatus, and its variations under different conditions, by M. Charpentier. A light being gradually increased from zero, you note when the eye perceives it; it may then be reduced considerably without the eye ceasing to perceive it. This difference is much greater, if the eye have been kept in the dark five minutes or more. But this effect of rest in the dark does not apply (or very little) to chromatic sensibility. The author considers the sensation of light wholly independent of that of colour.—On the phosphorescence of the lobster's flesh, by MM. Bancel and Husson. They consider it due to a kind of fermentation.—M. Mége Mouris presented a note on the properties of marine salt; and MM. Nasse and Decharme, notes on a liquid rain which lately covered the ground with a thick surface of ice.

CONTENTS

THURSDAY, FEBRUARY 13, 1872

THE "THUNDERER" EXPLOSION

NOW that the Committee appointed to inquire into the cause of the bursting of one of the 38-ton guns on board the *Thunderer* have made known the substance of their report, it is no longer necessary to suspend judgment and withhold comment for fear of prejudicing the case. The abstract only of the report of the Committee, which has reached us by telegram, states that the explosion was due to a cause, which, as far as we are aware, was not anticipated by any one of the numerous writers who have made suggestions or advanced theories on the subject. They simply report that the gun having missed fire when loaded with the battering charge, was again loaded with a full charge, and fired with both of the charges in the gun at the same time. We are bound to assume that conclusive evidence of this extraordinary occurrence has been obtained by the Committee, or the statement would be simply incredible. Cases are recorded in volley-firing of a soldier loading charge after charge into his musket or rifle in the days of muzzle-loaders, being unaware that the first had missed fire until the violent explosion, and perhaps bursting of his piece, or the apparent growth of his ramrod showed that something unusual was taking place, but that a squad of blue-jackets serving a 38-ton gun should be unaware, or even uncertain, if the 110 lb. of powder they were supposed to be firing had exploded or not is beyond comprehension. Even if the two guns in the fore turret were being fired simultaneously by electricity, the absence of recoil would have alone been sufficient to indicate that the gun had missed fire. That the rammer did not pass home might, it is conceivable, have escaped notice in loading the second round with the hydraulic gear, but we must look to the report of the Committee for some explanation of the first miss-fire having escaped notice.

In the many suggestions that have been made respecting the probable cause of the disaster by the writers of letters to the daily and weekly papers, it appears to us that a too low estimate has generally been made of the strength of the gun. It has been assumed by many that if it could be shown that the projectile slipped forward in the bore on the withdrawal of the rammer, a sufficient cause for the accident had been discovered. Experiments were at once tried at Woolwich which established the fact that when no ring-wad was used the shot frequently followed the rammer on its withdrawal for a distance of three or four feet. Instances have also been given of the bursting of fowling-pieces through an accidental stoppage of the muzzle by snow or a wad, and it was thought by many that a clear case had been made out for the cause of the explosion. Sir William Palliser's experiments, however, disprove the truth of this suggestion, for, many years ago, in firing an experimental 8-inch gun with various air spaces up to four feet between the powder charge and the shot, he found that there was no dangerous pressure in the bore. The gun made use of in this experiment was an old cast-iron 68-pounder which was converted into an 8-inch rifled gun, on Sir William Palliser's system by the insertion of a coiled wrought-iron tube $2\frac{1}{2}$ inches thick. This gun was fired with 22 lbs. of powder and a shot of 180 lbs. weight, with successive air spaces of 10 inches, 20 inches, up to 50 inches between the powder and the shot, and sustained no damage. Sufficient experiments have not yet been made to indicate accurately all the variations of pressure that would be introduced by an air-space between the powder charge and shot, but the only cause which would tend to raise the gas pressure, and in particular the longitudinal and twisting strain on the gun, above that due to an ordinary discharge is the fact that the pitch of the rifling would be sharper where the projectile started in the former case than in the latter. In the case of a fowling-piece the cause of bursting with a slight stoppage at the muzzle is probably due rather to the weakness of the barrel at that part than to the excessive pressure. In the 38-ton gun, with the heaviest charges employed on service, the central part of the length of the gun known as the 1 B coil is probably amply strong enough to resist the pressure brought to bear upon it, even if the shot was in this part of the bore and the maximum gas-pressure exerted. Although it has been shown by the report of the Committee that none of the conjectures as to the cause of the accident is correct, there is still utility in examining them, for, if valid, they might bring about a similar accident at some future time, and should therefore be provided against. Though the running forward of the shot when the gun is much depressed for loading with the hydraulic gear would cause but a small risk of explosion to the gun, it is so undesirable for many other reasons, that means should be taken to render it impossible, or at least render its detection a certainty. The means at present employed to obviate the shifting of the shot when once rammed home, consist of a ring wad which fits round the pointed head of the shot, and is firmly wedged in between it and the bore. Here again we are provided with a very possible chance of an accident. The two letters of Sir William Palliser published in the *Times* pointed out what appeared to us, before the publication of the report, by far the most probable solution of the problem: it consisted simply in the employment at the same time of a gas-check and a ring-wad. As clearly pointed out by Sir William, it is merely a question of the relative coefficient of friction between the shot and the wad and between the wad and the surface of the bore, which has to decide whether the wad is forced along the bore when the shot commences to move, or whether the head of the shot is forced further into the wad, wedging it tightly against the surface of the bore. This action would be impossible without the employment at the same time of a gas-check, as the gas rushing past the shot would inevitably dislodge the wad. The gas-check consists of a cupped disk of copper with the periphery turned down so as to form a flange; this is fitted to the base of the projectile, and, when fired, the pressure of the gas causes the flange to expand and press firmly on the surface of the bore, both in the grooves and in the lands between them, thus preventing the passage of the gas past the shot. Sir William says: "With regard to the friction between the wad and the gun, it is a fact that when the atmosphere is moist the residue of the powder deposits itself upon the surface of the bore in a black, greasy substance which reduces friction to a very low point. On a dry day, however, this deposit assumes the

form of a hard, dry, and rough coating like emery paper, and the friction in the bore is increased to a very large extent." Thus it may readily be seen that under varying circumstances the requisite conditions are very likely to occur, and experiments should most undoubtedly be undertaken to test the validity of this source of accident, and we think that great credit is due to Sir William Palliser for having been the first to point it out. Yet another cause has been insisted on as likely to bring about the destruction of some of our Woolwich rifled guns. In a paper read by Prof. Osborne Reynolds at the meeting of the British Association at Bradford, it was shown that the system of rifling employed at Woolwich, giving a gradually increasing twist to the grooves, threw a much greater strain on the gun than a uniform twist, and rendered it impossible for the studs on the shot to fit the grooves with accuracy throughout the whole length of the bore. The objections raised by Prof. Reynolds have, we believe, remained unanswered and perhaps unnoticed now for five or six years by the departmental officers, and during this time many hundreds of guns have been constructed on the condemned principle. Now that so much public attention is being directed to the whole system of gun manufacture as carried on at Woolwich, and the criticisms adverse to its merits are so numerous it would be well that the country should have further assurance that the system is founded on a safe scientific basis, or that the errors, if they exist, should be admitted and rectified.

It is admitted that with the increasing twist in the rifling the shearing of the studs has frequently shown that the projectile has some difficulty in centring itself in the bore while the great wear in the steel tubes of the guns occasionally used for practice on board ship points to a short life for the gun, but we are not aware that the destruction of a gun has hitherto been traced with certainty to the jamming of the studs between two grooves. In the event of this occurring it is shown conclusively by Mr. Longridge, in a letter inserted in *Engineering* last week, that the strain would be far more than sufficient to burst the gun. The bursting pressure of the gases would easily split the tube and coils, or the energy of the shot if only moving with a moderate velocity would cause a longitudinal stress which the steel tube would be quite unable to resist.

It may appear to some useless to draw particular attention to the various causes of this terrible accident that have been suggested now that the report of the Committee has been made known and has shown that all the suppositions are equally erroneous, but in our opinion it is of the utmost importance that these dangers to which we direct attention and which might at any time cause a similar accident, should not be passed over and forgotten.

CAPTAIN COOK

IT seems on first thoughts rather a strange proceeding to publicly celebrate the centenary of the death of a great man, especially when that death was a murder. But this is what the Paris Geographical Society have arranged to do to-morrow in the case, not of any of their own explorers or navigators, but in the case of England's greatest exploring navigator, Captain James Cook, who was murdered 100 years ago to-morrow by the natives of the Sandwich Islands. But we know that the generous-minded Frenchmen do not intend to rejoice at the death of this great man, as they would do were it his birth they intended to commemorate. Cook, they know, was one of the greatest of geographical explorers, and it is quite natural and commendable that the Society, in their enthusiasm for their science and its promoters, should wish in some way to show their reverence for a man like Cook on the centenary of his remarkable death. Cook, and with him England, owed some gratitude to the French, whose government of the time, though at war with this country, generously gave instructions to their war-ships and colonial governors, not only not to molest Cook in his pursuit of knowledge, but to render him all reasonable assistance. It is obvious that only about every third generation can take part in celebrating the centenary of a man's birth, and it is natural, therefore, that those of the intermediate generation who count him among their heroes, should take advantage of the occurrence of the centenary of his death to show their appreciation of his greatness. In Cook's case birthday and death-day were only about half a century apart, the date of the former being October 27, 1728.

Why our own Geographical Society should have left it to our French neighbours to commemorate so remarkable an event in the history of geographical discovery, we cannot undertake to say, though it seems to savour somewhat of dog-in-the-manger that they have declined the invitation to send an official representative. It would surely have been easy for them to have organised some kind of demonstration that would both have honoured the memory of one of our greatest naval heroes and most scientific of navigators, and at the same time have proved both interesting and instructive to the public. However, England will not be entirely unrepresented to-morrow, as we understand the Admiralty are contributing several original charts to the exhibition of the Paris Society. Mr. Brassey, who has visited the spot where Cook was killed, has sent to the Society a number of views and documents to be exhibited. Mr. Jackson has also promised to send valuable maps and manuscripts from Cook's own hand. Researches will be executed in the Archives to discover the original of the *Ordre du Roi*, forbidding French cruisers to molest Captain Cook's expedition, and in spite of the declaration of war, to assist him if necessary for the fulfilling of a mission interesting the whole of mankind.

We need not recount the claims of Captain Cook to be regarded as one of the greatest, as he was one of the most scientific, of navigating explorers; probably there are few of our readers who have not at one time or other read some account of the voyages of Captain Cook. The son of a peasant, he rose to his honourable position by sheer force of genius and its invariable accompaniment, hard work. To him we owe the discovery of the Sandwich, and many other Pacific Islands. His enthusiasm on behalf of science was manifested in his work at Tahiti in connection with the memorable transit of Venus of June 3, 1769. He gave certainty to our knowledge of New Zealand, and left not much to be done to fill up with accuracy an outline of the coast of Australia. He proved

that that continent was unconnected with New Guinea, and above all, dispelled the long-lived illusion of a great southern continent, having been the first to cross the Antarctic Circle. At high south latitudes he sailed nearly all round the confines of the Antarctic, adding greatly to a knowledge of the geography of this unknown region, and proving once for all, as we have said, that a "great southern continent" was a delusion, at least outside the Antarctic Circle. A comparison of the maps of 1762 and 1785 will serve to show how much was accomplished by Cook in this direction. In his second voyage of three years, 1772-75, Cook sailed over 20,000 leagues in the Pacific and Southern Oceans. And it was not only geographical knowledge that was thus advanced by his skill and determination. He was always accompanied by a staff of scientific specialists, to whom he gave every opportunity of pursuing research in their own departments, and thus of adding enormously to a knowledge of the natural history (in its widest sense) of great tracts of our globe. In the Transit expedition, for example, he was accompanied by the young Joseph Banks as naturalist. His third, and fatal voyage, was undertaken mainly for the discovery of a North-West Passage, Cook and Capt. Clarke sailing in the *Resolution* and *Discovery* from Plymouth in July, 1776, and after a roundabout voyage by the South Pacific, the Sandwich Islands were discovered on January 18, 1778. After attempting to penetrate the Arctic Ocean, he was compelled to turn back, and resolved to spend the winter in completing the survey of the Sandwich Islands. Here, as almost everywhere else that he went, Cook won the hearts of the natives by his gentle, firm, and perfectly upright dealing, in this respect being a pattern to all explorers. The end is too well known, and we need not repeat the details of the sad event which happened at Karakakooa Bay, on the south side of Hawaii, on February 14, 1779. No blame can be attributed to Cook, and, probably, very little to the natives themselves. Had the lieutenant who accompanied Cook on shore, and the sailors themselves, possessed a little of his tact and true bravery the catastrophe might have been prevented. There is reason to believe that the islanders regarded Cook as a sort of superior being, a kind of heaven-sent messenger whom they half-expected, and that they actually worshipped him as a god. Indeed it has been said that it was only when the first stunning blow from a club proved him human that their chagrin and disappointment vented themselves in barbarous massacre. There seems no doubt that the natives were sincerely sorry for what had occurred, and continued to worship his memory, if not his bones, for long after. It is commonly stated that his remains were obtained and buried in the sea, but we would refer our readers to a remarkable story published in NATURE, vol. viii. p. 211. From this it would seem that the large bones of Cook's body had been retained by the islanders, and tended and enshrined as those of a hero, if not of a deity. Whatever amount of truth there may be in the details of this story, it, along with other evidence, tends to prove that the catastrophe was a sad mistake, regretted by none more than the natives themselves.

Cook's instincts were thoroughly scientific, and he did all that his circumstances would admit of to qualify himself to carry on his great and important work on the basis of scientific principles. The results show that all things considered science profited largely by his labours, and that to-morrow a foreign society will strive to keep green the memory of one of England's most scientific navigators, one of her ablest and most lovable sons.

THE SAMOAN LANGUAGE

A Grammar and Dictionary of the Samoan Language. By the Rev. George Pratt. Second Edition. Edited by the Rev. S. J. Whitmee, F.R.G.S. (Trübner and Co., 1878.)

THIS is perhaps as complete a guide to the study of the Samoan language as could be expected under the circumstances. It consists, properly, of four parts: a grammar, which, for obvious reasons, is necessarily disappointing; a chapter on the native poetry, which would be much more useful were the specimens given accompanied by a translation, or at least by more copious notes; an English-Samoan vocabulary of about 4,500 fairly well-selected words, and a Samoan-English dictionary of more than double that number of terms. The editor informs us that many of the names of the indigenous flora and fauna collected by him still remain to be published. They will doubtless be embodied in the large "Comparative Polynesian Dictionary" he is now preparing, and when this is done we shall have at last a well-nigh complete dictionary of the most typical of the eastern Polynesian languages.

The grammatical portion of the work, notwithstanding the many extremely useful and suggestive additions of the editor, still leaves so much to be desired that we cannot but regret he did not re-cast this whole section, and give us a treatise more in harmony with the present state of linguistic studies. When we read in Mr. Pratt's preface that he was led to prepare a Samoan syntax "by observing, while reading Nordheimer's Hebrew grammar, that the Samoan in many points resembled the Hebrew," we feel at once that it would be hopeless to expect from him a sound exposition of the structure of this language, and the most cursory glance fully confirms this anticipation. Hebrew is a member of the Semitic family of languages, and is consequently an inflecting tongue. Hence it can have nothing beyond mere coincidences in common with the Eastern Polynesian group, which has scarcely yet got much beyond the isolating state, of which Chinese is typical. Its position, in fact, is quite unique, and until its true character is thoroughly realised we shall never get a rational treatment of the subject. This obvious truth was largely recognised by Gaussin, which at once explains the satisfactory nature of his work. Had it been based on the Samoan instead of on the Tahitian and Marquesan dialects, the result would doubtless have been still more satisfactory, and he would have avoided some of the misconceptions which detract from the value of that treatise. Yet even so it incidentally throws more light on the real genius of the Samoan itself than does the present work. Here the treatment of the verb is especially meagre and irrational. The schemes of tense and mood occupy less than two pages, and each tense is illustrated by a different verb, *pule* (rule) for the present, *alofa* (love) for the imperfect, *sao* (escape) for the perfect,

&c. The consequence is, that we get no general scheme at all of any given verb, which, however, is perhaps the less to be regretted, inasmuch as there are no true verbs at all in the language. These Eastern Polynesian tongues have certainly got beyond the purely isolating state of the Chinese, in which each root passes in its unmodified state directly into the sentence, where it becomes a true word only in virtue of its position. But they have not yet reached the next, or agglutinating state, because in them all parts of speech are not yet clearly differentiated. The so-called verb is merely a nominal predicate with the various temporal, personal, and modal relations more or less clearly expressed by determining particles. Hence the so-called second person present *e pule 'oe*, here rendered "thou rulest," is made up of the enunciative *e* equally applicable to present and future time, to the infinitive and to other words such as all the numerals (*e tasi* = one, *e lua* = two, &c., with which cf. *a* hundred, *a* thousand, &c.), of the noun *pule* = order, command, rule, and of the pronoun *'oe* = thou. Thus, the whole expression merely attributes the rule or command, that is, *the thing* in a vague way to the subject, and seems scarcely to convey the idea of action, that is, of the use of the thing as does the true verbal form *regis*, thou rulest, in which the original nominal conception is completely absorbed in the idea of action. We thus see that the verb, as a distinct part of speech, is not yet developed, though there is an evident tendency towards its evolution. Hence these so-called verbs are incapable of any change to express mood, tense, person, and even the plural forms, in which reduplication plays such a large part, are adjectival, as may be seen by comparing *sisina*, the plural of *sina* = white, with *nonofo*, the plural of *nofo* = to sit. On these plural forms the editor supplies some excellent supplementary matter at pages 13–16, which throws a strong light on the great influence of euphony in the development of language in its earlier stages. His remarks on the subtle distinction between the particles *a* and *o*, roughly corresponding to our possessive, are also very good. If to the active or transitive and passive or intransitive notions obviously involved in the use of *a* and *o* respectively, we add those of the *voluntary* and *involuntary* states, nearly all the difficulties will be removed, and the law may be confidently laid down that *a* is used with objects over which we have *free control*, *o* with those we possess, so to say, independently of ourselves, and which we must use in a definite way. Thus: *lona fale* = his house, *i.e.*, which he needs must use as a place of refuge or shelter; but *lana va'a* = his canoe, which he can apply to twenty different purposes. So also in the Tahitian: *ta'u va'a* = my canoe; *to'u fare* = my house; for such is the amazing homogeneity of these eastern Polynesian languages that the most delicate distinctions are often found to pervade the whole group from New Zealand to Hawaii, or from Samoa to Easter Island after a separation in some cases of certainly not less than six hundred years.

Mr. Whitmee's notes betray altogether such a deep insight into the true genius of this linguistic family that we earnestly hope, when another edition of this work is called for, he may be induced to suppress the author's grammar, and give us in its stead a thoroughly rational treatment of the subject. It will then be also very desirable in all cases to give a literal, or at least a close, translation of the examples quoted in illustration of the various rules and principles laid down. Many phrases are given in the present edition which may be useful to those already acquainted with the language, but which, for want of such a translation, it is to be feared will be thrown away upon the ordinary student, who may not have the opportunity of consulting a teacher. In the actual condition of these languages particles necessarily play a very large part, and are constantly heaped up in the sentence to a degree that must be very perplexing to the beginner. Where possible these particles should be translated, and when this cannot be done, which is very often the case, their various functions in the sentence should always be carefully indicated. This may, no doubt, demand more space, but the space can be saved by giving fewer examples and explaining them thoroughly. A comparative table of Eastern Polynesian alphabets, illustrating the interchange of letters between the various dialects, and throwing some light on their peculiar phonetic system, would also be a desirable addition, and might be brought within the compass of one or two pages.[1] But the essential point will always be to treat the language from a rational standpoint, independently of all fanciful Semitic, Aryan, or other affinities. The eastern Polynesian group has only just emerged from the isolating or lowest stage of human speech, and still hovers on the verge of the agglutinating or next stage, and must be dealt with accordingly. Hebrew, the classical tongues, English, French, and all others familiar to us, have passed upwards from the isolating through the agglutinating to the inflecting state, and have, therefore, little in common with Samoan, Maori, Tahitian, &c., beyond the faint reminiscences, still lingering on, of their former condition. When these simple truths are fully recognised grammarians may be expected to treat languages with some regard to their individual character.

A. H. KEANE

COAL AND IRON

Coal and Iron in all Countries of the World. By M. Pechar. (Manchester and London: John Heywood, 1878.)

AMONG the results of the International Exhibition at Paris which has just closed its doors, the reports and other permanent records of the actual condition of the great industries of the world are certainly not the least valuable. Even where, as in the case of the work now under review, the materials of comparison are not wholly or mainly derived from the Exhibition itself, still from it have been derived the desire and perhaps the opportunity to execute the work on so complete a scale.

The international character of the book is obvious from every part, even of the title-page; this is the authorised English edition; the subject it professes to treat is Coal and Iron in all Countries of the World; its author, M. Pechar, is a railway director in Teplitz, Bohemia. And it must be confessed that the contents of the book do not belie the title-page. Indeed, the first page of the General Remarks which introduce us to them would lead us to suppose that we were going to be treated to a

review not merely of all terrestrial space but of all terrestrial time as well, we are carried back "over an incalculable number of thousands of years" to the prehistoric time of crude tools of flint, bone, horn, and the like, and so by rapid strides through stone and bronze and iron to these last days, on which the age of steel is dawning.

This, however, is only by way of prelude, and the author straightway settles down to his facts and figures. These latter are naturally so numerous that the general reader will hardly find the book as a whole a light or entertaining one; yet apart from the very high value of the statistics for purposes of reference, there is much in the book to interest every one who cares for his country's or the world's welfare.

First, a few words with respect to the character of the statistics. These of course vary in completeness and in accuracy with the respective development or otherwise of the countries concerned; or of these special industries. With respect to our own country tabular statements are given of the number of pits for certain years between 1854 and 1876; of the production in each coal-district for the year last named; of the total produce in each year since 1854 as well as for certain years previously; of the percentage of increase in production and in value; of the chemical constituents of all the more important coals (sixteen in number); of the comparative heating power and general utility of certain English and Westphalian steam-coals used by the German navy; of the average price of coal for each year from 1865 and for certain years before; of the chief purposes to which the coal is applied and in what proportions; of the growth of British railways, and of the coal traffic thereon as well as by sea and canal; of the growth of British shipping; of the coal exports, and of the chief countries importing these, with the weight and value of their respective amounts. Add to these similar tables for the iron and steel industries of Great Britain; and multiply the total by a similar number of tables according to their degree for other great coal and iron producers such as Germany, United States, Belgium, and France; add also every available form of statistics for other parts of the globe, from the Arctic Circle to the Cape of Good Hope, and from Japan to Morocco, and one will readily admit that as a book of reference the work must be invaluable.

But the book is no mere statistical abstract. These tables, numerous as they are, are only scattered here and there throughout a very full and valuable text. The history of the rise and development of the great coal and iron industries here and elsewhere is stated briefly but sufficiently: the relative advantages and disadvantages of the several competing countries in their material and social aspects are well indicated: such questions as those of labour, increased or improved means of transport, the near prospects of such inventions as the Siemens-Martin and Bessemer steel processes, Barff's process for preventing iron from rusting, electric lighting, &c., are also discussed; and finally, the *literature* of the subjects dealt with is shown under each head at length, so that those whose special requirements demand more than even this work can give them, are at least shown where they may find the information they desire.

At a time of such profound depression in both these important industries, one naturally is eager to discover any rift among the clouds which the large accumulation of facts here contained, or the conclusions of so wide and an accurate an observer, may disclose. There is doubtless a consolation of a selfish kind to be got from observing that the state of affairs as to depreciation in the value of collieries and ironworks, in the prices of iron and coal, and in the wages of the workmen, is apparently quite as bad among our competitors as among ourselves. Thus the average price of coal in Germany was, in 1873, 10*s.* 9*d.*, and in 1877, 5*s.* 7*d.*; the wages, after rapidly rising, have now fallen back to the rates prevalent ten years ago, while the character of the workmen has deteriorated, and their relations to their employers have been changed in every way for the worse. In the iron trade the 125 joint-stock companies lost in 1877 alone a sum equal to 8·9 per cent. of their aggregate capital, which amounts to £24,335,709; and this capital at present represents, according to the quotation of the Berlin share-market, a current value of about £7,335,000 only. One regrets to notice that M. Pechar's chief remedy for these evils appears to be a return to protective tariffs so far as they have been abolished, and an aggravation of them where they exist. He is profoundly convinced that for many years to come no country in the world can hope to compete in the world's markets or even in their respective home-markets, with Great Britain in any but minor special products.

M. Pechar, however, has good hopes for the iron industry generally and consequently for the coal industry, whose fortunes depend so closely on those of iron. He expects that in most of the *present* applications of iron, steel will shortly supplant it, but that the iron industry will not therefore perish, but on the contrary will find new and larger spheres, partly as subsidiary to steel, partly by in its turn ousting wood and other substances from many of their present uses, as for example in buildings.

But it is unfair to summarise M. Pechar's conclusions apart from the many considerations by which he supports them, and we must therefore refer the reader on this and other points to the book itself. It will amply repay his perusal.

J. MARSHALL

OUR BOOK SHELF

Natural History, Sport, and Travel. By Edward Lockwood. (London: Allen and Co., 1878.)

MONGHYR is a large district in Bengal, divided into two nearly equal portions by the Ganges. Here Mr. Lockwood has spent many years as magistrate, and during that period had the inclination and the capacity to gather much knowledge of the district and its people. He laments in his preface that during twenty years' service he met only one Englishman (a Professor of Botany) who could identify the most common trees and plants. This is certainly lamentable, especially for the ignorant themselves, who thus miss a great and constant source of enjoyment and a fine opportunity of adding to a knowledge of the productions of one of our most important dependencies. This modest little volume is a good example of what may be done by a man who makes no pretensions to have more than a "desultory" knowledge of natural history. Mr. Lockwood is a very good observer, and his book contains many valuable notes on the animals and plants to be met with in the district of Monghyr. He has much

also to tell us about the people and their mode of life, the places of interest in the district, sporting experiences, and the various kinds of culture carried on. Altogether his volume is interesting and a distinct addition to our knowledge of the district over which its author ruled.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Weather, Past and to Come

SIMULTANEOUSLY with the appearance of some important remarks by Mr. Hyde Clarke, in the article "Sun-Spots and the Nile," NATURE, vol. xix. p. 300, I have been called up by a London clergyman of inquiring mind to answer the charge that a paragraph which he cut out of the *Times* last year, declaring on my alleged authority that that winter was to be severer in cold than any known for generations had been totally falsified by the event. I would request, therefore, Mr. Editor, a little space in your valuable pages for the following explanations:—

I give priority to Mr. Hyde Clarke, on account of his early labours in demonstrating a periodicity in human affairs, somewhat of the type of the sun-spot period subsequently discovered elsewhere. His remarks, too, now, of the probability of the existence of other periods of about 26 and 104 years, and that they "interfere," or mix up, with what he considers a ten-year period, are also worthy of note. In fact, they are the first public consent I have yet seen to my often insisted on conclusion from the Edinburgh earth thermometers, that the explanation of the eleven-year wave of heat exhibited there, being both immediately preceded and immediately followed by the deepest trough or wave of cold, for each whole eleven-year cycle on either side of it, was precisely caused by the near concurrence just there of two sets of waves of different periods of undulation. But when he goes on to say (line 38, p. 300) that such interference of two or more sets of undulations "prevented any absolute calculation as to the future," I object to the ruling of that sentence.

The complication may make the matter more difficult. It may oblige the State at last to set apart some good men for professionally prosecuting that subject, and to put them into a dungeon if they attend to anything else. But that is all! Two or three, even six or seven planets, pulling away at the earth and the earth at them in periods of different lengths and with different degrees of energy, do not prevent physical astronomers predicting the final outcome of it all on the earth's motion from day to day, and even minute to minute, and the alleged case of impossibility is only one of like kind.

But to the clergyman with the inquiring mind I would answer as follows:—

1. What I *did* write, in the summer of 1877, on the future weather is to be found at p. "25" of vol. xiv. of the "Edinburgh Astronomical Observations," a volume so liberally distributed by H.M. Government to scientific societies and libraries in London that no one there need have any difficulty in referring to my exact words if they are thought of consequence by any body.

2. Those words are, at all events, as to their general scope and bearing, widely different from the newspaper cutting alluded to. For while that treats only of a cold winter, my first and leading contention in the book was not about cold at all, but about heat. Namely, that the Edinburgh earth-temperature measures for forty years past show that a great *heat*-wave comes upon the earth from without, presumably from the sun, every eleven years, nearly; and that the date of the next such heat-wave was "1879·5, within limits of half a year each way." According to which the coming summer and autumn of this year may prove glowingly hot, and next winter unusually mild, in obedience to a grand cosmical action upon the earth as a whole. And who has yet disproved that?

3. My second, but only second and inferior, contention was, that such eleven-year heat-wave, of solar origination—contrary to the usual ideas of the learned as to the crest of a wave being removed from its trough or lowest point by about half of its length—was, in this case, both *immediately* preceded and *immediately* followed by a trough of extreme cold; the extremest cold, or lowest trough of each whole eleven years period on either side of the heat-wave's crest. Wherefore I contended (in 1877) that we had then still, between us and the good, warm time coming, a trough of extreme cold to wade through; and I did say that that preliminary cold-wave might be expected about 1878·0 "within limits of three-quarters of a year."

4. Because the winter of 1877-78 was not cold, and the winter of 1878-79 is now very cold, *in Great Britain*, the clergyman holds that my prediction was totally falsified. But to that conclusion I oppose the following consideration:—Is the surface of Great Britain large enough to be taken as expressing the condition of the whole globe under a cosmical influence from space without? Is not China much larger than the so-called Great Britain, and was not last winter preternaturally cold in China, with snow and ice down to the sea-coast even in lat. 29°, and inland such long-continued frosts and dry weather, that thence no crops, and the dreadful famine with depopulation of whole provinces?

Is not also the surface of North America larger than that of Great Britain; and at a central station of the former, Manitoba (as worthily reported and notified by Prof. E. D. Archibald in NATURE, vol. xix. p. 266) was not the December of the present winter astonishingly warm, almost hot, or no less than 25° above the mean temperature of former Decembers there?

5. Hence, if we look beyond our own immediate coasts, I suspect that the deficiency of radiation from the sun, called the cold trough, may have occurred, in reality, not far from the date I suggested in 1877, viz., in 1878·0. But as such influence from without has to act on the solid earth practically through the medium of an absorptive, locomotive, double revolving atmosphere, its full and extremest effects are experienced in different manners and at different dates in different parts of the earth.

Wherefore the "meteor" then becomes an affair for terrestrial meteorologists, not for astronomers, to follow up and explain; though the former may glean some useful hints from what the latter have long since ascertained as to the lunar tide-wave: viz., that it is raised, or coincides most nearly with a meridian full moon, near the middle of the Pacific; but at far different and later dates at other places, according to the length and difficulty of the path by which the tide-wave, once raised, has to travel to reach them. PIAZZI SMYTH

15, Royal Terrace, Edinburgh, February 1

Sun-Spots and the Plague

Apropos of the plague—I do not know whether the following curious coincidence has been noticed. In that admirable work, John Graunt's "Natural and Political Observations upon the Bills of Mortality" (second edition, London, 1662), which is probably the earliest treatise on vital statistics, I find the following statement (p. 31):—"There have been in London, within this age, four times of great mortality, that is to say, the years 1592 and 1593, 1603, 1625, and 1636." He shows that large numbers died of the plague in each of these years. Now, if we take the solar period to be 10½ years, nearly in accordance with Dr. Lamont's and Mr. J. A. Broun's estimates, we get the subjoined table, which sufficiently explains itself.

Corresponding solar years.		
1592·5	... Plague in London	1592–3.
1603	... ,, ,, ...	1603.
1613·5		
1624	... ,, ,, ...	1625.
1634·5	... ,, ,, ...	1636.
1645		
1655·5	... Great Plague in Naples	1656.
1666	... ,, of London	1665.
⋮		
1718·5	... ,, at Marseilles ...	1720.

If this particular coincidence has not already been pointed out, it deserves notice as supporting the theory that the rate of mortality is remotely connected with the solar period. There may be several chains of causation leading to the increase of mortality, but one chain is doubtless through the Asiatic famines, which would naturally develop the worst forms of germ disease.

W. STANLEY JEVONS

On the Combustion of Different Kinds of Fuel

I HAVE read with interest the criticism of Mr. I. Lowthian Bell (NATURE, vol. xix. p. 175) on my paper on the mode of combustion in the blast furnace hearth. You say with truth that the question is not simply technical, but is one of scientific importance. The prevailing opinions, which Mr. Bell has expressed with his usual force, rest upon experimental determinations of the gases in the hearth. I have never felt that trustworthy results have been obtained in any of the published analyses, and with your permission I would like to state the case, and see if my difficulties are removable by the wide experience of Mr. Bell or other investigators.

The blast-furnace hearth is a cylinder, closed at the bottom, but perforated near the top by a number of openings in which the tuyeres, or ends of the air blast-pipes are closely fitted. The air enters at a pressure which usually varies in anthracite practice between four and seven pounds to the square inch. As the discharge is at the top of the furnace, many feet higher, the air must describe a curved path from the point of entrance to the centre of the furnace, being acted upon continuously by a horizontal and a vertical force. It is evident that the level of the tuyeres is not the place to obtain the first products of combustion unless they are drawn through a tuyere in action. Elsewhere the samples would not be taken from the path of the air, which is upward from the tuyere from the instant it enters the hearth.

Mr. Bell and other investigators have analysed gases drawn from the hearth by means of porcelain tubes introduced through a closed tuyere aperture, or through holes drilled between the tuyeres. It seems to me these analyses are vitiated by the mode of drawing off the gas, and since this criticism applies to the experimental basis of existing views of combustion in confined spaces and with limited supplies of air, I will give a few details to show the scope of my objection.

At the Wear furnace Mr. Bell drew off gas through a tuyere that was closed for the purpose, but air was entering at other tuyeres on each side and four feet distant. Certainly this did not represent the product of that active combustion which takes place in the path of the air, but of these products after they had filtered through nearly four feet of glowing fuel. What the exact distance was depends upon the velocity of gas in the crucible of the Wear furnace and the inner diameter of the hearth, but was probably over three feet.

The quantity drawn off is not mentioned, but as it was taken for eudiometric analysis, the amount was probably less than five litres, and the movement of the gas through this glowing coal to the sample tube must have been extremely slow. Under these circumstances, whatever the product of combustion in the path of the air may have been, there could be only one gas drawn into the sample tube, and that would be carbonic oxide mixed with nitrogen. Even if we assume that the product of combustion in the furnace is carbonic anhydride alone (which is not true), this would be completely reduced to carbonic oxide by passing through the hot coal.

The experiments made on gas which was drawn through tubes in holes inserted between the tuyeres do not impress me more favourably. There the tubes were thrust "a little way into the contents of the furnace." That description does not apply to the mode in which gas samples were drawn off for analysis, but to experiments for testing the reducing powers of the gas by submitting pieces of ore to its action. Still it is probably also the mode in which samples were obtained, and the object of this note is to ascertain whether more careful means of sampling the unchanged products of combustion were used. If not, I submit that the analyses which form the basis of all modern reasoning on this subject must be rejected. The rapidity with which red hot carbon reduces carbonic anhydride, and produc s just the gas which experimenters find in their sample tubes, is well known, and methods of sampling which take no precautions to guard against this change cannot be accepted. I think the investigators owe it to science to give the world some hint of the means they have used to prevent this action, and to obtain the gas as it is formed. In the case of one of our American furnaces—a small on —the blast has an upward velocity of twenty feet per second at the level of the tuyeres, without considering the increase of volume by its rise in temperature in the furnace, and also allowing it to penetrate *instantly* to the centre, so as to cover the whole ar-a of the crucible. That cannot be true, and on the other hand the withdrawal of gas from the walls at the tuyere level, while the air is entering with great velocity four feet away, can hardly give a fair sample of the unaltered result of immediate combustion.

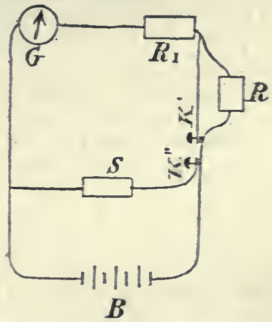
JOHN A. CHURCH

Columbus, Ohio, U.S.A., January 21

Internal Resistance

THE following method of measuring the internal resistance of a battery was devised some two years ago by Lieut. A. R. Conden, United States Navy, then attached to this station as Instructor in Electricity. It fulfils quite closely the conditions indicated by Clerk Maxwell on p. 412, vol. i. of his treatise on Electricity and Magnetism. As it is not generally known, I venture to call your attention to it.

In the figure, B is the battery, G a galvanometer, R and R_1 resistances, K″ a key for introducing the shunt S, and K′ another key for shunting out the resistance R.



When both keys are open the current through the galvanometer is—

$$S_1 = \frac{E}{B + G + R + R_1}.$$

When both keys are closed the current from the battery divides, part going through G and R_1, part through S. The current through the galvanometer is now—

$$S_2 = \frac{E}{B + \frac{(G + R_1)S}{G + R_1 + S}} \times \frac{S}{G + R_1 + S}.$$

If $S_1 = S_2$, then—

$$\frac{E}{B + G + R + R_1} = \frac{E}{B + \frac{(G + R_1)S}{G + R_1 + S}} \times \frac{S}{G + R_1 + S}.$$

Solving for B—

$$B = S\frac{R}{G + R_1}.$$

Finally, if R have been adjusted equal to $G + R_1$, then—

$$B = S \text{ directly.}$$

In practice R is a rheostat unplugged to equal $G + R_1$, and the two keys are combined in one. If the current be small enough with $G + R$ then R_1 may be omitted and R made equal to G. S is adjusted until, upon closing the double key, the deflection of the needle remains unchanged. The resistance of the battery is then the resistance of the shunt.

The case and concordance of the results obtained in this way through long series of measurements are no less striking than the rapidity with which the observations may be made.

Torpedo Station, Newport, R.I., January 12 — C. F. GOODRICH

The Formation of Mountains

MR. G. DARWIN has shown that, on the supposition that the earth is a cooling solid, the depth at which the maximum cooling and consequently, in all probability, the maximum contraction takes place, moves downwards, and, taking Sir W. Thomson's values of the constants, has not yet got down so far as 100 miles.

This shallowness of the layer hitherto chiefly affected is alone sufficient to prove how small an effect can be attributed to such a cause.

He inquires whether I may not have under-estimated the contraction of rock in cooling. In my calculation I put it at ·00007 linear for one degree Fahr. I derived this estimate from

the consideration of Mr. Mallet's experiments on cooling slag run from an iron furnace.[1]

This coefficient is somewhat larger than the mean of those obtained by Mr. Adie[2] for much lower temperatures. The mean of six of his values, half of them being for moist rock and half for dry, I find to be ·0000057.

Mr. Darwin recalls attention to M. Favre's experiments (out of which the present correspondence arose). M. Favre's experiments illustrate well the structure of an alpine district. But I would observe that, if ours is a cooling solid globe, and if that would give rise to such surface structure, we ought to find it everywhere, and not confined to definite geographical areas, as we do. O. FISHER

Harlton, Cambridge, February 8

Concerning the Colour of Eyes

MAY a portrait painter be allowed to remark that there are two kinds of green eyes, and the poets have duly appreciated both. The eye of the "green-eyed monster" is, no doubt, the cold grey, or stony blue eye, overspread with the yellow of biliousness, hence green; but when Dante called the eyes of the beatified Beatrice *emeralds* he did not mean to insult her. The image called up by his ecstatic words is that of those deep, soft eyes which are a warm brown in some lights—for instance with the light falling on them from one side only—and take a grey tint when facing the light of the sky, and green tints at other times, according to the lights that fall upon them; and are therefore sometimes a puzzle to portrait painters. Eyes, like the sea and precious stones, catch lights and transmute them. The sea is only green from the meeting of sunlight and blue sky light in it. J. M. H.

P.S.—Has it been remarked that the distinction between yellow and blue tints—the only one made by the colour-blind, according to Dr. Pole—is precisely the same as that made by the sun in photography: all the warm tints (as an artist—who makes the same distinction—would call those partaking of yellow) coming out darker, and all the cold ones—or those partaking of blue—lighter than in the object photographed?

Intellect in Brutes

A CORRESPONDENT in NATURE, vol. xix. p. 268, describes the actions of a water-rat which, he says, climbed up to a window-sill, inconvenient of access, and thirteen feet from the ground, in order to get some bread which was habitually put there for the birds during the cold weather. As the rat had never found food there before, the writer concludes that his conduct cannot be attributed either to instinct or to experience, but must be ascribed to a process of reasoning based on the observation of the flocking together of the birds, and the inference that they must be attracted by food. Now it seems to me that before we ascribe to a rat such complicated reasoning powers it is necessary to ask if there is no other, simpler, way of accounting for the phenomenon. I think there is. It is well known that different species of animals vary greatly in the acuteness of their senses. To man, sight is the most important sense, and the same is true of many other animals, and most birds. The cat is a representative of another, smaller, class of animals, whose most perfect organ of sense is the ear; while the dog lives in a world of sensations, the most important of which are contributed by the sense of smell. To this last class belongs the rat, which is noted for the acuteness of its scent. It is evident, therefore, that the water-rat in question was led to the window-sill by his nose, which, in his case, was a more trustworthy guide than his eyes would have been. I do not wish to deny, by any means, that animals have reasoning powers. On the contrary, I am convinced that human and brute intellect differ only in degree, not in kind; and I even adopt Haeckel's "cellular psychology," which attributes the elements of intellectual life—sensation and volition—to infusoria and organic cells in general, in opposition to the older "neural psychology," according to which psychic activity begins with the nervous system in the scale of animal life. But what we have to guard against is not to ascribe to animals reasoning powers of a higher type than is consistent with the development of their brain, especially when the actions which seem to postulate such powers can be readily accounted for by simply bearing in mind the extraordinary acuteness of one or more of their senses. We are altogether too prone to judge the intellectual life of animals by the human standard, to imagine that the eye is everywhere, as with us, the leading source of knowledge; and the neglect of the important *rôle* which the sense of smell plays in animal life has been particularly fruitful of errors in philosophical speculation. It has, among other things, helped to give a longer base of life to the old theory of instinct, regarded as a mysterious power of nature.

Berlin, February 8 HENRY T. FINCK

[1] *Trans.* Roy. Soc., paper read June 20, 1872.
[2] *Trans.* Roy. Soc. Edin., vol. xiii. p. 370.

Ear Affection

THE remarkable phenomenon described by your correspondent "P," in NATURE, vol. xix. p. 315, induces me to bring to your notice that precisely the same effect was produced in my own case a month ago, when partial deafness came on in both my ears, whilst suffering from congestion of the mucous membrane of the nasal passage and eustachian tube. Not being aware that any prior case had occurred of a distinct difference of a semitone, as indicated by the alternate application of a tuning-fork to the two ears, I at once drew up a memorandum on the subject, and handed it to Dr. Urban Pritchard, who was advising me. Like your correspondent "P," I have also noticed the double sound produced when I whistle, and more particularly when I close both ears with my fingers. G. L. WALLICH

February 11

Bees' Stings

THE American *Quarterly Microscopical Journal*, published last October in New York, contains an elaborate article on "The Sting of the Honey Bee," by J. D. Hyatt. Mr. Hyatt's experience does not tally with that of your correspondent, R. A. He says: "By allowing the bee to sting a soft piece of leather an excellent opportunity is offered for studying the action and mechanism, for the whole apparatus will be beautifully dissected, the *bee not appearing to be seriously injured by the loss*." I should be happy to send the journal to R. A. if I knew his address. W. RADFORD

Sidmouth

Electric Lighting

I NOTICE in an article in NATURE, vol. xix. p. 262, the following reference made to our electric light that it "does not appear to give very great satisfation through its fluctuation." It is true that at first we were caused some trouble owing to the Serrin lamp not working properly, but having overcome the difficulty we find it in our business, where it is necessary to show colours correctly, a very great improvement on all our former trials of lighting, and moreover, in its use we are not troubled in our galleries and upper floors with the heat and fumes which with gas alight no amount of ventilation seemed to remove.

It is not a pleasant light to read or write by owing to a certain flicker which seems common to all the regulators, but in warehouse or show-room use this does not cause any inconvenience, and we think in large places, especially those already having motive power, that it must eventually supersede gas.

Regent Street H. J. NICOLL

RELATION OF METEORITES TO COMETS[1]
II.

THERE are two classes of shooting stars which have been sometimes spoken of as unlike, but which are now admitted on all hands to be of common origin and character, namely, those which come in quantities on certain nights of the year, and give what is called a star shower, and the sporadic meteors, such as we can see on any clear night.

In November, 1799, von Humboldt saw during his travels in South America, a shower of shooting stars, and he has given a glowing description of the sight. These came on the morning of November 12. In 1832, November 13, there was seen in Europe a display of less brilliancy. It, however, attracted not a little atten-

[1] A lecture delivered in the Mechanics' Course at the Sheffield Scientific School of Yale College, U.S., by Prof. H. A. Newton. Continued from p. 317.

tion, as descriptions and newspaper notices show in every country in Europe. But no person seems to have connected it with any previous shower, nor does it appear that any one gave a hint of the true nature of the phenomenon.

The next year there appeared in this country, on the morning of November 13, a more brilliant shower, which some present doubtless witnessed. Through the morning hours of that day the stars shot across the sky like the flakes of snow in a snowstorm. Not a little difference was there in the way people looked at it. The negroes at the south thought the day of judgment had come. The owner of a plantation told me that his negroes had gathered in the "praise-house," and that he on being waked went down to quiet their fears. They had concluded not to call "Missus," as she would soon hear Gabriel's trumpet, and they well knew that she was ready to go. A student here in College was going to prayers, and saw a ball of light pass across the half lighted moving sky. He rubbed his eyes, thinking that something was the matter with them. A second flight made him sure that his eyes were troubled, and he looked down and hurried on to chapel. A servant girl by chance returning home in the early morning, saw it, but said nothing until it was talked of the next day. "Oh," said she, "I saw that." "Did you? Why did you not call us?" "Really, I didn't know but that the stars went out that way every morning." Prof. Twining saw it, and observing that all the flights were away from one point in the heavens, and that that point moved along with the stars as they rose in the morning sky, he said, "These are not, as some say, meteorological phenomena; they are not, as others say, electric; these are bodies coming to us from beyond the air, and they belong to astronomy." This was the first definite proof of the cosmic origin of meteors.

Nine hundred and thirty-one years earlier, that is, in the year 902, there was a like brilliant shower of fire. A cruel Aghlabite king then reigned at Tunis. He had driven the Christians out of Sicily, penning up the Bishop of Taormina and the remnant of his people in the church, and burning it and them together. He had crossed to the mainland, and was besieging Cosenza, then an important city of Calabria. He suddenly died, and the flying monks were relieved of their terrors. They connected his death with the star-shower which occurred at or near the same time, and in all the annals it is repeated in varied phrases that on the night when King Ibrahim Bin Ahmad died an infinite number of stars scattered themselves like rain to the right and left.

Between the years 902 and 1799 the November meteors were seen in unusual numbers in at least nine different years. The showers in the table which I show you are not selected out of an indefinite number in our histories. On the contrary, they are nearly all which we have found in the records as having occurred near that time of year.

Epochs of November Star-Showers

Year.	Day.
902	October 13
931	„ 16
934	„ 14
1002	„ 15
1101	„ 17
1202	„ 19
1366	„ 23
1533	„ 25
1602	„ 28
1698	November 9
1799	„ 12
1832	„ 13
1833	„ 13
1863–68	„ 14

Notice now in this table that the showers came either near the beginning or near the end of the first third, or else near the end of the second third of the century. In other words, they all come near the end of a cycle whose length was 33¼ years. Again, notice that the day of the month advanced with slight irregularity about three days in the century. The large advance of twelve days between 1602 and 1698 is due to the change of ten days in the reckoning in passing from old style to new style.

I have added, as you see, the six years from 1863 to 1868, in each of which, but especially in the latter three, these meteors came, as we had expected, on the morning of November 14. They seemed in all these years to pass, as they did in 1833, across the sky, as though going away from the constellation Leo, or rather from the sickle in Leo. This means that the small bodies really came into the air in parallel lines, the apparent radiation being the way in which parallel lines appear to us. There can be no doubt that there was the same parallelism of paths in all the earlier star-showers.

Here we have a group of solid bodies coming into the air all moving in one given direction. They come to us only on a particular time in the year, for the slow change from the middle of October to the middle of November can be explained. They come to us only at intervals of about a third of a century. These facts can only be satisfied by supposing that vast numbers of these small bodies are moving in a long thin stream around the sun, and that the earth, at the proper times, plunges through them taking into the air each time some scores of millions of them. Each of them must be moving in an orbit having the same period as every other, and approximately the same path.

Now it may be shown that there are but five orbits about the sun that can meet these conditions. Further than that, there is but one of these five that can explain the change of date from the middle of October to the middle of November, and this fifth one does explain the change perfectly. I cannot in the time you kindly grant me give in such detail that you can clearly understand them, the reasons for thus limiting the path of the meteoroids first to five possible orbits, and then to one of these five. I must ask you to accept the statement in view of the fact that no astronomer has, so far as I know, ever questioned the proofs of it.

That orbit is one which is described in 33¼ years. The meteoroids go out a little further than the planet Uranus, or about twenty times as far as the earth is from the sun. While they all describe nearly the same orbit they are not collected in one compact group. On the contrary they take four or five years to pass a given place in the orbit, and are to be thought of as a train several hundred millions of miles long, but only a few thousands of miles in thickness.

Now right along with this train of meteoroids travels a comet. It passed the place where we meet the meteoroid stream nearly a year before the great shower of 1866, and two or three years before the quite considerable displays of 1867 and 1868. It was therefore well towards the front in the great procession.

How came it that this comet and the meteoroids thus travel the same road—the comet with the meteroids and the meteoroids with each other? The plane of the comet's orbit might have cut the earth's orbit to correspond with any other day of the year than November 15. Or cutting it at this place the comet might have gone nearer to the sun or farther away. Or, satisfying these two conditions, it might have made any angle from zero to 180° instead of 167°. Or, satisfying all these, it might have had any other periodic time than 33¼ years; even then it might have gone off in any other direction of the plane than that in which the meteoroids were traveling. All these things did not happen by chance; there is something common.

The comet which I have named is not the only one that has an orbit common with meteoroids, though it is the only case in which the orbit of the meteoroids is

completely known aside from our knowledge of that of the comet. Every August, about the tenth day, we have an unusual number of meteors—a star-sprinkle, as it has been called. A comet whose period is about 125 years moves in the plane, and probably in a like orbit with these meteoroids.

So near the first of December we have had several star-showers—notably one in 1872—and these meteoroids are travelling nearly in the orbit of Biela's comet. In April, too, some showers have occurred which are thought to have had something to do with a known comet.

Thus much as to the meteors of the star-showers. The sporadic meteors are with good reason presumed to be (and observed facts prove some of them to be) the outliers of a large number of meteoroid streams, and the leading problem of meteor-science to-day is to find these streams so faintly shown, and, if possible, the comets they belong to.

Come back with me to the November stream and its comet. The several bodies move along a common path not at all by reason of a present physical connection. They are too far apart, in general a thousand times too far apart, to act on each other so much that we can measure the effect. No; their connection has been in the past. They must have had some common history.

Looking now at the comets, we see that they have been apparently growing smaller at successive returns. Halley's comet was much brighter in its earlier than in its later approaches to the sun. Biela's comet has divided into two, if not more than two, principal parts, and seems to have entirely gone to pieces. It could not be found in 1872, when and where it ought to have been visible. Several comets have had double or multiple nuclei. In the year 1366, in the week after the star-shower, a comet crossed the sky exactly in the track of the meteors. A second comet followed in the same path the week after. Both belonged no doubt to the November stream, and one of them may perhaps have been the comet of 1866.

This stream of meteoroids is a long thin one. In miniature it would be perhaps a mile long to an inch in thickness. We have crossed the stream at many places along a length of a thousand millions of miles, sometimes in advance of, and sometimes behind, the comet, and all along this length have found fragments, sometimes few, sometimes many. This form of the stream suggests continuous action producing it. A brief violent action *might* give this form, but a slowly acting cause seems more natural.

Again, in the history of Biela's comet we have distinct evidence of continued action. The comet divided into two parts not long before 1845, and yet in 1798 fragments of it were met with so far from the comet, that they must have left the comet long before, probably many centuries ago.

Thus are we led to say, *first*, that the periodic meteors of November, of August, of April, &c., are caused by solid fragments of certain known or unknown comets coming into our air; *secondly*, that the sporadic meteors such as we can see any clear night are the like fragments of other comets; *thirdly*, that the large fireballs are only larger fragments of the same kind; and *finally*, that this stone, which was broken off from one of those large fragments in coming through the air, must once have been *a part of a comet*.

Here I should naturally close, yet I am sure that you will ask, How came the comet to break up? Perhaps the prior question would be, How came the comet together? In its history there is much that we cannot yet explain, much about which we can only speculate. Thus, how came this stone to have its curious interior structure? As a mineral it resembles more the deepest fire-rocks than it does the outer crust of our earth. It seems to have been formed in some large mass, possibly in one larger than any of our existing comets. Some facts show that the comets have almost surely come to us from the stellar spaces. Out somewhere in the cold of space a condensing mass furnished heat for the making of this stone. The surrounding atmosphere was unlike ours, since some of these minerals could hardly have been made in the presence of the oxygen of our air. Either in cooling, or by some catastrophe, the rocky mass may have broken to pieces, so as to enter the solar system, having little or no cohesion, like a mass of pebbles; or, it may have come and probably did come, a single solid stone. In either case, as it got near to the sun, new and strong forces acted on it. The same heat and repulsion that develops and drives off from a comet in one direction a tail, sometimes a hundred millions of miles long, may have cracked off and scattered in another direction solid fragments. One of these contained in it this stone, and it wandered in its own orbit about the sun, itself an infinitesimal comet, how many thousands or millions of years we know not, until three years ago it came crashing through the air to the earth in Iowa. Thence this fragment came here to serve as a text to my discourse.

METEOROLOGICAL STATION ON BEN NEVIS

WE are glad to learn that the Scotch Meteorological Society's scheme of a station on Ben Nevis is evoking cordial support from those who have the administration of Government funds available for such objects. The London Meteorological Council, of which Prof. Henry Smith is chairman, has unanimously agreed to offer to the Scotch Society 100*l.* yearly towards the support of the station, provided a copy of the observations is sent regularly to London. This is at once testimony by the most competent judges to the importance of the scheme, and a proper encouragement to the Scotch Society to proceed in its spirited enterprise. We understand that to uphold the station and induce two competent observers to take it by turns to live on the top of the mountain with an assistant will cost about 300*l.* yearly. It is estimated that to purchase a full stock of instruments and erect a building for them and the observers a capital sum of 800*l.* will be required. The Scotch Society has applied for a grant of 400*l.* towards this expenditure from the Committee appointed by Government to distribute 4,000*l.* annually to encourage scientific research. We believe the Committee has not yet met to consider the various claims which are, no doubt, as usual made on the funds.

RESEARCH UNDER DIFFICULTIES

THE following short preface to a very valuable account of the stages of development from the egg of one of the centipedes (Geophilus), no member of which group had been studied previously to this account, gives so convincing a picture of the enthusiasm for investigation which may animate the modern naturalist, that it is worthy of a place in NATURE for the encouragement of the "craft." Elias Metschnikoff has during the past fifteen years worked more assiduously with the microscope at the observation of the minute details of embryology than any other student. To him we are indebted for our first accurate knowledge of this subject in the case of many important animal forms, *e.g.*, sponges, various jelly-fishes, marine worms, the scorpion, and the book-scorpions, various insects, crustaceans, starfishes, and ascidians. One result has been the injury of his eyesight. In reading to-day his memoir on Geophilus, published in 1875 (*Zeitschr. für wiss. Zoologie*), it occurred to me that the following passage has more than technical interest:—

"After having for many years sought in vain for material suited for the investigation of the embryology of the centipedes, I chanced to obtain a quantity of the eggs of

Geophilus. My find, however, took place under such circumstances, and these interfered so much with my investigation, that I feel justified in describing them more minutely. For some considerable time I had been afflicted with a chronic affection of the eyes, and consequently commenced in the spring of the present year a journey to our south-eastern steppes in order to turn my attention to anthropological studies. Instead of taking with me as in previous years all the apparatus necessary for microscopical research, I took this time on my journey only anthropological measuring instruments. When, then, I was in the neighbourhood of Manytsch, nearly in the heart of the Kalmuk steppes, and was visiting a small forest plantation, I discovered quite unexpectedly a number of eggs of Geophilus which had been deposited under the bark of a rotten tree-stem where the females were watching over them. I gathered up the precious material, and having packed it carefully in two bottles, set off with all speed to Astrachan, in order there to set about the microscopic investigation of the eggs. But when, after four days' travelling I arrived in a Russian village, Jandiki, near the shore of the Caspian Sea, and inspected my two bottles, I found in them only a couple of dead, opaque eggs, all the others having entirely disappeared. Fortunately I succeeded in Jandiki, where there is also a small plantation, in obtaining fresh material of the same kind, and this I brought in good condition to Astrachan, making the journey by steamboat. In the town of Astrachan I was able to borrow a Hartnack's microscope from a medical man practising there, and on a second journey took it with me to Jandiki. In this way I was enabled to make out the chief features of the developmental history of Geophilus by the use of my less seriously affected left eye. At the same time, in spite of the very favourable character of the Geophilus-eggs for microscopic research, I could not bring my work to the desired degree of completeness."

Determination and pluck have their scope in embryology! E. Ray Lankester

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ON THE RECENT ERUPTION AND PRESENT CONDITION OF VESUVIUS

At the end of the great eruption of 1872 the crater of Vesuvius was left as a wide and deep abyss, the floor of which did not possess a very high temperature, and was free from fumarole. Gradually, however, fumarole appeared, the temperature increased, and large quantities of steam and carbonic acid were evolved. The temperature continued to increase and sulphurous acid made its appearance, finally in 1875 the evolution of carbonic acid diminished, and that of hydrochloric acid commenced. This is always the commencement of the highest stage of fumarole activity. In January, 1875, when I ascended the mountain, large quantities of sulphurous acid were being evolved, and it was quite impossible to descend into the crater. On December 18, 1875, a deep chasm opened in the bottom of the crater, at the bottom of which glowing lava could be seen. This was the commencement of a new period of eruption, which Palmieri predicted would last a long time, and which is still going on. The lava gradually rose to the top of the chasm, and a new eruptive cone was soon afterwards formed on the floor of the great crater. Small quantities of lava issued from time to time from the new cone, and spread over the interior of the crater, until on the night of November 1, 1878, it rose to the lowest portion of the edge of the crater, and began to flow down the great cone of Vesuvius in a north-westerly direction. The lava continued to flow in a somewhat intermittent manner until nearly the end of the year, but it did not go beyond the foot of the cone.

On December 29 last I visited the new cone. I left Naples at 8.45 A.M., drove to Portici, and walked to Resina. Left Resina on foot at 10 A.M., came upon the lava of 1451 (according to the guide, but I suspect it was lava of 1631) at 10.30, then bore somewhat to the west, and struck the lava of 1858. Reached the observatory at 11.15 A.M., the foot of the cone at 11.45, and the summit of the cone at 12.40 P.M. Thus the ascent of the cone occupied fifty-five minutes, including about ten minutes of rest. The angle is approximately 32°, and the ash of which the cone is composed is very loose. On arriving at the summit we turned to the west, and walked along the edge of the great crater, until we came to its south-western extremity, beyond which it is broken down by the recent flow of lava. Then we descended the crater by a very precipitous path, and presently found ourselves upon the new lava, surrounded on three sides by precipitous walls of apparently not more than 100 feet in height. Facing due north-east, we had on our right the new cone of November, 1878, and on our left the stream of lava which had issued from it, and which was still very hot, and in some places could be seen to be red hot a little distance beneath the surface. Occasionally a puff of very hot air was blown into our faces from the hotter portions of the lava. In many places hot fumes of hydrochloric acid escaped from the lava, and great cavities (in one or two cases small caverns), from whence the hot acid vapours issued, were coated with brilliant red and yellow sublimates of sesquichloride of iron. These sublimates are constantly spoken of as sulphur. I am inclined to assert that in more than ninety-nine cases out of a hundred, they are sesquichloride of iron formed either by direct sublimation of previously formed chloride from lower recesses in the lava bed, or by the action taking place then and there of the hot hydrochloric acid upon the exposed surfaces of the lava. Sublimations of salt were also apparent in certain portions of the lava bed. Prof. Palmieri informs me that he has detected sulphates in the sublimates, also lithium and boracic acid. I have not yet had time to examine various specimens of sublimates, which were collected from the new lava, and were placed in a dry bottle as soon as I reached Naples.

Prof. Palmieri has kindly furnished me with a MS. account of "Il Vesuvio dopo la grande eruzione del 1872," from which some of the above facts were derived. My own recent experience on the mountain does not, however, allow me to agree with him when he says: "Comunque sia, in tutto il tempo trascorso il cratere ha di mostrato poca attivita dinamica. Pochi brani di lava gettati fino all' altezza di 20 o 30 metri, soffii piu o meno vigorosi e qualche rara detonazione han rappresentato il vigore della forza eruttiva." The new cone, when I saw it, was pouring out vast volumes of smoke and steam, detonations occurred at frequent intervals, and loud noises as if of the lava surging within the crater. At intervals, also, the smoke was intensely illuminated as if the lava had leapt up within the cone. The cone discharged a perpetual shower of red-hot pieces of lava of a more or less cindery character, and certainly to a height far exceeding the "20 o 30 metri" of Prof. Palmieri. It is difficult to judge of heights under such circumstances, but many of the fragments appeared to be projected to a height equal to that of an ordinary sky-rocket. The ejected masses nearly all fell on one side of the cone, and helped to raise it. Occasionally, however, a sudden burst would come which scattered the red-hot masses in all directions. We approached as near as we could to the cone, and stood upon the bank of cinders (*vide* the accompanying woodcut) in immediate contact with it, and not a dozen yards from its vomiting crater. Showers of red-hot stones were projected from the crater, many of which fell into it again, and the rest for the most part on the side remote from us. There came a sudden burst, however, which shook the ground under our feet, and scat-

tered red-hot masses in all directions. A piece weighing four ounces fell within six feet of where I was standing, and the guide ran up to it and pressed a copper coin upon its still soft surface. A few minutes later a piece of red-hot lava, weighing at least seven times as much as the preceding, fell within four feet of me, and I promptly retired to a safer distance. Fifteen days before a guide had been killed by a falling red-hot stone from the crater. The projectiles from the crater are doubly dangerous, because you cannot "dodge" them. They do not come down straight like a cricket-ball, but waver in their flight like a boomerang. In the case of the larger of the two masses which fell so near to me, I had not only time before it fell to watch it in the air above my head, but also to speculate as to where it would fall. Judging by its position when about forty feet from the ground, it would certainly, I thought, fall behind me; a moment later it swerved, and fell about four feet in front of me. The cone, with its lurid smoke, and loud detonations, and showers of red-hot stones, presented a most fascinating spectacle. What, then, must be the effect when the whole great cone of Vesuvius is in a like condition?

The new lava is very leucitic, and does not resemble that of 1872. When in a viscous state it can easily be drawn into threads, and when cold it is jet-black and possesses a fine lustre.

Chloride of ammonium does not appear to have been at all a common product in this eruption, although it was conspicuously present during the eruption of 1872. Great differences of opinion still exist as to the formation of sublimates of chloride of ammonium in lavas. Bunsen considers that it is mainly formed by the action of the hot lava upon vegetable soil, and he has proved that "a square metre of meadow land yields on dry distillation a quantity of ammonia corresponding to 223·3 grammes of chloride of ammonium." Palmieri, while he admits that he has found more chloride of ammonium in those portions of lavas which have passed over cultivated ground, asserts that he has also found it high upon Vesuvius far above the range of vegetation, and in localities where the new lava has simply flowed over older and perfectly barren lava fields. He accounts for its formation by supposing that aqueous vapour undergoes dissociation in the heated crevices of the lava, and that the nascent hydrogen combines with the nitrogen of the air to form ammonia. We do not know what chemists will have to say to this theory.

New Eruptive Cone within the Crater of Vesuvius, which opened on November 2, 1878, and is still active.

Not far from the active cone I found a very interesting specimen of volcanic cinder which had obviously been exposed to the action of hydrochloric acid at a very elevated temperature, and had then probably been ejected before the action was complete. The central portions consisted of undecomposed cinder, and this was surrounded by a thick layer of perfectly white decomposed substance consisting chiefly of silicate of alumina and silica; the hot hydrochloric acid having formed sesquichloride of iron with the iron in the superficial layers of the mass, which sesquichloride had been afterwards volatilised out of the mass. By passing hydrochloric acid over lava heated to redness in a porcelain tube, the same effect was produced, the portions of lava most strongly heated, and longest submitted to the action of the hydrochloric acid, became perfectly white, while a copious sublimate of chloride of iron and chloride of aluminium passed into the receiver.

I ascended from the new lava (viz., from the bottom of the great crater of Vesuvius, *vide* the foreground of the accompanying woodcut) at 1.30 P.M., ran down the sides of the great cone, which had taken fifty-five minutes to climb, in seven minutes, reached the observatory at 2.30 P.M.; Portici, by a roundabout way to the west near Monte Somma, whither we went to search for minerals, at 4.30 P.M.; and Naples at 5.40. The next evening

while steaming out of the bay, *en route* for Tunis, I noticed that the smoke at the apex of the mountain was ruddy from the reflection of the lava within the small crater of 1878, and then for many days after, the summit of the mountain was obscured by clouds, and snow lay upon it when I next saw it towards the middle of last January. G. F. RODWELL

POPULAR NATURAL HISTORY[1]

VOLUME II. of this handsomely illustrated work on natural history is equally well got up as the first, which we noticed some months ago; it contains brief histories of the Carnivora, Cetacea, Sirenia, Proboscidia, Hyracoidea, and Ungulata.

The terrestrial, or on-the-land-living Carnivores, are described by Mr. Kitchen Parker, assisted by his son Jeffery. The father's pleasant style and his power of apt illustration will be recognised in the too few pages introductory to this group, and some of the woodcuts are from drawings made by the author. The marine Carnivora, the Whales, and the Sirenia, are described by Dr. Murie, while the editor, with the assistance of Prof. Garrod and Mr. Oakley, describes the Proboscidia, the Hyracoidea, and the Ungulata.

The land carnivora are, undoubtedly, as Mr. Parker tells us, one of the most compact as well as one of the most interesting groups among the mammalia. So many of the animals contained in it have become "familiar in our mouths as household words," bearing as they do an

FIG. 1.—The Binturong (*Arctictis binturong*).

important part in fable, in travel, and even in history. Many of them are of wonderful beauty, and many of them are of terrible ferocity. Two she-bears out of the wood tore up the forty and two naughty mocking little children near Bethel, and the narrative thereof frightens our own little children to this day. Packs of enraged lions, "fierce with dark keeping," were by the noble Romans let loose to mangle and devour helpless men and women in the arena, and as for the wolf, what terrible stories are not told about him? He was the very dread of the shepherd in the far distant times. As Mr. Parker reminds us, his bad character for ferocity was so well known in the early days, that "a very old sheep-master, addressing his sons on his death-bed, these sons being eleven out of twelve of them shepherds, said, knowing they would understand him, of the youngest, 'Benjamin shall raven as a wolf: in the morning he shall devour the prey, and at night he shall divide the spoil.'"

And with all this ferociousness of character, it is from among the number of the land carnivora that man has selected his faithful and devoted follower the dog. For a wonderfully interesting account of this friend of our race, a friend in whom, as Mr. Darwin observes, it is scarcely possible to doubt but that the love of man has become an instinct, an instinct, as Mr. Parker naively observes, not as yet certainly developed in man—there is a pleasant chapter, one that tells of what is known of prehistoric dogs, of the origin of the dog, and of the many varieties of the dog.

As an illustration of the general character of the woodcuts which so profusely adorn the volume, we have

[1] "Cassell's Natural History." Edited by P. Martin Duncan, M.B., F.R.S., Professor of Geology, King's College, London. Vol. ii. Illustrated. (London, Paris, and New York: Cassell, Petter, and Galpin.) 4to.

selected one of an interesting animal which has been a great puzzle to the systematic zoologist (Fig. 1).

"The Binturong (*Arctictis binturong*) is a curious little animal of a black colour, with a white border to its ears; it has a large head and a turned-up nose; its tail is immensely long, thick, and tapering, and which is very remarkable, it is prehensile, like that of a new world monkey. It is from twenty-eight to thirty inches in length from the snout to the root of its tail, and the tail itself is nearly the same length. It is quite nocturnal, solitary, and arboreal in its habits. In creeping along the larger branches, it is aided by its prehensile tail. It is omnivorous, eating small animals, birds, insects, and fruits. Its howl is loud. It walks entirely on the soles

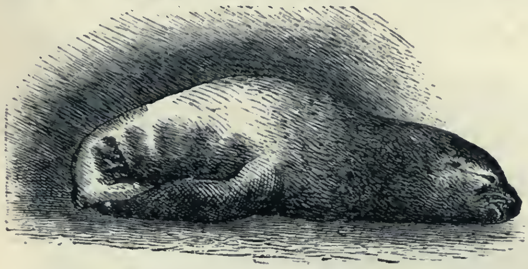


FIG. 2.—The Seal asleep.

of its feet, and its claws are not retractile. While it is wild and retiring in its manner, it is said to be easily tamed. It is placed by Mr. Parker among the group of the civets."

In his description of the fur and hair seals, Dr. Murie, as was to be expected, is quite at home, and we have, among other accounts of these wonderful creatures, a long one of that sea lion which lived so long in the London Gardens. This animal seemed to pass its time between sleeping and eating, and we give two out of a series of illustrations which depict its habits—one of it when fast asleep (Fig. 2), the other when it is in "a watchful attitude," waiting to be fed (Fig. 3); it was well known to all visitors to the gardens. It was in the habit of devouring upwards of twenty-five pounds' weight of fish every day, and not thinking this too much. It was originally captured in

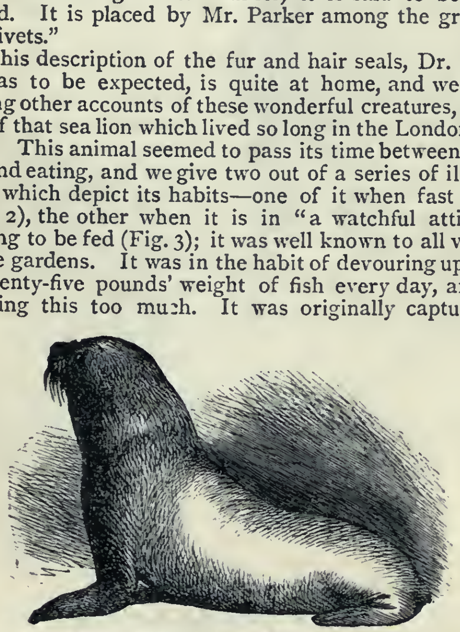


FIG. 3.—Waiting to be fed.

the neighbourhood of Cape Horn; and François Lecomte, the French sailor into whose possession it fell, exhibited the animal for a short time at Buenos Ayres before bringing it to London, where for a short time he earned a living by showing it off. By kindness and dint of training he taught it to become quite a performer in its way. It mounted a ladder with perfect ease, and it could descend either head or tail foremost, so that it seemed a marvel of docility, and its appearance in London seems to have created quite a general interest in the group hitherto so little studied of the eared seals.

The volume concludes with an account of the non-ruminating members of the even-toed sub-order of the Ungulates, embracing the pigs of the Old World, the peccaries of the New World, and the hippopotami. The next volume will contain a description of the Ruminants, a large and very interesting group. E. P. W.

THE KEITH MEDAL OF THE ROYAL SOCIETY OF EDINBURGH.

ON the 3rd inst., at a meeting of the Royal Society of Edinburgh, the President, Professor Kelland, in presenting the Keith Medal which had been awarded by the Council to Professor Heddle, of St. Andrews, delivered the following address:—"Professor Heddle—I am here to-night to exemplify a remark which is often made, that to insure success in an address, such as I am about to deliver, the best way is to commit the charge of it to one absolutely ignorant of the subject. No false pride will then stand in the way of the best sources of information, nor will any undue admixture of half knowledge clog and darken the truth. For every particular contained in these remarks, then, I at once unhesitatingly acknowledge myself indebted to Professor Geikie. When I first became acquainted with this Society, forty years ago, there used to frequent our meetings men who had the reputation of being mineralogists rather than geologists—Lord Greenock, Allan, and perhaps Jameson himself. That race has now died out, and with them mineralogy, as a distinct science, has all but lain dormant amongst us. During the preceding quarter of a century that science had flourished nowhere more vigorously than in Edinburgh. Professor Jameson introduced the definiteness of system of the Freyberg School, and infused into his pupils such a love of minerals that numerous private cabinets were formed; while under his fostering care the University Museum grew into a large and admirable series. One of my first acts as Professor in the University was to vote out of the Reid Fund, which had just come into our hands, a large sum (some thousands) to pay back moneys expended on minerals throughout a series of years preceding. During these years, Geology, as the science is now understood, hardly existed. For, as the nature and importance of the organic remains embedded in rocks became recognised, their enormous value in the elucidation of geological problems gradually drew observers away from the study of minerals. Consequently, as Palæontology increased, Mineralogy waned among us. To such an extent was the study of minerals neglected, that geologists, even of high reputation, could not distinguish many ordinary varieties. But, as a knowledge of rocks presupposes an acquaintance more or less extensive with minerals, the neglect of mineralogy reacted most disadvantageously on that domain of geology which deals with the composition and structure of rocks. The nomenclature of the rocks of Britain sank into a state of confusion, from which it is now only beginning to recover. To you, Professor Heddle, belongs the merit of having almost alone upheld the mineralogical reputation of your native country during these long years of depression. You have devoted your life to the study, and have made more analyses of minerals than any other observer in Britain. You have not contented yourself with determining their composition and their names; you have gone into almost every parish in the more mountainous regions, have searched them out in their native localities, and, by this means, have studied their geological relations, treasuring up evidences from which to reason regarding their origin and history. After thirty years of continuous work, you have communicated the results of your labours to this Society. For the first two of these papers on the Rhombohedral Carbonates and on the Feldspars, in which you have greatly extended our knowledge of pseudomorphic change among minerals, enunciating a law of the shrinkage so frequently resulting therefrom, the Society proposes now to express its gratitude to you. The value of your papers is undoubted.

Through the kindness of Mr. Milne Home, I have been favoured with the sight of letters addressed to you by four eminent mineralogists, Dana of America, Rammelsberg of Berlin, Szabo of Buda-Pesth, and King of Queen's College, Galway. Szabo states that the notice of your paper on the Feldspars, which appeared in Groth's *Zeitschrift für Mineralogie*, greatly interests him, and makes him desirous of placing himself in direct communication with the author. Dana says, 'I have read your paper on the Feldspars, in the Transactions of the Royal Society of Edinburgh, with great satisfaction. Your thorough method of work leads towards important results of great geological, as well as mineralogical value.'

"I have the satisfaction, in the name of the Council of this Society, of presenting you with the Keith Medal. It is hoped that this recognition of your labours will not be without encouragement to you in the arduous researches in which you are engaged."

OUR ASTRONOMICAL COLUMN

TEMPEL'S COMET, 1867 II.—Now that Brorsen's comet of short period is again under observation, the next comet of the same class to be sought for, is that discovered by Dr. Tempel at Marseilles, on April 3, 1867, which was also observed at its next appearance in 1873; it is probable there may be greater difficulty in recovering this object, than appears to have been the case with Brorsen's comet, the reason for which may be made clearer if we briefly detail its history since the year 1867. Less than a month after it was discovered in that year the deviation of the orbit from a parabola became evident, and several of the German astronomers, Prof. Bruhns, now Director of the Observatory of Leipsic, in the first instance, deduced elliptical orbits, with periods of between five and six years. The most complete investigations on the motion of the comet in this year were due to Dr. Sandberg and Mr. Searle. The comet was observed by Dr. Julius Schmidt at Athens till August 21, and the perihelion passage having taken place on May 23, a considerable arc of the orbit was included within the limits of visibility. Dr. Sandberg, after taking into account the effect of planetary perturbations during the comet's appearance, found the period of revolution 2,080 days. On examining the track of this body with reference to the orbits of the planets, it was seen that near the aphelion it must approach very near to the orbit of Jupiter, the least distance being within 0·37 of the mean distance of the earth from the sun, and from the position of this great planet near the time of aphelion passage of the comet early in 1870, it was obvious that great perturbations in the elements of the latter must ensue, and without at least an approximate knowledge of their amount, there might be difficulty in recovering the comet at its next return to perihelion. The first publication of results in this direction was by Mr. W. E. Plummer, from Mr. Bishop's observatory, Twickenham, in February, 1873, followed shortly afterwards by particulars of similar independent investigations undertaken by Dr. Seeliger, of Leipsic, and the late Dr. von Asten, of Pulkowa. It was found that the effect of the attraction of Jupiter, which planet was only 0·32 distant from the comet on January 20, 1870, caused a retrograde change in the longitude of the node to the amount of 22½°, and increased the inclination of the orbital plane to the ecliptic nearly 3°; the period of revolution was lengthened by more than three months, and the point of nearest approach to the sun was removed further from him by upwards of 0·2 of the earth's mean distance. Changes in the elements to this amount would of course entirely alter the track of the comet in 1873, but they had been so closely determined, that immediately after receiving an ephemeris in which their effect was included, M. Stephan re-detected the comet at Marseilles, and as early as April 3, or five weeks before the perihelion passage, and it was observed until the last week in June. We subjoin Dr. Sandberg's elements for the two appearances:—

	1867.	1873.
Perihelion passage	May 23·9204 G.M.T.	May 9·0134 G.M.T.
	° ′ ″	° ′ ″
Long. of perihelion ...	236 9 24	237 38 42
,, ascending node	101 10 10	78 44 39
Inclination to ecliptic...	6 24 36	9 44 13
Angle of excentricity...	30 38 39	27 30 58
Log. semi-axis major ...	0·503658	0·517057
Revolution	2080·1 days.	2178·6 days.

The longitudes are reckoned from the mean equinox of the commencement of the respective years. The period of revolution applies to the ellipse which the comet was describing at perihelion passage.

With regard to the length of the actual revolution, it is certain that no very material perturbation can result from known causes: Jupiter, the great disturber of the cometary motions, was at almost his greatest possible distance from the comet when the latter passed nearest to his path about May, 1876. A recent investigation by M. Raoul Gautier, of which, however, no details are yet published, assigns a longer period of revolution corresponding to the perihelion passage in 1873 than was given by Dr. Sandberg, the difference being about 10½ days, so that if the mean motion at the last appearance does not, as M. Gautier implies, admit of exact determination, there will be an uncertainty in the date of the approaching perihelion passage, which will necessitate a pretty extended and careful search in order to detect the comet. It belongs to the fainter class, and although in 1867 and 1873 it appeared under rather favourable circumstances for observation, and is likely to do so to a certain extent this year, it has never been a good telescopic object. When at its greatest intensity of light early in May, 1867, its nucleus, which was stellar, had not the brightness of a star of the ninth magnitude. At the last observation at Athens, in that year the theoretical intensity of light was 0·21, and the comet would have the same degree of brightness about March 27 next, whether we assume the time of perihelion passage (perturbation neglected) from the orbit of Dr. Sandberg or M. Gautier, but the uncertainty of position may probably delay its rediscovery till some time later. We may hope that the publication of the further results of M. Gautier, who mentions being engaged on the calculation of perturbations during the present revolution, will not be long deferred. It is desirable the comet should be under observation as long as practicable at this return, since the period being now nearly equal to half that of Jupiter, the two bodies will come into proximity again towards the month of November, 1881, though their mutual distance may not be less than 0·55. This will involve a new, strict investigation similar to those undertaken in 1873, to enable the epoch of ensuing perihelion passage to be ascertained.

Using Dr. Sandberg's orbit of 1873, the comet's place at Greenwich midnight, on March 27, would be in R.A. 253°·9, N.P.D. 103°·7, or, if the perihelion passage be assumed ten days later, which would more nearly accord with M. Gautier's calculation in R.A. 247°·0, N.P.D. 101°·0.

THE INTRA-MERCURIAL PLANET QUESTION.—It appears that this subject has lately engaged the attention of that excellent practical astronomer Dr. Oppölzer, of Vienna, who has communicated to the *Astronomische Nachrichten* some curious results of his examination of the records of rapidly-moving dark spots upon the sun's disk. His inquiry resolves itself simply into the conclusion, that even introducing rather more extended data than were used by Leverrier, who, it will be remembered, found several possible periods of revolution for the hypothetical planet, they may all be represented so far as regards the necessity of a transit across the sun's disk on

the days named, by an orbit, the elements of which Dr. Oppölzer gives as follows :—

Epoch 1850, January 1·0, Paris M.T.

Mean anomaly	356° 0′	
Longitude of perihelion	27 45	Equinox of 1850.
,, ,, ascending node	178 0	
Inclination to ecliptic	7 0	
Angle of excentricity	14 13 or $e = 0·2456$	
Log. semi-axis major	9·0906	
Mean diurnal motion	22°·789529	

The period of revolution would therefore be 15·797 days. Comparing with the observations employed the following are the differences shown by the above orbit in geocentric longitude; the calculated geocentric latitude is annexed :—

	Diff. longitude.	Latitude.
1800, March 29, Fritsch	+ 0°·6	+ 14′
1802, Oct. 10, ,, ...	+ 0·4	− 14
1819, Oct. 9, Stark	+ 0·2	− 13
1839, Oct. 2, Decuppis	+ 0·5	− 7
1849, March 12, Sidebotham ...	− 0·8	− 7
1857, Sept. 12, Ohrt	+ 0·1	+ 7
1859, March 26, Lescarbault ...	0·0	+ 10
1862, March 20, Lummis	+ 0·1	+ 2

But it is unfortunate that notwithstanding this almost perfect representation of the longitudes assumed and the circumstance that the latitudes point to a transit across the sun's disk on every date, there are apparently fatal objections to our admitting the existence of a planet with these elements, several of which are pointed out by Dr. Oppölzer. It may be sufficient to mention here the first of them :—With so short a period and small inclination, a transit across the sun's disk would occur every year, and we know that observation by no means supports such a condition. However, the existence of a body moving in this orbit will admit, as Dr. Oppölzer states, of very early decision :—On March 18 a nearly central transit should occur—

Ingress at 18h 8m Berlin M.T. ...	Angle of position	74°
Egress at 23 15 ,, ...	,,	254

We give these particulars as affording another illustration of the difficulties attending any trustworthy inferences from the observations of suspicious spots upon the sun's disk. The above orbit, it should be mentioned, will not accord with either of Prof. Watson's presumed planets, though possibly, by increasing the excentricity, elements might be found which would agree with one or other of his positions, while representing most of the observations used by Dr. Oppölzer. We ignore the idea of a want of *bona fides* on the part of the observers on so many occasions, but there is still to be remembered the fact that comets have traversed the sun's disk, and with small perihelion distances might do so without our discovering them except in the course of transit, supposing them to possess the degree of condensation which some have indicated. The object observed by M. Coumbary at Constantinople in May, 1865, could only have been a comet, with a perihelion distance so small as, like the great comet of 1843, almost to graze the sun's surface.

GEOGRAPHICAL NOTES

AT the meeting of the Geographical Society on Monday, when the Earl of Dufferin occupied the presidential chair for the second and last time, Mr. T. J. Comber, of the Baptist Missionary Society, who is about to lead an expedition above the Yellala Falls of the Congo, gave some account of his explorations inland from Mount Cameroons, in the course of which he visited a district of country previously unknown, and discovered a small lake to the northward of the mountain; he also ascertained that there was a broad valley there instead of a continuous mountain range. He mentioned one fact with regard to Mount Cameroons, which goes far to show that there may be some difficulty in finding a suitable spot for the proposed missionary sanatorium. When at an elevation of 2,000 feet there were such heavy mists, that, although he slept between two fires, his blanket was wet through. It is, of course, possible that here, as in the Neilgherry Hills, and other places, positions may be found to which the miasmatic influences do not extend. Mr. Comber next gave some brief and interesting notes of a journey which he made through Congo to Makuta, the place which Lieut. Grandy saw from the brow of a neighbouring hill, but was not allowed to enter. Sir Henry Barkly afterwards read some observations on the Bamangwata country in South Africa, to which a melancholy interest attached from the fact of their having been drawn up by the late Capt. R. R. Patterson, who recently met with his death by poison when some three days' journey from the Victoria Falls of the Zambesi. The country would appear to be of a not very promising nature, for its soil is sand, covered with stunted bush, and there are few mountain fastnesses, except those near Shoshong, the capital; in the winter it is badly watered, as the Limpopo, Zambesi, and Zouga (or lake river), are the only rivers which run continuously, while the Tati, Shasha, and Makalapogo, are sand rivers. The open country is sparsely inhabited by Veld-people of two classes, the Bakala and Masarwa, of whom the former enjoy the right of possessing cattle and gardens, but the latter neither; they are, in fact, slaves, living on game and roots. The Bamangwata country is ruled by a chief named Khame, whom Capt. Patterson described as a very good man, an opinion in which Sir H. Barkly concurred.

IN connection with the meeting on Monday at Preston in reference to a Central African railway, the letter in yesterday's *Times* from the Alexandria correspondent of that paper is of interest. It seems that the Khedive had some time ago devised an excellent scheme for bringing the riches of the great lake district to the outer world by way of the Indian Ocean, at the mouth of the Juba river. The distance between that point and Victoria Nyanza is only 280 miles, and McKillop Pasha was instructed to work gradually from the coast, planting colonial stations at regular distances, while Gordon Pasha was to cooperate from the lake side. Though the plan seems to have been ably, but too secretly devised by the Khedive, it rather unfortunately fell through, we think on account of the jealousy of the Sultan of Zanzibar and his friends. There is no talk by the Khedive of a railway, and we think with the *Times* correspondent, that something more elementary should be attempted, with a country so totally undeveloped as that of Africa. That it will be opened to trade soon by some nation is evident. English, Germans, Italians, French, Portuguese, are all striving from various points. There is plenty of room for all.

A LARGE amount of material for arriving at some approximately correct notion of the mean depth of the sea, has accumulated in recent years. In a note to the Göttingen Academy, Dr. Krümmel has lately attempted this, in view of the vague and variable statements on the subject in text-books. Soundings were wanting for the Antarctic and a part of the North Polar Sea, *i.e.*, about 475,000 square miles, or 7 per cent. of the entire sea-surface, so that he gives his estimate only as a closer approximation. He estimates, then, the mean depth of the sea as 1,877 fathoms, or 3,432 metres, or 0·4624 geographical miles. It was natural to compare the mean height of dry land above the sea-level. Humboldt's estimate of 308 metres is regarded as quite out of date. Leipoldt has since estimated the mean height of Europe as 300 metres. Accepting this number for Europe, 500 for Asia and Africa, 330 for America, and 250 for

Australia, Dr. Krümmel obtains the mean of 420 metres, or 0·0566 miles. The surface-ratio of land to water being considered 1 : 2.75, the volume of all dry land above the sea-level is inferred to be 140,086 cubic miles, and the volume of the sea 3,138,000 cubic miles. Thus the ratio of the volumes of land and water is 1 : 22·4. That is, the continents, so far as they are above the sea-level, might be contained 22·4 times over in the sea-basin. Reckoning, however, the mass of solid land from the level of the sea-bottom, the former would be contained only 2·443 times in the sea-space. Dr. Krümmel also compares the masses (taking recent data); he finds that of the sea 3,229,700 cubic miles, and that of the solid land 3,211,310 (a small difference). If the specific gravity of the land were raised merely from 2·5 to 2·51432, we should thus have perfect equilibrium. Such equilibrium is probably the fact.

NEWS has been received from Moscow that Colonel Grodekoff, of the Russian general staff, has returned to that place from a somewhat venturesome expedition in Central Asia, during which he travelled in European clothes, and without any attempt at disguise. He was accompanied by a Kirghiz and two Persians, and traversed the northern part of Afghanistan, reaching Persia by way of Herat.

IN connection with the Russian scheme for a railway from Orenburg to Tashkent, it is stated that the Grand Duke Nicholas is preparing a third expedition for 1879, which is to set out in the end of March. After having passed Tashkent and Samarkand, it will cross the Amu and pursue its researches to the defile of Bamian, in Afghanistan, in the direction of Kabul. The explorers will then descend the Amu in a native boat, from the meridian of Balkh to Khiva, for the purpose of investigating the navigation of that river. From Khiva they will follow the ancient bed of the river to its old mouth in the Caspian.

CAPT. HOWGATE has presented to the United States Congress a supplementary note on the advantages arising from the creation of a polar colony on the border of the great palæocrystic ocean. The whaling interest is fast on the decrease in the States; the total value of imports being only two million dollars instead of ten millions twenty years ago. This deficit has been attributed by Agassiz and other competent authorities to the whales taking refuge in that almost inaccessible polar basin, to which, by the creation of a civilised station at Lady Franklin Bay, access might be gained.

THE Conference on the civilisation of Africa held a meeting recently at Brussels under the presidency of the King of the Belgians. It is stated that Mr. Stanley, who was present, "will be placed at the head of the approaching Belgian Exploring Expedition to Africa."

PETERMANN'S *Mittheilungen* for February contains a detailed account, with map, of Dr. Woeikoff's travels through central and southern Japan in 1878. In connection with a narrative of the discovery of the island Einsamkeit to the north-east of Novaya Zemlya is a map of the island showing its configuration and relative position. Prof. Hann contributes a short paper on the climate at the Victoria Nyanza on the basis of data collected by Dr. Emin Bey and the Rev. Mr. Wilson.

THE January number of the *Bolletino* of the Italian Geographical Society contains a long letter from Lieut. Bové, who accompanies Nordenskjöld's expedition; it was written from the mouth of the Lena, and gives many important details of observations made up to that point.

THE December *Bulletin* of the Paris Geographical Society contains a valuable sketch of the work done in Sumatra by the Dutch expedition, which started in the beginning of 1877 under the late M. Santwoort, and of which we have from time to time given news. The sketch is by Col. Venteggio. The number contains also the addresses by M. Huber in presenting the medals for 1878 to Mr. Stanley, M. Vivien de St. Martin, and Dr. Harmand.

A YOUNG Austrian painter, Herr Joseph Ladein, of Mödling near Vienna, has recently started for a tour through Central Africa. In a letter dated from Oran he states that his intention is to proceed through Marocco, to cross the Great Desert to the Senegal River, then to turn eastward to Haussa and the Nile Lakes, and to return to Europe along the course of the Nile.

A BRANCH of the new Berlin "Society for Commercial Geography and the furtherance of German Interests Abroad" has been established at Leipzig.

M. L. BABÉ has announced to the Paris Geographical Society that he proposes to explore the globe by means of an improved Montgolfier balloon, capable of storing heat in all regions and of maintaining a sufficient height for several weeks.

BAD news has been received from Zanzibar by the French Geographical Society. It appears that two of the French missionaries who were exploring this part of Africa have died, one of them by illness, and the other having been killed by a lion.

NOTES

AT a full meeting of the Council of the Zoological Society, held on the 5th inst. at the Society's office, in Hanover Square, Prof. William Henry Flower, F.R.S., Conservator of the Museum of the Royal College of Surgeons, was unanimously elected president of the Society, in succession to the late Marquis of Tweeddale. The new president, who, we need hardly inform the readers of NATURE, is one of the most learned zoologists and anatomists of the present day, has been for some years on the Council of the Society and one of its vice-presidents, and has communicated many valuable memoirs to its *Transactions* and *Proceedings*. Prof. Flower is the seventh president elected since the foundation of the Society in 1826. Sir Stamford Raffles, the first president, who died a few months after the foundation of the Society, was succeeded by the Marquis of Lansdowne, who resigned in February, 1831, in favour of the thirteenth Earl of Derby, then Lord Stanley. He held the presidentship for upwards of twenty years, and on his death, in 1851, was succeeded by the late Prince Consort. On the death of the Prince Consort, in 1861, Sir George Clerk, of Penicuik, was chosen as his successor, and retained the presidentship until his death, in 1867. He was succeeded in January, 1868, by the late Lord Tweeddale, then Viscount Walden, whose death has caused the vacancy to which Prof. Flower has succeeded.

THE Chemical Society have received from the executors of the late Mr. Sydney Ellis a legacy of 1,000*l*. free of duty.

THE friends of Prof. Clifford, who has been compelled by ill health to relinquish active work and reside in Madeira, are anxious to present him with a substantial testimonial in public recognition of his great scientific and literary attainments. At a meeting held at the Royal Institution, Albemarle Street, on Friday, January 31, Dr. William Spottiswoode, President of the Royal Society, in the chair, it was resolved that a fund should be raised for the above-mentioned purpose, and that the sums received should be placed in the hands of trustees, for the benefit of Prof. Clifford and his family. Contributions may be paid to the account of the "Clifford Testimonial Fund," with Messrs. Robarts, Lubbock, and Co., or to either of the Honorary Secretaries. Among the gentlemen who have kindly consented to act on the General Committee are the following:—Dr. William Spottiswoode, Dr. Andrew Clark, Prof. R. B. Clifton, F.R.S., Prof. T. H. Huxley, Prof. Henry Morley, Prof. A. Newton, F.R.S., Sir Fred. Pollock, Bart., Prof. Roscoe, F.R.S., Prof. H. J. S. Smith, F.R.S., Hon. Mr. Justice Stephen, Sir Henry

Thompson, Prof. John Tyndall, D.C.L., F.R.S., Prof. Alex. W. Williamson, Ph.D., F.R.S. Dr. William Spottiswoode, Pres. Roy. Soc., and Sir John Lubbock, Bart., M.P., F.R.S., are Joint Hon. Treasurers and Trustees.

A GRANT of 75*l.* has been made from the Worts (Cambridge) Travelling Scholars' Fund to John Edward Marr, B.A., St. John's, to enable him to travel in Bohemia and collect evidence and specimens bearing upon the question of the classification of the Cambrian and Silurian rocks, with the understanding that specimens be sent by him to the University, accompanied by reports which may be hereafter published.

WE have already referred to the fact that the inhabitants of Heilbronn, desirous of paying due respect to the memory of Dr. Julius Robert Mayer, who was born there, have resolved to erect a suitable memorial on the spot where he lived, laboured, and died. They invite co-operation, and, with the view of enabling the admirers of Dr. Mayer in England to join in this tribute of recognition, some of the most eminent men of science in England have agreed to form a Mayer-Memorial Committee; the list is headed by Dr. William Spottiswoode, Pres. R.S. Subscriptions exceeding 1*l.* may be sent by cheque to Messrs. Robarts, Lubbock, and Co., 15, Lombard Street. Smaller sums may be sent by Post Office Order to the Hon. Secretary, T. Archer Hirst, Royal Naval College, Greenwich.

M. LÉON LALANNE, Director of the School of Ponts et Chaussées, has been elected to fill the place of the late M. Bienaymé, in the Paris Academy of Sciences.

WE are glad to see that several important and much-needed reforms are being introduced into the British Museum. The Museum is now open free to the public on every week day—Monday till Friday from 10 o'clock, and on Saturday from 12 o'clock till the ordinary hour of closure. Special arrangements have been made to enable students to carry on their work without interruption. Students of natural history will have Tuesday and Thursday reserved for their studies, students of archæology Wednesday and Friday. On Monday and Saturday the public will be able to view the whole of the collections; on Tuesday and Thursday, all except the natural history specimens; and on Wednesday and Friday, all except the Greek and Roman sculptures, and antiquities in the upper gallery. Persons holding tickets of admission to the reading-room, the department of prints and drawings, the sculpture galleries, and the departments of natural history will not be required to renew them every six months, as the tickets will be granted to readers and students without limit of term, but subject to withdrawal. The actual presentation of the ticket will not be considered necessary for entrance into the reading-room. These and several other new arrangements seem to show that the Museum powers have at last come to the conclusion that the institution exists for the benefit of the public, and that their convenience ought to be made paramount in all arrangements.

THE obituary list of foreign men of science is heavy this week. We much regret to announce the death, on January 24, of Dr. Heinrich Geissler, the celebrated inventor in the field of physical mechanics. Dr. Geissler died at Bonn at the age of sixty-five years. Amongst his inventions are the well-known Geissler tubes, the vaporimeter, the mercury-pump, &c. We regret to have to record the deaths of two other eminent German men of science, viz., that of Dr. Eduard Lösche, Professor of Physics at the Royal Polytechnic Institution of Dresden, and well known naturalist, who died on January 25, aged fifty-eight; and that of Dr. Benedikt Stilling, of Cassel, whose name was but recently mentioned as one of the hon. secretaries at last year's meeting of the German Association. Dr. Stilling was an eminent anatomist; he was born at Kirchheim, near Marburg, on February 22, 1810, and died at the same place on January 28, aged sixty-nine years. The death is announced of Dr. Steinheil, the eminent optician of Munich. M. Paul Gervais, the distinguished palæontologist and Professor at the Paris Jardin des Plantes, died on Monday in his sixty-third year.

Apropos of the statue to Gauss, the following extracts from a Brunswick correspondent may interest some of the readers of NATURE. The sum collected, including contributions of 3,000 marks each from the Emperor of Germany and the Duke of Brunswick, promised subscriptions and interests, has now reached 41,000 marks. Herr Schaper has almost, if he has not quite, finished the statue; the casting in bronze is then to be superintended by Prof. Howaldt of Brunswick. A Berlin firm will provide the pedestal of red granite from Sweden. The statue will be one and a half life size, standing eight feet four inches (Rhenish measure), and the pedestal will be of about the same height. It is hoped that the unveiling of the statue will take place on April 30, 1880—the anniversary of Gauss's birth.

A CONCURRENT resolution of thanks to Prof. Hayden for his "accurate and comprehensive survey of the State of Colorado" passed the Colorado legislature, January 14. Senator Gaussion, who is himself an eminent mining engineer, remarked—"These reports, coming from a scientific and authoritative source, do more to answer and satisfy the inquiries of capitalists than everything else. They tell the world what the great western country is made of. The western domain of the United States is to-day the glory of the nation."

WE are asked to state that supplemental meetings, for the reading and discussion of papers by students of the Institution of Civil Engineers, have been appointed for the following Friday evenings:—February 14, 21, 28, and March 7. The papers to be read on these evenings are respectively: "The Excavating of a Tunnel in Rock by Hand Labour and by Machinery," by John C. Mackay: "The Design and Construction of Wrought-Iron Tied Arches," by Percy W. Britton; "The Cost and Construction of a Cheap Light Railway," by Alfred W. Szlumper; and "The Interlocking of Points and Signals and the Electric Block System," by George D. Marston. The chair will be taken at 7 o'clock on each evening, and successively by Mr. Hayter, Mr. Barlow, F.R.S., Mr. Bruce, and Mr. Woods, Members of Council.

SOME recent experiments by Herr Holmgren in Prof. Kühne's laboratory in Heidelberg seem to prove that the variations of the retina current through the action of light have no essential relation to the blanching and regeneration of the so-called "visual purple," thus increasing the difficulty of regarding that purple as directly connected with vision. In one experiment an eye of a newly killed frog was kept half an hour to an hour in sunlight, till all the visual purple was blanched. Variations of the retina current on incidence of light were found to occur in the ordinary way. Again, one eye of a rabbit was protected against the action of light by sewing the ear over it, while the other eye was exposed to the light. The animal showed normal variations of the retina current, and when the eyes were examined, with the necessary precautions, the normal amount of purple was found in the covered eye, while the other, which gave variations of the current, was quite without purple. Conversely, eyes of frogs and rabbits were taken out and treated with alum solution in the dark. The visual purple was thus maintained twenty-four hours, and it was then exposed to light. The light blanched it, like that of a fresh eye, but there was no trace of variation of current in consequence of this action. The author's conclusion regarding vision finds support in the facts that visual purple is wanting in some animals, which must be supposed capable of sight, and that it is absent from the yellow spot in man, and so from that part of the retina in which vision is most distinct.

IN a recent series of demonstrations at La Salpêtrière, Paris, Prof. Charcot has shown, *inter alia*, that it is possible to produce in one subject a state of catalepsy on one side of the body, and a simultaneous state of lethargy on the other. The patient is first thrown into catalepsy by looking at the electric light (in this state the limbs are supple and will retain any position one chooses to give them). To produce the state of lethargy or somnambulism on one side, it is sufficient to close the corresponding eye, or shut off the light with a screen; the two states are then co-existent on the two sides of the body.

WE learn from the *North China Herald* that at the December meeting of the Shanghai branch of the Royal Asiatic Society, a paper was read by M. A. A. Fauvel, on the alligators of China, on which occasion the author exhibited a live specimen obtained last October from Chinkiang, on the Yangtsze-kiang, as well as a cranium and skeleton, and a stuffed crocodile for comparison. The paper commenced with a philological dissertation on the names by which the saurians of China have been known at various times, and the specimen exhibited (between 5 and 6 feet long) was identified with the *t'o* or *ngo* of the old writers. Among modern writers, the late Mr. Swinhoe seems to have been the first to allude to its existence, and in 1869 a specimen was exhibited in the Chinese city of Shanghai. Père Heude more recently nearly became the possessor of a specimen which he only lost through his servant haggling over the price. At various times reports have reached Shanghai of crocodile-like animals being seen in the Yangtsze-kiang, but Mr. L. E. Palm, of the Chinese Customs' service, was the first to obtain a genuine specimen, which was presented to the Society last year. A careful examination soon showed, according to M. Fauvel, "that it was no crocodile, but a genuine alligator, a most interesting fact, as hitherto no alligator has been met with in the Old World, the genus being supposed to be confined to the rivers of America." M. Fauvel then explained from the specimens, and by means of careful drawings, the peculiarities of the genus. The Chinese animal seemed to resemble most the *Alligator lucius* of the Mississippi, but, as it seemed to belong to a distinct species, he proposed for it the specific title of "*sinensis*," until further research should establish or disprove the distinction.

THE *North China Herald* publishes some notes of a journey made during last October by the Rev. J. S. Crossette, in the northern part of the Chinese province of Shantung, in the course of which he visited a cotton-growing district. His account of this industry is not very encouraging. The scraggy little shrub, he writes, not as high as the knee, black as if blasted by mildew or killed by frost, looks very different from that in the cotton-fields of the United States; and in his opinion the introduction of some foreign cotton-seed would effect a great improvement.

ACCORDING to the *Democrat*, San Francisco is to be the first city whose streets are to be lighted entirely by the electric light. It is proposed to divide the city into districts, varying in extent from one to three miles, each of which will be fed by a sufficiently powerful Gramme machine. The machines have already arrived at New York, and arrangements have been made for adopting two or three different patents. The *Electrician* says:—"The experimental trial of the electric light at the works being executed at the port of Havre has given complete satisfaction. Without waiting for the report of the committee nominated by the French Board of Works, the Chamber of Commerce has authorised the establishment of ten lights in the outer port."

IN his last report to the Foreign Office, H.M.'s Consul at Tamsui, Formosa, calls attention to one or two features of interest in connection with the camphor trade there, which is assuming considerable proportions. Formosa, it is well known, is one of the very few districts in the world which produce camphor, the others being Japan and some parts of the Malay Archipelago. The tree from which it is obtained (*Camphora officinarum*), is said not to be known on the mainland of China; camphor, at any rate, is not extracted from it there. The camphor-producing tree of the Malay Archipelago differs from that growing in Formosa and Japan, and, in addition to the crude camphor, produces a valuable medicinal gum, known to merchants as "camphor baroos," the duty on which in China is about 6*s*. per pound. The Formosan tree, on the contrary, does not produce this gum.

AS showing the much greater attention gradually being directed to Singapore and the Malay Peninsula, to which we have from time to time alluded, it will not be uninteresting to record that some land at Tanjong Pagar, which, not a great number of years ago, was bought from the East India Company at the rate of 1 rupee for fifteen acres, was recently sold at prices ranging from 20,000 dols. to 30,000 dols. per acre.

OWING to the melting of mountain snows and torrential rains, the Lake of Geneva reached a higher level last month than it has been known to attain at this season in the forty-one years during which its fluctuations have been noticed.

IT is rumoured that M. Ferdinand de Lesseps will be appointed Governor-General of Algeria.

M. MAURICE SAND has discovered and published an account of a "pre-historic flint-implement workshop" near the village of Loges, on the frontier of the Indre and the Cher. On the summit of a heath, almost on a level with the surface, he has picked up hundreds of fragments of flints, convex on one side, concave on the other; also lanceolated axe-heads, arrow-heads, a jasper axe, and many scrapers in red and white cornelian or sardonyx. The flint-layers cropping out to the surface bear marks of human labour.

THE collection of maps, plans, and views of London and Westminster, made by the late Mr. Frederick Crace, and lent to the South Kensington Museum by his son, Mr. J. G. Crace, is now on view from 10 to 4 daily, in two of the upper rooms in the galleries on the west side of the Horticultural Gardens. The plans and views selected from the collection for exhibition are 3,085 in number. A complete catalogue, compiled from Mr. Crace's larger work, has been issued by the Stationary Office.

NEW GUINEA is rapidly ceasing to be *terra incognita*. Thanks to both scientific and missionary enterprise we have lately learnt much both of its geography and ethnology. Recently, and in this country alone we have had the results of the experience of D'Albertis, Comrie, Kiehl, Lawes, Moresby, Stone, and Turner. In the *Chronicle of the London Missionary Society*, October, 1878, we have, under the title "New Guinea," an account of the journeys made by the Rev. Jas. Chalmers. He is stated to have visited one hundred and five villages on the south coast of the mainland during the spring of last year, and ninety of these villages are reported as being visited for the first time by a white man. From Mr. Chalmers's general ethnological notes we learn that these natives cook the heads of their slain enemies to secure clean skulls to place on sacred places. Each family has a sacred place where offerings are carried to the spirits of deceased ancestors. This ancestor worship seems to be carried on with great fear. The spirits of their dead are invoked at the commencement of planting, at starting on trading expeditions, &c. They have one great spirit—Palaku Bara, who dwells in the mountains. Pigs are always killed in the sacred place and offered to the spirit, after which the carcase is carried back to the village to be divided and eaten. During this expe-

dition the entire coast line from Keppel Point to McFarlane Harbour was traversed on foot.

PERHAPS no branch of anthropology has been more cultivated or yielded larger results during the last few years than that relating to prehistoric man. At the same time no inconsiderable part of the evidence has been derived from the examination of osseous and other remains in caves both in England and on the continent. The greatest interest has always been felt in the revelations that might accrue to the science of anthropology by an investigation of the bone-caves of Borneo, situated as that island is on the confines of the lost continent Lemuria, where Dr. Peschel thinks it possible that the first appearance of man may have taken place. In his late presidential address to the Anthropological Institute, Mr. Evans referred to the fact that Mr. Everett, a well-known naturalist, had undertaken to devote a twelvemonth to the prosecution of cave-researches in Borneo. Mr. Everett commenced his researches in October last, and the principal proceeds from more or less extensive excavations in several caves are now on their way to this country. Mr. Everett's first quarterly report had just been received by Mr. Evans, in which the discovery is reported of numerous mammalian remains, the age of which has still, of course, to be determined, and also of remains of a race of men of whom no local tradition seems to be extant, and who habitually used the caves of Upper Saráwak either as domiciles, or as places of sepulture, or possibly for other purposes. Though unknown to history or tradition, this race of men appear to have been acquainted with the use of manufactured iron, so that, probably, no great antiquity is to be assigned to the remains already discovered. Mr. Evans stated that at least 100*l*. has still to be forthcoming for the exploration fund, in addition to what has been already subscribed, and that he would be happy to receive subscriptions.

THE magnetic behaviour of iron in the form of powder has lately been investigated by Prof. von Waltenhofen, of Prague. Three samples of finely pulverised and chemically pure iron, filling well-closed glass tubes, were magnetised by means of spirals of wire, through which were sent currents of increasing intensity, and the magnetic moments thus produced were measured. A comparison of these with those excited in equally heavy iron and steel bars by equal magnetising forces shows that the specific magnetisability of pulverised iron is not only much smaller than that of coherent iron, but even smaller than that of the hardest kind of steel known, viz., glass-hard Wolfram steel. Prof. Waltenhofen seeks an explanation of this in the circumstance that the magnetic mutual action of the polar molecules, which strengthens the action of exterior magnetising forces, is greatly lessened through the comparatively great intervals between the particles of the powdered iron; and the numbers he obtained led him to the conclusion that the electromagnetism of an iron bar is to be regarded only in the least part as due to direct action of the magnetising current, and mainly due to that reciprocal action of the molecular magnets.

IN his ninth Bridgewater treatise, Mr. Babbage refers to the possibility of constructing an automaton which would play the simple game of tit-tat-to (or "oughts and crosses"). Such a machine (probably the first ever constructed), working on the principle of a mechanical table, has been made by Mr. Freeland of Philadelphia. It is described in the January number of the *Journal of the Franklin Institute*, and was exhibited at the Institute on October 16 last year. It is now at the University of Pennsylvania, where, since its final adjustment, it has played a large number of games without losing a single one.

EARTHQUAKES are reported from Waldkirch and Buchholz, in Baden, on January 26, at 10 P.M. Both villages are situated in the Elz Valley, on the slopes of the Kandel Mountain, which measures 1,380 metres in height. A violent shock was also felt in the Swiss Canton of Uri on January 24, at 2 A.M.

THE Italian Secretary for Agriculture, Industry, and Commerce, has offered a prize of 3,000 lire (about 115*l*.) for the best monograph on the cultivation, growth, and diseases of the species *citrus* (the common lemon-tree). The competition will last until May, 1881, and all particulars may be learnt direct from the Secretary's office at Rome.

A VERY satisfactory Report comes to us from the Free Library Committee of Dundee. Under the care of Mr. Maclachlan, the library, in the quality of its contents and its organisation, is becoming one of the first of the kind in the kingdom, and is evidently well appreciated by the busy and inquisitive workers of Dundee. Scientific works have a large share of attention, and the museum, we are glad to see, is rapidly extending, and is likely ere long to be worthy of one of our great commercial centres. We have received a carefully compiled fourth supplement to the catalogue of the Library.

THE additions to the Zoological Society's Gardens during the past week include two Slender Loris (*Loris gracilis*) from Ceylon, presented by Mr. Leith Bonhâte; a Common Rhea (*Rhea americana*) from South America, presented by Major Venables; a Purple-crested Touracou (*Corythaix porphyreolopha*) from East Africa, presented by the Rev. J. A. Gould; a Geoffroy's Dove (*Peristera geoffroii*), bred in the Gardens.

OXYGEN IN THE SUN[1]

THE paper referred to appeared in the October number of this Journal.[2] A cursory glance at it gives the impression that the methods had been carefully criticised beforehand, that the experiments had been made with minute accuracy and that the results were trustworthy; but closer examination of it raises most serious questions on all the points mentioned. Errors of method and of experiment appear which make it quite impossible to accept the conclusions reached. It is the purpose of this note to point out some of these.

In the first place the author throughout the paper confounds Ångström's scale numbers with wave-lengths. Thus, for example, p. 257, he says, line 18, the photographs were "in sections of eighty to one hundred wave-lengths," line 24, "each wave-length being five millimeters in extent," and line 34, "each section of one hundred or more wave-lengths;" p. 258, line 24, "Error amounting to half a wave-length could therefore exist in the position of a line, according as it fell on one side or the other of a figure on the scale expressing a wave-length;" p. 259, line 18, one iron line "to every eleven wave-lengths was used;" p. 261, line 18, no element gives "a line within two or three-tenths of a wave-length of that position;" p. 264, line 16, "no other element furnishes a line which falls on the same wave-length." In proof that he really means scale divisions, he gives a section of his chart on p. 259, and says, line 38, "On the first space below the line is the scale of wave-lengths, each wave-length being five millimeters in extent;" and p. 260, line 28, "in the eighteen wave-lengths represented in the diagram." There are eighteen scale divisions in the diagram, each scale division being five millimeters long. Again, p. 264, referring to the coincidence of oxygen and solar lines in his table, he says, line 2, "in four, the difference is only five one-hundredths of a wave-length; in twenty-two, ten one-hundredths of a wave-length; in four, fifteen one-hundredths of a wave-length; in eleven, twenty-one one-hundredths of a wave-length; and in the remainder, the greatest difference is only thirty-five one-hundredths of a wave-length." Referring to the table, the four lines first mentioned are given as 3982·75, 4075·50, 4345·15, and 4483·80; the corresponding oxygen lines being given as 3982·70, 4075·45, 4345·20, and 4483·75. The difference is obviously five one-hundredths of a scale division, not of a wave-

[1] Note on J. C. Draper's paper "On the Presence of Dark Lines in the Solar Spectrum which correspond closely to the Lines of the Spectrum of Oxygen." From the *American Journal of Science* for February. Communicated by the Author.

[2] See NATURE, vol. xviii. p. 654.

length. From the fact that this error runs through the entire paper, it would almost seem as if the author was not aware of the distinction between wave-lengths and scale numbers. Using Ångström's scale he confuses wave-lengths with ten millionths of a millimeter; whereas in the case of D for example, the wave-length is nearly 6,000 times greater. If the author really means what he says, he asserts that the wave-length of the mean ray of the spectrum is one two-hundred and fifty-millionth of an inch instead of about one forty-thousandth, as we know it is.

Second, the author deems it of the greatest importance in the preparation of his solar photographs to use reflected rays exclusively; saying, p. 256, last line, "*at no time did the solar rays pass through glass*; all error that might arise during refraction was thus avoided." After this virtual condemnation of the use of refraction at all, he not only uses for comparison Ångström's wave-lengths made with achromatic lenses and a refracting grating, constructing even his chart upon them as a basis, p. 258, line 7, "the values assigned to the wave-lengths in this chart are those of Ångström"; but the very spectrum of oxygen by which the coincidence of the lines of this element with those of the sun spectrum were to be established, was photographed with glass prisms and achromatic lenses.

Third, the author states that the prisms with which the spectrum of oxygen was photographed were adjusted "to the minimum deviation of D'." Supposing D_1 to be meant, this precaution, which gives the appearance of extraordinary accuracy to the adjustment, is practically an impossibility with the apparatus employed. Minimum deviation of the D line as a whole could not under these circumstances be distinguished from that of either of its components, nor could that of D_1 be distinguished from that of D_2. Moreover, it is difficult to understand why he adjusts to minimum deviation for D' and not for G, near which the work is to be done. Instead of D', the line for which his apparatus was adjusted should have been chosen in the photographic portion of the spectrum, for example between G and H.

Fourth, on page 265, line 25, the author says that this "is a problem not to be solved by the comparison of two spectra of small dispersion." Hence it is a matter of some surprise to find that in getting his oxygen spectrum, he uses only "two flint glass prisms of 60," and for objectives, "achromatics of ten inches focus." The bright line spectrum of oxygen taken by Henry Draper, which the author in this paper inferentially attacks, was made, as we find on examination, with a direct vision battery of nine prisms and an observing telescope of forty-two inches focal length. The original negatives taken with the latter apparatus must consequently have been eight or nine times as long as the author's; and even these were none too large for the proper solution of the question.

Fifth, the author seems to have attempted to compare together a diffraction spectrum of the sun with a prismatic spectrum of oxygen. Such a comparison, by the method adopted, is manifestly of no value. Owing to the irrationality of dispersion of various refractive media it is an extremely difficult thing to compare accurately two prismatic spectra of different kinds. But the matter rises to an absurdity when a comparison is attempted between a grating spectrum and a prism spectrum. The graphic method, employed to supplement the direct method, does not appear to help the comparison, since the author nowhere gives both co-ordinates to the curve constructed.

Sixth, it is more than questionable whether the measurements of the solar lines actually made by the author are capable of the accuracy he assigns to them. The values in his table of wave-lengths are given to one hundredth of a division of Ångström's scale. As the author says on p. 257 that each division of this scale, which is one millimeter, was enlarged to five millimeters upon the paper scale on which the photographs were projected, to measure to one hundredth of a scale-division would require the measurements on the screen to be made to one-twentieth of a millimeter or the one five-hundredth of an inch, about; a degree of refinement highly improbable under these conditions. Moreover the accuracy of the results of such measurements is seriously impaired by the variation in the position of the lines on the screen, due to the fact that the large number of negatives (eight or nine apparently), required to give the whole photographic spectrum, must, unless special precaution was taken (of which there is no evidence), have been made with glass of different thicknesses. When projected in the lantern, this variation in thickness would necessitate a change in focus and so cause a change in the magnifying power. The smaller sizes of photographic glass vary in thickness from one to two millimeters. Consequently the displacement of the lines due to the difference of magnifying power arising from this cause would exceed considerably the limit of measurement, which, as above stated, was the one five-hundredth of an inch. But another and a more serious cause of inaccuracy must here be pointed out. From the data given by the author, it may readily be calculated that his original photographs of the oxygen spectrum, taken with two prisms of 60° and with lenses of ten inches focus, could not have been over half an inch long in the region from G to H. Since Ångström's chart from G to H is sixteen inches long, the author's spectrum would have to be magnified thirty-two times to make it the size of this. But as each millimeter of Ångström's scale was made five millimeters on the author's scale of measurement, the original negative as thrown on the screen must have been magnified one hundred and sixty diameters. Any one who has worked at all in spectrum photography, knows that it is utterly futile for purposes of measurement to magnify a photograph taken under these circumstances, as much as this, since then the size of the silver grains becomes larger than the details of the picture. In the absence of any precise statement the reader has to make the calculation for himself; but the figures above given cannot be far astray.

Seventh, there is only an appearance of accuracy when the attempt is made to fix the position of the oxygen spectrum lines to hundredths of one of Ångström's scale divisions. The projection method by which his solar lines were measured, has already been proved inadequate. And as to the method of graphical interpolation, used as auxiliary to the lantern, it does not appear that, as used by the author, it was capable of any such accuracy as that claimed. In constructing the curve, the iron lines are taken with Ångström's values for the wave-lengths; but these, though estimated to tenths, were read only to whole divisions of the scale. Moreover, only forty-seven iron lines were used in all, or one to every eleven scale divisions; the reading being to one one-hundredth of a scale division, or 1,100 numbers to one iron line. Since the author measured no wave-lengths directly, he was obliged to construct a considerable "portion of the curve from the wave-lengths of oxygen and air lines already given by various authorities." These values were taken, p. 258, from Watt's "Index of Spectra." On referring to this book, the values are given only to the units place. And even then, discrepancies amounting to from three to five entire units, or from three hundred to five hundred times the author's limit, appear in the wave-length as given by the various authors relied on for the measurements employed in the paper before us.

Eighth, the author nowhere states the peculiar character of the lines in the oxygen spectrum and appears not to know that they have any. He has apparently taken it for granted that the lines of oxygen are intrinsically as sharp as the lines of the solar spectrum. But this, at least in many instances, is known not to be the case. Consequently it is quite impossible to measure the oxygen lines as accurately as the solar lines, and even these, as has been shown, cannot be measured to the accuracy which the author claims. Ångström himself admits that there may be an error of one-tenth of a division in his scale numbers.

It would seem sufficiently obvious from what has been said that the results given in this paper are entirely vitiated by the errors of method and of experiment which it contains. The author must not be confounded, because of the similarity of initials, with the distinguished investigator, Dr. J. W. Draper.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

UNIVERSITY education for women may be taken as firmly established in England. The names of nine out of eleven female candidates have just been posted at Burlington Gardens as having passed the winter matriculation examination of the University of London, half-a-dozen in the honours division, besides three more in that next below, and are thus now on the high road to its B.A. degree, on the occasion of its first decorating their sex three academical years hence. From two interesting articles in the *Daily News* it is seen that Girton and Newnham Colleges have attained to unexpected success. The former is so much too small for the number of students that yearly flock to it that it is to be nearly doubled in size. At both institutions the students work much more earnestly than the average student of the hitherto privileged sex, and the examinations passed, at least at Girton, would

suffice to give the most successful student a high place in the Cambridge list of Wranglers. No doubt, in the near future, English girls all over the land will have as few obstacles in the way of their higher education as English boys.

MR. H. T. WOOD, of the Society of Arts, writing to Col. Donnelly, of the Science and Art Department, South Kensington, informs him that sufficient funds will be provided by the City Companies for the payment of teachers of classes for instruction in technology, on the same scale as that on which teachers of science classes are now paid by the Science and Art Department, and without any proportionate reduction, as stated in the Society's Programme of Examinations (p. 14), on the ground that all the amount at the disposal of the Society might be insufficient to allow of the full payment in all cases. Mr. Wood asks that this important fact be brought under the notice of the Secretaries of Science Classes and Schools. The arrangement will apply to the examinations held in May next.

DR. T. LAUDER BRUNTON, who was recently appointed to the Thomson Lectureship in the Aberdeen Free Church College, delivered the opening lecture on Monday, the 3rd inst. The course will consist of ten lectures.

THE University Library at Strassburg, founded after the recent Franco-German War, numbered no less than 470,000 volumes at the end of 1878.

WE are glad to see that in the second number of the *University College of Wales Magazine* science finds a corner in the shape of "Museum Notes." One of these, by Prof. Rudler, is a brief *résumé* of what is known of the Moa, *apropos* of a collection of moa-bones in the College Museum.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, January.—The error of some wild statements made about the size and cost of the cable that would be needed to convey the power of Niagara Falls several hundred miles by electricity is here shown by Professors Thomson and Huston, who calculate, *e.g.*, that 1,500 to 3,000 horse-power could be conveyed 1,000 miles (50 per cent. out of 3,000 horse-power) with a copper cable about $\frac{3}{4}$ inch in thickness. Less than $\frac{1}{2}$ inch would suffice for 500 m. For consumption of 1,000,000 horse-power a 3-inch cable would suffice. The same authors describe a curious thermo-magnetic motor, and an induction apparatus for reversed currents.—Dr. Henry Morton finds the coating of the "luminous clocks" sulphide of calcium attached with some resinous medium, and seems hopeful that advances in this direction may by and by give us houses lit at night by phosphorescent walls.—Mr. Huston gets some remarkable effects (needing confirmation from repeated tests) in subjecting iron to a continued and constantly increasing strain.—Dr. Dudley considers toughness, rather than hardness, the essential quality for durability of steel rails.—An automatic machine for playing the game of tit-tat-to is described by Mr. Freeland.

Reale Istituto Lombardo di Scienze e Lettere, vol. xi. fasc. xix., xx.—On some derivatives of ethyl-paraoxybenzoic aldehyde, by S. Corbetta.—The English sanitary laws, by Dr. Zucchi.—On the use of salts of berberine as febrifuge, and on tumours of the spleen from malarian infection, by Dr. Zucchi.—Studies on milk at the Pavian laboratory of cryptogamic botany, by Drs. Pirotta and Riboni.—Value of two meteorological instruments of Bellani, by Prof. Cantoni.—Observations on the seismic microphone of De Rossi, by Prof. Cantoni.

Vol. xii. fasc. i.—Studies on milk (continued), by Drs. Pirotta and Riboni.—New physio-pathological researches on pulmonary phthisis, by Prof. de Giovanni.—First lines of introduction to the study of the Italian bacteria, by S. Trevisan.—On modular equations, by S. Klein.—On pentahedral equations of surfaces of the third order, by Prof. Beltrami.

Bulletin de l'Académie Royale de Belgique, No. 11, 1878.—Besides M. Montigny's paper on variations in the scintillation of stars (referred to last week), this number contains a valuable memoir by M. Fredericq on the organisation and physiology of the poulpe (or octopus), which he studied at the laboratory of Roscoff. The arterial blood yields a blue colouring matter (called *hæmocyanine*), which corresponds somewhat to hæmoglobin, but contains copper, not iron. Both respiration and nutrition of tissues depend on it. The heart has both accelerating and moderating nerves. The blood pressure is considerably greater than in cold-blooded vertebrates; the venous system shows rhythmic contraction; exterior water does not mix with the blood; the urine contains neither urea nor uric acid, but a substance like guanine, &c. M. Fredericq localises the centres of the respiratory movements, and gives some interesting observations on the "chromatic function."—M. Van der Mensbrugghe finds confirmation of his theory of the variations of potential energy of liquid surfaces, in some peculiarities of liquid sheets, which he accounts for on thermodynamical principles.—M. Lienard communicates researches on the structure of the digestive apparatus of Mygales and Nephiles, and M. Longchamps concludes his synopsis of the Gomphines.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 30.—"On the Effects of Heat on the Di-iodide of Mercury, HgI_2," by G. F. Rodwell, Science Master, and H. M. Elder, a pupil, in Marlborough College.

The authors, in continuation of experiments of a similar character previously communicated by one of them to the Society, on the anomalous expansion of the iodide of silver, have examined the iodide of mercury with a view to the detection of similar anomalies. This body is dimorphous, existing in the form of yellow prismatic crystals, which change under external mechanical influence, or during the process of cooling, to brilliant scarlet octohedrons with a truncated summit. The mass melts at 200° C. to a red-brown liquid, which volatilizes at a slightly higher temperature. The following volumes were determined at the temperatures given:—

	Volume.		Specific Gravity
Liquid at 200° C.	1·1191147	...	5·286
Solid ,, ,, ...	1·0190453	...	6·179
Yellow prismatic condition at 126° C.	1·0115378	...	6·225
Red octohedral condition at 126° C.	1·0043337	...	6·276
,, ,, ,, at 0° C.	1·0000000	...	6·297

Thus the di-iodide of mercury possesses one coefficient of expansion = ·0000344706 for 1° C. between 0° and 126° C., it then undergoes a sudden expansion = ·00720407 in changing from the red octohedral to the yellow prismatic variety, while between 126° C. and the melting-point (200° C.), it expands under a higher coefficient than before = ·0001002953 for 1° C.

"A Comparison of the Variations of the Diurnal Range of Magnetic Declination as recorded at the Observatories of Kew and Trevandrum." By Balfour Stewart, F.R.S., Professor of Natural Philosophy in Owens College, Manchester, and Morisabro Hiraoka.

1. Generally speaking, maximum points or risings in the one curve must be associated with maximum points or risings in the other, rather than with minimum points or depressions. Indeed, the researches of Broun and others, from a different point of view, strengthen this conclusion, which is, however, abundantly supported by a glance at the curves themselves.

2. The oscillations of the Trevandrum curve are greater than than those of the Kew curve.

3. In many cases where there is a want of striking likeness between the oscillations of the two curves, there are yet noticeable traces in the one curve corresponding to the oscillations of the other. There are, however, a few cases where there is a want of apparent likeness.

4. In general, though not invariably, the oscillations of the Trevandrum curve follow rather than precede the corresponding oscillati ons of the Kew curve.

February 6.—"Absorption of Gases by Charcoal. Part II. On a new Series of Equivalents or Molecules." By R. Angus Smith, Ph. D., F.R.S.

In the *Transactions* of the British Association, 1868, Norwich, on p. 64 of the "Abstracts," there is a preliminary notice of an investigation into the amount of certain gases absorbed by charcoal. I made the inquiry from a belief previously expressed in a paper of which an abstract is in the *Proceedings* of the Royal Society, p. 425, for 1863. I said in that paper that the action of the gas and charcoal was on the border line between physics and chemistry, and that chemical phenomena were an extension of the physical; also that the gases were absorbed by charcoal in whole volumes, the exceptions in the numbers being supposed to be mistakes. The results given were:—

Hydrogen	1
Oxygen	7·99
Carbonic oxide	6·03
Carbonic acid	22·05
Marsh-gas	10·01
Nitrous oxide	12·90
Sulphurous acid	36·95
Nitrogen	4·27

It was remarked that the number for nitrogen was probably too low; I had some belief that the charcoal retained a certain amount which I had not been able to estimate.

For common air, the number 40·065 crept into the paper or abstract instead of the quotient 7·06.

I considered the numbers very remarkable, but was afraid that they would be of little interest unless they could be brought more easily under the eyes of others; my experiments were somewhat laborious; the exact numbers were seldom approached by the single analysis, but were wholly the result of a series of irregular averages and apparently irregular experiments. The cause of this was clear, as I believed, namely, the irregular character of the charcoal with which I had to deal. The experiments were forgotten, I suppose, by most men, but the late Prof. Graham told me that he had repeated them with the same results that I had published. I might have considered this sufficient, but waited for time to make a still more elaborate investigation of the subject, and to take special care with oxygen, in the belief that, the rule being found, the rest of the inquiry would be easy; this was extended to nitrogen, but not by so many experiments as with oxygen. I am now assured of a sound foundation for inquiries, which must take their beginning from the results here given.

It is found that charcoal absorbs gases in definite volumes, the physical action resembling the chemical.

Calling the volume of hydrogen absorbed 1, the volume of oxygen absorbed is 8. That is, whilst hydrogen unites with eight times its weight of oxygen to constitute water, charcoal absorbs eight times more oxygen by volume than it absorbs hydrogen. No relation by volume has been hitherto found the same as the relation by weight.

The specific gravity of oxygen being 16 times greater than hydrogen, charcoal absorbs 8 times 16, or 128 times more oxygen by weight than it does hydrogen. This is equal to the specific gravity of oxygen squared and divided by two $\frac{16^2}{2}$, or it is the atomic weight and specific gravity multiplied into each other, 16×16, and divided by two $\frac{256}{2} = 128$.

Nitrogen was expected to act in a similar way, but it refused. The average number of the latest inquiry is 4·52, but the difficulty of removing all the nitrogen from charcoal is great, and I suppose the correct number to be 4·66. Taking this one as the weight absorbed, $14 \times 4{\cdot}66 = 65{\cdot}3$, or it is $\frac{14^2}{3}$. Oxygen is a dyad; nitrogen a triad.

We have then carbonic acid not divided, but simply 22 squared = 484.

Time is required for full speculation, but the chemist must be surprised at the following:—

Carbonic oxide	6	volumes.	
Carbonic acid, CO_2	6 + 16	,,	= 22.
Marsh-gas, CH_4	6 + 4	,,	= 10.
Protoxide of nitrogen, NO	8 + 4·66 (N) (4·9).		= 12·66.

These four results belong to the early group not corroborated lately, but so remarkably carrying out the principle of volume in this union giving numbers the same as those of weight in chemical union, that they scarcely require to be delayed.

I am not willing to theorise much on the results; it is here sufficient to make a good beginning. We appear to have the formation of a new series of molecules made by squaring our present chemical atoms, and by certain other divisions peculiar to the gases themselves. Or it may be that the larger molecule exists in the free gas, and chemical combination breaks it up. These new and larger molecules may lead us to the understanding of chemical combinations in organic chemistry and whenever there is union not very firm, and may also modify some of our opinions on atomic weights and the motion of gases.

Of course, I cannot pretend to give the result of these results; but as we have here the building up of a molecule by volumes, so as to form an equivalent of physical combination analogous to the chemical equivalent, it is impossible to avoid seeing that it indicates the possibility of our present equivalents being made up in a similar manner.

I did not expect these numbers; but I certainly, as my previous paper showed, had in full view a necessity for some connection between physical and chemical phenomena more decided than we possessed.

Chemical Society, February 6.—Dr. Gladstone, president, in the chair.—This meeting was occupied by the discussion on the processes for determining the organic purity of potable waters, a paper read by Prof. Tidy some time since. Dr. Frankland opened the discussion and criticised at some length the objections urged by Prof. Tidy against his method of estimating the carbon and nitrogen in a water residne by combustion. The discussion was continued by Mr. Wanklyn, Mr. Kingzett, Prof. Bischof, Dr. Voelcker, Mr. Grosjean, Dr. Dupré, Mr. W. Thorp, and Dr. Hake. Prof. Tidy then briefly replied, and the proceedings terminated with a unanimous vote of thanks from a crowded meeting to Prof. Tidy for his paper.

Zoological Society, February 4.—Dr. Günther, F.R.S., vice-president, in the chair.—Mr. Sclater exhibited and made remarks on a specimen of a Curassow, belonging to the Royal Museum of Copenhagen, which he had received from Prof. J. Reinhardt, F.M.Z.S., for examination, and which Prof. Reinhardt had proposed to refer to a new species (*Mitua salvini*).—Mr. R. Bowdler Sharpe exhibited a series of Bulwer's Pheasant (*Lobiophasis bulweri*), from the Lawas River, N.W. Borneo, collected by Mr. W. H. Treacher, Acting-Governor of Labuan. The series represented every stage of plumage of this pheasant, and conclusively proved that *L. castaneicaudatus*, Sharpe, was the immature male of *L. bulweri*.—A communication was read from Prof. A. H. Garrod, F.R.S., containing some notes on certain points in the anatomy of Hoatzin (*Opisthocomus cristatus*).—Mr. Sclater read some notes on the breeding of the Argus Pheasant and other Phasianidæ in the Society's Gardens.—A communication was read from the Rev. O. P. Cambridge, C.M.Z.S., containing the description of a new genus and species of spiders, proposed to be called *Fritzia muelleri*.—Mr. W. Ottley read the firs part of a series of observations on the structure of the eye-muscles in the mammalia.—A communication was read from Mr. Osbert Salvin, F.R.S., on some birds transmitted by the Rev. Thomas Powell from the Samoan Islands, amongst which were two new species proposed to be called *Pinarolestes powelli* and *Fregetta mœstissama*.—A communication was read from Mr. W. H. Dale containing remarks on the use of the generic name *Gouldia* in zoology.—Mr. George A. Shaw read notes upon the habits of four species of Lemurs, specimens of which had been brought alive to England in 1878, from the province of Betsileo in Central Madagascar.—A communication was read from Mr. F. Moore, F.Z.S., containing descriptions of some new Asiatic diurnal lepidoptera.—Dr. A. Günther, F.R.S., pointed out the characters of a new rodent from Medellin, U.S. of Columbia, for which the name *Thrinacodus albicauda* was proposed.

Linnean Society, January 16.—Mr. W. Carruthers, F.R.S., vice-president, in the chair.—Prof. Allen Thomson exhibited and made some remarks on a block of wood, during the growth of which a portion of the shank-bone of an ox had become centrally inclosed; he also called attention to an imperfect frond of a palm (Chæmærops?) asserted to have been discovered within a plank of rosewood.—Mr. Christy in some observations referred to the Chalmugra tree (*Gynocardia odorata*), its therapeutical properties being highly extolled, especially in rheumatism.—Mr. J. G. Baker read a paper on the Colchicaceæ and aberrant tribes of Liliaceæ. Colchicaceæ is the smallest ot the three sub-orders of Liliaceæ, it includes 39 genera and 153 species. Its geographical dispersion agrees completely with true Liliaceæ. In its typical form it is marked by extrorse anthers, a septicidal capsule, and three distinct styles; but as 24 out of 39 genera do not possess all these three characters in combination, but recede more or less decidedly from the type in the direction of true Liliaceæ, it seems injudicious to follow those who have proposed to keep up Colchicaceæ or Melanthaceæ as a distinct natural order. Mr. Baker defines seven tribes, Colchiceæ, Merenderæ, Veratreæ, Anguillareæ, Helonieæ, Uvularieæ, and Tofieldieæ. There are several anomalous genera of the Colchicaceæ, for instance Hewardia, which connects the Liliaceæ with the Iridaceæ. Again, there are three aberrant tribes of Liliaceæ, viz., (1) Conantheræ a connecting link between Liliaceæ and Amaryllidaceæ, (2) Liriopeæ

(formerly Ophiogon) and (3) Gilliesieæ; genera among the two latter receding widely from the liliaceous type and others bridging over the interval between the extreme form and ordinary lilies. The author then enters into lengthened descriptions with ample diagnosis, &c., forming in fact a valuable continuation of his former series of monographs of the natural order of Liliaceæ. —Messrs. G. Brook, A. P. Luff, J. E. Griffiths, C. Sharp, and J. Woodland, were balloted for and elected Fellows of the Society.

Anthropological Institute, January 28.—Anniversary Meeting.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—The election of Mr. A. H. Keane, B.A., as a Member was announced.—The following gentlemen were elected to serve as officers and council for the year 1879. President—E. B. Tylor, F.R.S. Vice-Presidents—Hyde Clarke, J. Evans, F.R.S., Prof. Flower, F.R.S., Maj-Gen. A. Lane Fox, F.R.S., Francis Galton, F.R.S., Prof. Rolleston, F.R.S. Directors and Hon. Secretaries.—E. W. Brabrook, F.S.A., W. L. Distant, J. E. Price, F.S.A. Treasurer—F. G. H. Price, Esq., F.R.G.S. Council—Lt.-Col. Goodwin Austen, J. Beddoe, F.R.S., Prof. George Busk, F.R.S., C. H. E. Carmichael, M.A., J. Barnard Davis, Esq., F.R.S., W. Boyd Dawkins, F.R.S., Capt. Harold Dillon, F.S.A., A. W. Franks, Esq., F.R.S., J. Park Harrison, M.A., Prof. Huxley, F.R.S., A. L. Lewis, Sir J. Lubbock, Bart., M.P., R. Biddulph Martin, F. W. Rudler, F.G.S., C. R. Des Ruffières, F.R.S.L., Lord Arthur Russell, M.P., Rev. Prof. Sayce, M.R.A.S., Dr. Allen Thomson, F.R.S., C. Staniland Wake, M. J. Walhouse, F.R.A.S. The retiring president delivered his annual address, in the course of which he alluded to the researches now being carried on in the caves of Borneo by Mr. Everett (see p. 352).

Geological Society, January 22.—Henry Clifton Sorby, F.R.S., president, in the chair.—John Edward Marr and Lieut. Henry Tryon Wing were elected Fellows of the Society.—The following communications were read:—On community of structure in rocks of dissimilar origin, by Frank Rutley.—Distribution of the serpentine and associated rocks, with their metallic ores, in Newfoundland, by Alexander Murray.

Institution of Civil Engineers, February 11.—Mr. W. H. Barlow, F.R.S., vice-president, in the chair.—The following papers were read:—On the Geelong water supply, Victoria, Australia, by Mr. Edward Dobson, Assoc. Inst., C.E.—On the Sandhurst Water Supply, Victoria, Australia, by Mr. Joseph Brady, M. Inst. C.E.

Victoria (Philosophical) Institute, February 3.—A paper on the Torquay caves was read by Mr. J. E. Howard, F.R.S., in which he reviewed the reports given by geologists who had excavated and examined the various deposits in these caves. Mr. Howard examined into the nature of these deposits and the conditions under which they must have taken place, and pointed out the peculiar nature of the evidence by which it was possible to arrive at some conclusion as to the age of those deposits. Prof. Challis, F.R.S., and others took part in the discussion, either by sending communications to be read or by attending to do so.

PARIS

Academy of Sciences, February 3.—M. Daubrée in the chair.—The following papers were read:—Remarks on the third reply of M. Pasteur, by M. Berthelot.—On the development of the perturbative function, &c. (continued), by M. Tisserand.—On the fermentation of cellulose, by M. van Tieghem. The author gives observations on amylobacter, the figured ferment of cellulose. It affects different plant tissues differently; only in the young state have all the cells of all plants their membranes equally dissolved by it. The results have a physiological bearing (digestibility of cellulose from different plants), and a palæontological (unequal chances of fossilisation of different plants). The amylobacter first transforms soluble starch into dextrine, then into glucose, and it is really the glucose that ferments. It seems to be by direct contact of amylobacter with cellulose that the latter is dissolved, not through a diastase of cellulose acting without at a distance.—On the construction of the international geodetic scale, by MM. Sainte-Claire Deville and Mascart.—An account of the physical and chemical properties of the scale (of iridised platinum) prepared by Johnston and Matthey, and of experiments to determine the coefficient of dilatation of a thermometric tube made of the material.—On the invention of several arrangements of the heliometer, by M. De la Gourmerie. He attributes the half objectives not to Dollond but to Bouguer, considered the original inventor of the heliometer.—M. Cosson called attention to a case of fire in the laboratory of his herbarium, arising from carbonisation of boards of flooring exposed to hot air from an air-hole fed from a stove 4 m. off on the floor below.—M. de Lesseps presented a fourth volume of his "Letters, Journal, and Documents to serve for the History of the Suez Canal." He quoted a long letter he had addressed to Mr. Layard, vindicating the enterprise against opposition then offered.—M. Lalanne was elected Free Member in room of the late M. Bienaymé.—On the conditions of existence of a determinate number of roots common to two given equations, by M. Simonnet.—On some invariants of linear differential equations, by M. Laguerre.—On the motion of a body which is displaced and deformed while remaining homothetic with itself, by M. Fouret.—Integration, in finite form, of three species of linear differential equations with any coefficients, by M. André.—Extension of the metric system of weights and measures; development of concordant monetary systems in the various states of the civilised world, by M. de Malarce. The metric system of weights and measures is now established obligatorily in eighteen states, with 236·6 millions of inhabitants; it is legally optional in three states with 75·5 millions; and it is admitted in principle, or partially for customs, in five states, with 343·6 millions.—Liquefaction of siliciated hydrogen, by M. Ogier. It is liquid at $-11°$ under 50 atm.; at $-1°$ under 100 atm. At zero it remains gaseous up to 150 and 200 atm.—Memoir on the determination of methylic alcohol in commercial methylenes, by MM. Bardy and Bordet.—Influence of duration and intensity on luminous perception, by MM. Richet and Breguet. A weak light, perceived distinctly when it impresses the retina some time, becomes invisible when its duration diminishes. It may be rendered visible anew by making it more intense, or increasing its duration, or repeating it rapidly. Coloured lights are subject to the same laws, and are always seen with their proper coloration, whether strong or weak, long or short.—On the minute structure of the central nervous system of decapod crustaceans, by M. Yung.—On the Wagnerite of Bamle in Norway, and on a retinite of Russia, by M. Pisani.—The glazed frost of January, 1879, by M. Godefroy.—On the effects of the same at Fontainebleau, by M. Piebourg. This (somewhat rare) phenomenon did great mischief to trees, the greatly increased weight breaking down their branches, &c.

GÖTTINGEN

Royal Academy of Sciences, November 2, 1878.—The following, among other papers, were read:—On a propagation of the growth-stimulus produced by fertilisation on vegetative organs, by Herr Reinke.—Observations on the value of a ligature of the great brain-arteries, for experimental pharmacological researches, by Herr Marmé.

CONTENTS

THURSDAY, FEBRUARY 20, 1872

THE COAL MINE COMMISSION

MUCH misapprehension of facts, and much ignorance of scientific principles are, it is to be feared, the most prominent characteristics of the knowledge possessed by most people regarding the nature and mode of occurrence of colliery explosions. Pompous platitudes about the carelessness of the men, and windy panegyrics on the virtues of the safety-lamp have been freely pronounced times without number during the last two generations of mankind. Committees on accidents in mines have met, discussed the question, and separated; inspectors of mines have been appointed, stringent regulations have been framed and enforced which bind both employer and employed, the public have listened to the tale of woe, have wept over the fate of the hapless miner, and have subscribed hundreds of thousands of pounds in aid of his widows and orphans, and yet with all this, and more untold, the history of the past eighteen months would seem almost to indicate that we are as far as ever from a true solution of this much-agitated question.

Can science, then, do nothing? is her hand unable to save even a moiety of the lives that are being constantly cast away before her very eyes? To this question, which is very often asked, the reply is simple. Science can undoubtedly solve the question, but she must do it in her own way; she must approach it with the sap and mine of investigation and research; she must have her own time to do it, and above all, she requires to be encouraged. Scientific men have, no doubt, sometimes turned their attention to the subject without much apparent result, but their efforts have, as a rule, been of short duration, and they have too often met with discouragement, active opposition, and incredulity. We have only to point to Dr. Birbeck's letter to the South Shields Committee (1843) at p. 48 of their report, and to the report addressed to the committee of the coal trade by the special committee appointed to take into consideration the report of Lyell and Faraday on the Haswell Colliery explosion to exemplify what we mean. We wish we could say that there are no examples of the same kind at the present day, but unfortunately we are unable to do so.

Putting aside these gloomy thoughts, however, we are rejoiced to see science so well represented on the Commission just appointed to inquire into and report upon this matter. We had always imagined it to be a purely scientific question, having almost no relation to the art of mining in its stricter sense. We confess that ours is not the common view, but that, on the contrary, it is usually supposed that if practical men, including inspectors of mines, are unable to prevent explosions, nobody else need try to do so. We cannot accede to such a proposition; nay, more, we protest against it. If disasters of this kind could be stopped by hedging round the collier and his employer, with an impenetrable palisade of instructions and restrictions, then we are bound to acknowledge that the practical man with a legal turn of mind could do it effectually. This method has been tried, however, or rather, it is now in force, and we see its results.

The new Commissioners will, no doubt, seek to acquaint themselves to some extent with the work of their predecessors and contemporaries; they will also weigh carefully the opinions that will be expressed before them, and take account more of the facts and reasoning that can be adduced in support of such opinions than of the age and position of those who express them. For example, they will not come to any conclusion regarding the influence of fluctuations of atmospheric pressure and temperature upon the issue of fire-damp from coal until they have obtained a sufficiently large and well-authenticated mass of information to enable them to arrive at sound conclusions. The data which have been collected and tabulated by Messrs. Scott and Galloway and others are amply sufficient to show that the matter is worthy of further careful consideration, but they do not extend over a lengthened period of time, and we venture to say that their weight would be greatly augmented by a further addition of similar facts. As to the use of fire-damp indicators (and by this we suppose are meant instruments for giving notice of the occurrence of outbursts of gas), we fear that their importance has been absurdly exaggerated and that they are brought forward by those who know nothing about mining. It will be for the Commissioners to inquire, however, whether there are any mines in existence in which they could really be of any service. The systematic observation of the air in mines may be taken, we suppose, to apply both as to its quality and quantity. As regards its quality, it will have to be determined whether observations of the height of the cap on the lamp-flame reduced to small dimensions, such as those described by Mr. Galloway in the *Proceedings* of the Royal Society, 1876, are sufficiently accurate and reliable, or whether it will be necessary or expedient to bring into use some kind of instrument like the *grisoumètre*, by means of which a rapid analysis of the air can be made. As regards its quantity, on the other hand, it may be suggested that if the velocity and drag of the currents could be continuously recorded in a manner similar to the pressure and temperature of the air at the meteorological stations, a check might be kept upon the ventilation, and changes in its efficiency could be ascertained by a mere inspection of the curves. Improved methods of ventilation and illumination are important objects. Powerful machines are now being erected at most new collieries, but these alone cannot prevent explosions, if we may judge by the case of Abercarne, which was thoroughly well equipped in that respect. The Commissioners must beware of being drawn into the belief that some advantage can be gained by forcing the air into the mines instead of driving it out by way of keeping the gas in the coal, as it were, and thereby escaping the effect of atmospheric influences. They themselves will be able to balance the slight gain of pressure that could be obtained in this way with the normal pressure of gas in the coal. It will be well worth their while, however, to consider the means that have been suggested of late years for producing an artificial barometric depression in the workings with the view of extracting the gas from the open fissures, old works, and even to some extent from the face of the coal, when the men are out of the mine, and then sweeping it out by admitting air freely through the downcast shaft, and at the same time continuing to work the

exhausting machine. As to ourselves, we thought the idea chimerical when it was first brought under our notice; but after reading a pamphlet on the subject by M. Francis Laur, of Saint-Etienne, we were forced to change our mind. The kind of safety-lamps that ought to be employed will form a subject of warm debate, but we can confidently assure the Commission that if they separate without recommending the universal adoption of lamps that will not continue to burn in an explosive atmosphere, and of more perfect locks than the present ones, they will have failed to satisfy one of the most important requirements of the time. We say this without prejudice to the means that may be thought best for examining the workings for accumulations of gas: we refer to the lamps supplied to the common workmen. The employment of explosive agents in getting the mineral, and other particulars relating to mines and mining operations include, we think, the most important of all the questions that will come before the Commissioners. What is coal-dust? and what bearing has it on the subject? If the presence of coal-dust is, after all, the cause of all these great explosions, and if, as has been once before asserted in these pages, great explosions never happen in damp or wet mines, but always in dry ones containing coal-dust, then surely the Commissioners would be but dallying with the subject if they omitted to carefully weigh all the facts that have been adduced in favour of this hypothesis. They will have to turn to other sources, however, than the reports of the inspectors of mines for information; and they might do worse than consult the report drawn out by M. Haton de la Goupillière of the doings of a similar commission appointed by the French Government in 1878, under the title of *Rôle des Poussières de Charbon*, in which they will find it alluded to, and an historical account given of the steps that have led to our present knowledge regarding it. Otherwise the literature of our own country is not quite so sterile in this respect as it was three years ago; but doubtless the Commissioners will provide themselves with all the most recent information.

The dangers due to the use of explosives in mines are of three kinds: firstly, the shot may ignite an accumulation of gas directly; secondly, it may effect the same thing indirectly by driving the flame of a safety-lamp burning in the accumulation through the meshes of its wire gauze cylinder; thirdly, the sudden rush of flame and the violent disturbance of the air caused by a blown-out shot (that is to say, one which expels its tamping without bringing down or even breaking the rock) may raise and ignite the coal-dust in front of it, and more especially if it is directed towards or parallel with and near to the floor. In the last case the flame has been often known to extend to a distance of thirty, forty, and even eighty yards from its origin, and such a disturbance taking place under favourable conditions is quite sufficient to initiate explosions such as those of Blantyre, Haydock, Abercarne, and Dinas, in which close upon a thousand lives have been lost within the last eighteen months. When we add that each of these mines was very dry and contained plenty of coal-dust, and was not known to contain more than insignificant accumulations of fire-damp, quite insufficient to account for a tithe of the extent and violence of the explosions, we have said enough, we think, to make it apparent how pressing is this matter.

Blasting operations can be safely carried out in any mine if the shots are not fired near explosive accumulations, if the lamps are of such a construction that they cannot burn in an explosive atmosphere, and if the mine is a damp one. It is easy enough to provide the two first requirements, and it is obvious that by plentifully watering the roadways we can turn a naturally dry mine into a damp one and avoid danger such as we have described from blasting, as well as localise explosions of fire-damp. We have seen it stated in a footnote at p. 661 of the last number of the *Bulletin de la Société de l'Industrie Minérale* that dusty mines, which are at the same time well ventilated, may be watered frequently without obtaining permanent humidity. Fortunately, however, we are in a position to point to an example which has come under our own immediate notice, of an extensive mine in which the workings are kept damp throughout their whole extent by a constant application of water. In the case to which we refer the daily output of coal is 800 tons, the temperature of the workings is between 70° and 80° Fahr., and the amount of air passing into and out of the mine is between 80,000 and 90,000 cubic feet per minute.

In conclusion we are eminently satisfied that science is so well represented on the Commission; and if its various members will pull together like a well-balanced team, we anticipate the happiest results from its researches and labours.

KINGZETT'S ANIMAL CHEMISTRY

Animal Chemistry; or, The Relations of Chemistry to Physiology and Pathology. A Manual for Medical Men and Scientific Chemists. By Charles Thomas Kingzett, F.C.S. (London: Longmans, Green, and Co.)

FOR many years the want of a good manual of physiological chemistry or animal chemistry in the English language has been a standing reproach to English science. The causes of this want are not far to seek. Physiology has not many votaries in England, and physiological chemistry, being in interest one step farther than physiology from the verge of medical practice, has still fewer followers. The number of possible writers of a text-book of animal chemistry has, therefore, been small; and, among them, the number of men whose capability and opportunities for such an undertaking might justly have led them to hope for a successful issue to their labours has, it is needless to say, been smaller still. The qualifications to be looked for in one who attempts the task of writing such a manual are indeed not slight. He must be a thoroughly trained chemist whose judgment has been much exercised in the appreciation of chemical questions; he must be a physiologist with a sound and direct knowledge of most of the practical methods of physiology; he must be an anatomist who is fairly well acquainted with the microscopic structure of animal tissues; and he should have some insight, exact if not special, into morbid processes and pathological states. We need not wonder, then, that the labourers have been few.

The absence of any very recent English work of the kind does not lighten the difficulty of writing one; but the disadvantage of this absence is not so great as it might at first sight seem; for on the Continent there are text-books which in case of need might have served as a standard or lay-figure.

The difficulties of the task have, therefore, without a doubt, been many; but the task itself should not have been hopeless or ungrateful. English workers in physiological chemistry have at present to betake themselves to Hoppe-Seyler, Kühne, Gautier, Gorup-Besanez, for the chemico-physiological facts they stand in need of. English students have to make do the brief sections on Animal Chemistry—admirable, but of necessity imperfect and categorical—contained in the English text-books of physiology. Hence an English book specially devoted to animal chemistry, if at all exhaustive, accurate, and modern, would be likely to bespeak for itself a hearty welcome, and a disposition to extenuate its shortcomings.

Such were the prospects, unfavourable and favourable, of an attempt to fill up the book-shelf of English workers in natural science by a manual of animal chemistry. We turn to Mr. Kingzett's book, and, after a careful and reiterated perusal of it, we can say that never was attempt so rashly undertaken. We had expected a sound, if modest, substratum of physiological knowledge, and we find slipshod notions and the speculations of the amateur. We had expected apposite illustrations from pathology, and we find, in most cases, trivial and meaningless references to disease. We had expected a complete and careful account of the more purely chemical portions, and we find a degree of imperfection which sends us back with thankfulness to the chemical sections of Foster and Hermann.

Nor is this all. The book is styled a book of animal chemistry, and we therefore expected animal chemistry; but in addition to the sections so called we find scattered here and there reflections on life, character, and the morals of scientific work which, even were they not mere platitudes, would be utterly out of place in a work like this.

These strictures may seem severe, but they would not be wholly unexpected by any one who had read Mr. Kingzett's preface. He says:—"For four years I was occupied with the practical study of subjects comprehended in the following chapters, and during the whole of that time there were no fluctuations in the success attending the labours in which my services were involved. . . . It was therefore a matter of sincere regret with me that circumstances (which are said to be stronger then men) ultimately necessitated the discontinuance of my connection with work which had given me so much real pleasure." Then follows a page or so of reflections on the pleasure to be derived from original investigation; comparisons of the "scientist" and the "sentimentalist;" and so forth. "It was natural, then," he continues, "that, having experienced so much pleasure, I should be moved with equal regret in resigning the practical study of physiological chemistry; and in order to complete a well-remembered but brief connection with this subject, I determined to attempt a task which should prove of service to scientific men, namely, to collect and systematise, as far as could be, all the trustworthy work on record in relation to animal chemistry, so far as it concerns the human body." We submit that four years' practical work at a subject like animal chemistry, be the success of the worker never so unfluctuating, is hardly warrant enough to undertake that which taxes the matured judgment even of a master, viz., the making of a useful and comprehensive text-book. Nor do we perceive that Mr. Kingzett's desire to signalise his departure from the field of chemico-physiological research adds any urgency to the warrant. We hope to show in the sequel that this prefatory confession on the part of the author, of practical unpreparedness for the task he had set himself to do, is fully borne out by the internal evidence of the book.

Mr. Kingzett does not profess to have included any account of the practical methods of the science. This, while it much lightens the labour of writing the book, is, we think, doubly to be regretted; because the book is to be read by medical men, who are not supposed in every case to remember chemical methods, and by scientific chemists who cannot be expected to know, for example, the modes of practising fistulæ, or rapidly removing blood from brains intended for analysis.

Mr. Kingzett's haphazard preparation for his task is well displayed in his seeming ignorance—not of the latest, but even of the penultimate—advances of the chemistry of physiological processes. Thus, while treating of peptones, whether in this part (p. 63) or on p. 386, Kühne's well-known and suggestive researches on the action of the digestive juices on proteids are inexplicably omitted; and nowhere, indeed, is the formation of peptones and their relationship to the albuminous bodies attempted systematically or adequately to be discussed. In treating of pancreatic juice the beautiful and conclusive work of Heidenhain on the production of the active ferment—a subject surely of the first chemico-physiological interest—is not once referred to, and indeed, by implication, ignored. (Cf. p. 70, where the preparation of extracts of pancreas is briefly described without any caution being given as to the time the pancreas should be let stand before being used.)

The inaccuracy of the author may be illustrated by a reference to pp. 49 and 60. There the effects of alkalis and acids of various strengths in amylolysis and proteolysis are mis-stated, and the important inferences from the facts altogether ignored.

Let us now turn to the chapter on the blood in Part III. Here, if anywhere in the book, we should expect to find completeness and the traces of careful work. On the contrary, on turning up Coagulation (p. 144)—the much-investigated, if not best-understood, process of the blood—we find the phenomenon itself imperfectly stated, the retarding influence of alkaline salts described without any quantitative conditions being given, and the theory of A. Schmidt discussed so slovenly and unintelligently that the important *fibrin-ferment* is nowhere directly treated of, but only implied (p. 146), if, indeed, it is not confused with paraglobulin. (See p. 146, the first paragraph, and p. 144, the second paragraph: the various statements taken together will, undoubtedly, bear such an interpretation.)

The blood as a respiratory tissue fares no better (p.

165). What are we to think of a manual of animal chemistry in which the author treats of the gases of the blood in mere general terms without reference to quantities? or what of one in which the author, among the spectroscopical properties of hæmoglobin, forgets to mention the shortening of the spectrum and the situation of the absorption-bands, or even the chemical method of deoxidation? What are we to think of the judgment of an author of such a manual who refers only to Bert and Fernet among those who have studied the affinity of hæmoglobin for oxygen (p. 166)? And what of the erudition of one who considers the following statement (p. 167) a sufficient discussion of the liberation of CO_2 in the lungs?—"It is easy to understand how the free carbonic acid is liberated, but not so simple to explain the liberation of that part previously in combination with alkaline bases. Thudichum supposes that when the venous blood reaches the small breathing cells the hæmato-crystalline is partly oxidised into what he calls hematic acid, and this, passing into the serum at the same moment, decomposes the carbonates in the blood, setting free carbonic acid, which, with the watery vapour, escapes through the lung tissue into the respiratory passages."

The salts of the blood are dismissed with bare enumeration.

But it is in the section on Food and the oxidations of the body (in Part II.), that the author discovers the appalling inaccuracy of his physiology. In many statements he seems to assign great importance to oxidations occurring in the blood and even in the lungs. Thus on p. 152 he speaks of "alcohol which must be placed side by side with fat as a respiratory food or substance which admits of oxidation in the lungs." Again (p. 154), "From the fact that oxidation in the lungs is a process of combustion and the source of muscular power, the foods which undergo this process are termed heat-producers; but we shall see presently that it is by no means clear that blood oxidation is attended directly with the evolution of animal heat": (he is here alluding to certain views of a Dr. Hake to be immediately referred to). In other parts of the book, also, the same exaggerated importance seems to be attached to oxidations in the lungs and blood. Thus, at p. 321, he inclines to the view that "the cerebro-spinal system does not generate its own force, but derives it through the chemical changes of the lungs." See also at p. 463, where a similar statement is included in the chapter on "Character." And finally, on p. 198, speaking of the seat of production of urea, the author says that "more modern researches tend to show that vitality consists more in the changes occurring in the blood, and that these changes may result in the direct production of urea."

These statements are clearly made in the spirit of those who hold that the interior of the blood-capillaries is the arena of oxidations, if they do not indeed take us back to the view of Lavoisier, who considered the lungs to be the heating-furnaces of the body. The latter view, we need hardly say, was long ago given up: the former is not seriously advocated by any recent physiologist. The lungs and the blood, like most of the organised tissues, doubtless suffer oxidation in the performance of their functions; but the degree of it is unimportant as a source of heat compared with the universal oxidation of other active organs. Had the author adhered throughout to the view under the influence of which the above statements were set down, he would at least have escaped the charge of inconsistency. But this was hardly possible. In the course of his reading and extracting among modern papers for the purposes of this book, he could not but meet references, direct or implied, to the generally received doctrine of the origin of animal heat in the functional oxidations of tissue-cells; and we therefore find that, side by side with the false, the true doctrine is taught.

But, notwithstanding that Mr. Kingzett's physiology, even at a point which peculiarly affects the chemist, is unsound and wavering, he yet ventures to enlarge upon matters of mere speculative interest which have but a superficial connection with his subject. We shall quote the author's own words at the page where the subject is most fully dealt with, though by no means the only page where it is to be found. We are very sorry we cannot give the whole of it. The author is citing in his own words—and citing with approval—the views of Dr. Thos. G. Hake, M.D., F.C.S., which are contained in a paper entitled "On Vital Force: its Pulmonic Origin and the General Laws of its Metamorphoses," 1854 and 1867. "He (Dr. Hake) believes that the chemical changes as they occur in the blood system, and comprised in the act of oxidation, do not result in the evolution of heat, but force, which becomes electric by the agency of the blood corpuscles; and it is certain that this is perfectly consistent with what we know of cell-life. On this hypothesis, the blood-cells form chains and conductors for the electric current thus generated, and this is subsequently metamorphosed into heat at every point of the system. On reaching the cerebro-spinal centres it becomes vital force—another name for electric force—and this becomes eventually heat, namely, when it is transmitted to enable the consummation of vital acts, such as sensation, muscular motion, or secretion. Faraday and Du Bois Reymond, and hosts of other experimental inquirers, have insisted on the identity of electrical and vital force, and the experiments of Du Bois Reymond in particular, go to prove that nerve force is only electric force manifested through media not met with out of the living bodies." . . . "Our author even goes further, and with consummate skill, reasons that when this cerebro-spinal force is united in action within the same organic medium with other forces influencing us from without, viz., light, sound, heat, &c., new results are attained, and phenomena of sense and intelligence are observed." Why, he goes on to ask (whether the question is Mr. Kingzett's own, or only Dr. Hake's, endorsed by Mr. Kingzett, is not clear), why, in anæmia, does the brain lose somewhat of its intense vital force? "Because," he answers, "the source of vital force, viz., blood oxidation, is interfered with."

We have merely to add, before leaving this section, that, as a matter of course, the fine investigations of Prof. Hermann into the chemical changes in contracting muscle, upon which so much of our knowledge of cell-function in its chemical aspect is based, seems to have eluded Mr. Kingzett's eye altogether.

Part V. Mr. Kingzett heads "Chemical and Philosophical Subjects." We shall say nothing further respecting

the chemical part; for a treatise having the pretensions of the book we are reviewing, it is notably incomplete. English students and medical men who wish to inform themselves in the chemistry of the animal tissues will find far more to their purpose in Prof. Gamgee's annotated edition of Hermann, or Mr. Lea's Appendix to Foster, than they will find in Mr. Kingzett's Manual.

The last chapter but one of the book is devoted to a discussion of "Character." This, not being a chemical subject and not a physiological one, we presume Mr. Kingzett includes under the title "philosophical." Why Mr. Kingzett should select "character" out of the multitude of extraneous subjects; why "character" should be called specially a "philosophical" subject; and why it was deemed advisable to serve up scraps of philosophy at all in a "Manual of Animal Chemistry;" are difficulties which at once arise in our mind as we peruse the list of contents, and which are nowhere fully explained on closer inspection of the book. It is true that when we come to find that by "Character" Mr. Kingzett does not exactly mean character, but the whole mental and moral nature of man and its, at present, inexplicable connection with his physical nature, the difficulties recede if they do not diminish. They are certainly not entirely effaced. If the pure physiologist is content for the present to leave such subjects to the psychologist, the chemist must recognise, when he takes them up, that he does so quite gratuitously. But whether or not it is expedient to undertake discussions on psychology in chemical books, it is at least expedient, if they are undertaken, that they should be sensible and to the purpose; that they should not be encumbered by commonplaces or crude analogies; and that they should be got over as quickly as possible. Although it is a pity to disturb the order of Mr. Kingzett's reflections with the scissors, only space is granted us for a paragraph or so. They shall be neither worse nor better than the rest; and we strongly advise those readers who are in search of amusement to borrow the book and read the whole chapter.

He begins: "Character is almost universally regarded as something apart from the body of man himself; something for which man is individually responsible, something which, born with man, is developed and cultured into maturity by education and training, be that mature state one for evil or one for good." Mr. Kingzett does not appear to believe this, whatever it may mean; for he continues by way of antithesis, "Let first causes be what they may, and so also let us hide our face from the infinite future and regard man as an intelligent machine, complete, so far, in himself." This resolution having been taken, the difficulty of justly judging men's thoughts leads up to a magnificent simile: "And thus man never understands his fellow-man aright; he picks out a few crystalline threads of an individuality; he sees a few bright or black bands in the spectrum of his neighbour's life, and without touching the colloid mass which will not crystallise, and being blinded to those parts of the spectrum which are not revealed (*sic*), man judges his fellow."

The characteristic of man among living animals is then summed up in the following startling epigram: "In short, man is a cerebrating creature, as the cow is a ruminating creature;" and we immediately afterwards learn that he cannot help cerebrating. Nor, it appears, can he help being a genius—or the reverse—if his brain-cells are fitly formed and he has been judiciously trained,—"and it is quite true that brain-cells do differ in form and composition just in a similar sort of way (!) as lungs and hearts differ."

This naturally leads to the question of moral responsibility, and the difficulty of the materialist is thus stated: "Man, the result [*i.e.*, 'of a predetermining influence in the very fœtus'], steps on to the platform of life in some measure at least an automaton. He is born of others, and finds himself with a head upon his shoulders, but the quantity and quality of brain-matter in the head is not ordained of himself. He may be a genius; but, horror! he may prove a fool!"

We then reach what appears to be the *raison d'être* of the chapter, viz., a conclusion which however nowise follows from any premisses before stated: "Thus even mundane chemical science has a part to play in the *rôle* of what poor mortals call their souls; it has something to do with every poeism (*sic*) originating in the mind of the poet, with every transcendent hope of the philosopher, with the logic of a Mill, and the teeming intelligence of all."

Mr. Kingzett is then arrested by the thought that all the body, brain-cells included, are elaborated from food—a thought which leads him to exclaim, "Eat, drink, and be merry, for verily that which we eat and drink takes part in that with which we think!"

This is Mr. Kingzett's treatment of "character" as a philosophical subject! We can only say, as Dryden once said in a criticism of a play of Elkanah Settle's, "I am mistaken if nonsense is not here pretty thick sown." We challenge any one to find us five such pages of silly reflection and irrelevant twaddle in any other seriously-intended work.

At the end of the book there is an "Index of Authorities Quoted"—not, of course, the Index of the book. Turning up the K's, we find Mr. Kingzett's own name, and under it we discover that Mr. Kingzett is an "authority" on Character—the reference to the book being to p. 462, the very chapter we are discussing. As it nowhere appears that Mr. Kingzett has, in other places, treated of this subject, we have the happily rare spectacle of an author endeavouring to take time and the critics by the forelock by writing himself down an "authority" ere he knows his book will live. Sure self-complacency never touched a loftier pinnacle!

After this, a good anti-climax might have been regarded as hopeless; but Mr. Kingzett has achieved one. He closes his book with a list of "Suggested Matters for Research," in the hope and belief (as he tells us in his preface) that they may be a guide to the "scientific chemist." The "scientific chemist," if he has but a smattering of physiology, will know how to shrug the shoulder at such puerile, general, and useless suggestions as the following:—

"(1) The chemical composition and formula of ptyaline; its chemical relationship to albumin; a proper explanation of its ferment-power, and a better study of its general nature."

"(7) An explanation of the oft-recurring deposition of biliary matters near the pyloric end of the stomach."

"(18) The complete composition of lymph, chyle, and blood."

"(19) Particular studies of the blood-corpuscles."

"(40) Prolonged studies of the physics of the body directed particularly to work out the history of the force generated in blood oxidation."

OUR BOOK SHELF

The Patentee's Manual. By James Johnson and J. Henry Johnson. Fourth Edition. (London: Longmans and Co., 1879.)

THE law relating to letters-patent for inventions, as at present administered, has been the growth of one short sentence in a declaratory statute passed in the twenty-first year of the reign of James I. (A.D. 1623), by which the Crown was restrained from making extravagant or oppressive grants of monopolies. The history or details of patent cases may often form an interesting subject of inquiry for the scientific reader; for although men of the highest intellect may be content with the discovery of general laws, and may leave their useful application and development to the crowd of humbler followers whose only power consists in the exercise of mechanical ingenuity, yet it cannot be denied that the successive steps which have been made in the steam-engine, in the electric telegraph, in machinery for spinning, weaving, or sewing, for manufacturing paper, or for printing a newspaper, may each in turn afford matter of considerable interest to a philosopher whose imagination is wearied with an endeavour to trace the fantastic excursions of a molecule, or to carry his dynamical laws into new and unexplored regions.

A book which shows the manner in which the property in inventions is dealt with in our Courts, and which, in order to accomplish its object, must of necessity review the various cases in an historical and logical order, affords, in a small compass, an epitome of much valuable learning. It is remarkable that the first patent case of any importance involved the validity of Arkwright's invention of machinery for drawing out and spinning cotton (A.D. 1785), while the second occurred ten years later, and related to the invention of the separate condenser of a steam-engine by James Watt. Since that period a number of distinct steps in the useful application of physical or mechanical laws have successively passed the ordeal of judicial inquiry, and those who take up the volume before us will find a reference to such matters as Wheatstone's telegraph, the hot blast for smelting iron, the interlocking of railway points and signals, the operation of currents of air between the grinding surfaces of mill-stones, the combing of wool, the laying of submarine telegraph-wires, &c., and so on in a list which appears almost interminable.

But although the variety of subject-matter may be great, the principles which govern the cases are few and easily comprehended, and, in reading the statements of principles laid down by Chief-Justice Tindal and other judges who have moulded our patent law into a coherent form, the thought may arise that the purely scientific writer who is composing his manual for the use of students might with advantage borrow something of power of style and of clear logical exposition from the lawyer, who is popularly believed to be tied down and hampered by the jargon of technical phraseology.

The book now under notice has already passed through three editions, and the authors have enlarged it by the interpolation of recent cases, as well as by the addition of new chapters. It is not within the scope of this journal to examine such a treatise from a strictly legal point of view, but we should describe it as exhibiting abundant evidence of being the work of writers who are practically engaged in professional pursuits. One important appendix consists of a digest of the patent laws in force in foreign countries, and in the body of the work there is a chapter on the "oppositions to the grant of patents," which suggests many melancholy thoughts to the sanguine inventor, and leads one to hope that some improvement of the enactment of 1852 will be conceded at an early period. In conclusion we have only to say that the book has fairly earned the circulation which has carried it to a fourth edition.

T. M. G.

A Manual of the Carbon Process of Photography, &c. By Dr. Paul E. Liesegang. Translated from the German by R. B. Marston. With Illustrations. (London: Sampson Low, Marston, Searle, and Rivington.)

WHEN, forty years ago, Mungo Ponton discovered that a sheet of paper, moistened with a solution of potassium dichromate, became darker when exposed to the rays of the sun, he made the first of a series of experiments which have led to the discovery of a method of rendering photographic pictures as permanent as engravings made in printing ink, though the completion of the work to a point at which it could fairly be said to be capable of competing with the well-known silver chloride print was not made till nearly thirty years afterwards, when Swan, by an admirable series of inventions, made it a practical means of producing prints.

In the history of the long struggle with nature which has produced so great a result every Englishman has reason to be proud, for it may be fairly said that the world owes the process from first to last to English workers. The process is now worked on an immense scale in this country by the Autotype Company and others, while another branch of the same stem has developed into the well-known Woodbury-type system of press-printing. Notwithstanding, however, the success of the process in its original home, we are somewhat deficient in connected accounts of it, most of the English publications on the matter being, like the autotype manual, confined to working details of the methods in use. We therefore welcome Dr. Liesegang's work as attempting something more than this, and presenting what is really a most interesting account of the whole subject, interesting, indeed, to any one who has a taste for well-written scientific technology, and apart from its value as a manual for actual working details. In one respect, indeed, the carbon process has all through been singularly fortunate. It seems, from the first, to have fallen into competent scientific hands, and to have escaped the dreary round of mess and muddle experimenting which is so characteristic of the history of the collodion negative processes, and which reminds one of nothing so strongly as of the story ascribing the invention of a certain process for the purification of sewage to its inventor going into a laboratory and taking down bottles at random, to the number of some half dozen, adding their contents to a sample of sewage, and patenting the mixture. From this misfortune the carbon process has been free, and Dr. Liesegang has been able to make its history instructive and interesting; he has given clear and precise accounts of the processes in use, and we note that he has kept well up with the latest improvements, while the illustrations are well and clearly cut. The popularity of the work in Germany has caused no less than six editions to be demanded.

It would be unfair to close this notice without a word of praise to the translator, who, in a modest note, states that his share of the work was done in leisure hours. We can only wish that he will continue, as he has begun, to introduce sterling foreign technical works to the public in as vigorous and correct English as that in which he has dressed Dr. Liesegang's little book.

R. J. F.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Gulf-Weed (*Sargassum bacciferum*) a Means of Migration for Fishes and Marine Invertebrates

OWING to the October number of NATURE having been mislaid, I have not had an opportunity until lately of seeing Mrs. Merrifield's remarks upon Gulf-weed which appear in vol. xviii. p. 708, where the Bermudas are alluded to as a locality where this species grows *in situ*.

Having during my several visits to those islands of late years paid some attention to the Sargassum and its inhabitants, perhaps the few facts I am in possession of may prove interesting to botanists, and those who study the geographical distribution of marine animals.

The Bermudas, being situate within that somewhat circular area of the North Atlantic, formed by the currents of the Gulf Stream, the North African, and equatorial currents, within which exists that vast accumulation of weed known from the time of Columbus to the present day as the "Sargasso Sea," afford excellent opportunities for studying the plant in its floating condition, and also adherent in its natural state to the reef. During the winter months the prevailing gales, which are generally from south-east to south-west, bring to the islands large fields, as well as isolated patches, of the Gulf-weed, which prove a great boon to Bermudan farmers, who, but for this ocean waif, would often be minus manure sufficient to raise their root-crops with. To an observer a field of weed coming in from sea presents a somewhat variegated surface as regards colour, the major portion of it being of a dark brown, interspersed with spots and patches of light yellow. On closer inspection, these masses of floating weed are found to be inhabited by various species of pelagic and littoral crustaceans, particularly a small light brown crab, having a blotch of white on the carapace. Here and there the eye rests on a little pearly-white object, the well-known shell of that almost unknown cephalopod, *Spirula prototypus*, of Peron. The pretty purple shell of *Ianthina communis* is also to be seen, as are the singular forms of those truly oceanic acalephs, *Valella communis* and *Physalia pelagica*, which occasionally occur in large numbers, as they did during a heavy southerly gale on April 16, 1861, when countless myriads were literally wrecked upon the shores, together with the shells and rafts of Ianthinæ. About the margins of these floating fields, which are of some depth, may be seen various species of fishes, most of which have, no doubt, accompanied the fields, and lived in them, as game would do in a preserve where food and shelter are found. There is one species of fish which, above all others, seems to belong to the Sargassum, viz., the Marbled Angler (*Antannarius marmoratus*), which, from its peculiar arm-like pectorals, is specially fitted to rest upon the weed. Here it makes its wonderful nest amidst the mass, suspended by means of those silk-like fibres, which prove amply strong enough to support the large bunches of eggs, which hang like grape clusters within their orbicular case. These nests are occasionally to be found, but cannot be considered common; and only a few have been obtained from the weed on the Bermudan shore.

There is hardly a doubt that it is from this fish-preserve in mid-Atlantic that those tropical and semi-tropical forms which occur incidentally at the Bermudas, Azores, Canaries, Madeira, and also on the east coast of America, come, for I have frequently obtained from these masses of gulf-weed, species which are not recognised as Bermudan, and would probably never have visited the island waters unless under the friendly shelter of the weed. Moreover, I have observed even in heavy storms that the sea never breaks throughout these floating fields, but although heaving and swelling to the usual height, remains unruffled just as if oil floated on the surface. This absence of disturbance would of itself commend the field of weed to the fishes; but when we consider other suitable adjuncts, such as supply of food, and shelter from enemies, we cannot fail to realise the excellent means of migration which this common possession affords, not only to fishes, but to all kinds of those lower invertebrate forms, many of which have most certainly been brought to the shores of the Bermudas by this means. The isolated patches of weed, which follow the course of the Gulf Stream, and become broken into lesser fragments, are also accompanied by those tropical and semi-tropical fishes which are found almost every summer on the coast of Nova Scotia, and even as far north as Newfoundland; and it is evident that without some such agency we could never account for the abundance of certain southern pelagic fishes which annually occur in our high latitude.

In regard to the original habitat of *S. bacciferum*, as also the origin of that vast mass of floating weed which exists in mid-Atlantic, and is wholly composed of this species, I fear we must await further oceanic exploration. Although I am well aware that it grows in certain places on the Bermuda shores, those shores, even if they were wholly clothed with it, could not supply a tithe of the material which forms the vast accumulation of the weed existing in the Sargasso Sea. As to the allusion in Mrs. Merrifield's paper (quoting Agardh), made concerning the *S. bacciferum* being an inhabitant of the banks of Newfoundland, and other parts of the coast of north-east America, I can safely say that it is wholly unknown on this coast, save occasional sprays, which are brought north by the Gulf Stream, as are the fishes I have before alluded to.

Halifax, Nova Scotia, January 25 J. MATTHEW JONES

The Highest Tide on Record

IN Lyell's "Principles of Geology," tenth edition, 1867, vol. i. p. 494, occurs a statement, given on the authority of Admiral Sir F. Beaufort, to the effect that the tides at Chepstow on the Wye sometimes rise to 69 and even to 72 feet. The statement is familiar to all who have read Lyell's work. If it be correct then this tide of 72 feet at Chepstow is apparently the greatest in the world, that in the Bay of Fundy being given as 70 feet in the extreme. I can find no authority for a tide so great as 72 feet at Chepstow other than that above cited. The old "Bristol Channel Pilot" books of 1821 and 1839 say nothing of the matter, as I am informed by Capt. Tizard, R.N., and the latest published "Pilot" gives 56 feet as the extreme rise of tide at Chepstow. There is thus no official knowledge of so high a tide as 72 feet, and I can find no published account of Admiral Sir F. Beaufort's observations; Sir C. Lyell refers to none such.

I should be extremely obliged to any reader of NATURE who can refer me to any certain record of exceptionally high tides at Chepstow and confirmation of Sir C. Lyell's statement. There seems to be some uncertainty as to whether the highest tides on record occur in the British Islands or not.

Exeter College, Oxford H. N. MOSELEY

The Glacial Period and Geographical Distribution

PROF. ASA GRAY, in his very interesting lecture on the distribution of the forest trees of the northern temperate region (NATURE, vol. xix. p. 327), after pointing out the remarkable differences that exist between the forests of the eastern and western sides both of North America and the Old World, suggests that the great poverty of the European as compared with the Japan-Manchurian region in this respect was caused by the Mediterranean cutting off the retreat of the flora which then occupied Europe, as it retired, at the approach of the glacial epoch, before the ice from the north. This explanation derives considerable support from some other facts in geographical distribution. The most characteristic Alpine and Arctic butterflies of the Palæarctic region belong to the three genera, *Parnassius*, *Chionobas*, and *Erebia*. Of *Parnassius*, Dr. Staudinger, in his latest catalogue (1871) enumerates fourteen Palæarctic species, of which three occur in North and Central Europe, ranging as far south as the Balkans, but always in or near high lands, about a dozen occur in temperate Asia, ranging as far east as the Amur, and probably as many in North America, where they also are truly Alpine butterflies. Of *Chionobas* one species (*C. aëllo*, confined to the Alps) occurs in Central Europe, whilst six or seven others range from Lapland over Russia and Siberia, Mongolia, &c., to the Amur, and there are numerous species in Arctic and Alpine North America. Of *Erebia* there are forty-five Palæarctic species enumerated by Staudinger, and of these no less than twenty-five occur in the central Alpine chains of Europe. The genus likewise ranges all over temperate Asia, going as far south as the Himalayas and Moupin, and in North

America is represented by a dozen or more species. Now, though an *Erebia* (*E. Tyndarus*, var.) occurs as far south in Europe as the Sierra Nevada, *not a single species of any* of these three genera occurs in North Africa, although the Atlas Mountains would seem eminently well suited for such Alpine insects. In this case, then, it seems clear that the same cause—the barrier of the Mediterranean—which in the case of the miocene flora of Europe prevented any further retreat south, has operated to prevent any similar southerly spread amongst the victorious invaders from the north which pressed on the retiring host.

With regard to the general similarity in facies and richness between the East American and East Asiatic tree-flora, certain facts pointing in the same direction will at once occur to the zoologist. Thus the *Menopomas* of the Ohio and Alleghany have their only near relations in the gigantic *Sieboldias* of north-east Asia, one species of these occurring in Japan, the other being one of Père David's discoveries in Moupin. Similarly with the genus *Polyodon* amongst ganoids. Only two species of this genus are at present known, *P. folium*, inhabiting the Mississippi, *P. gladius* the Yang-tse-kiang. The recent discovery of at least two species of *Scaphi rhynchus* in Turkestan makes it probable that ere long species of that Americo-Asian genus will be found in the Chinese rivers as well. The parallelism in the case of the salamanders is particularly interesting, when one remembers the celebrated *Andrias Scheuchzeri* of the Œningen beds, and it tends to favour the view that at that time practical identity in the forms of animals and plants reigned throughout the northern temperate zone. W. A. FORBES

Cambridge, February 14

P.S.—The reported discovery (NATURE, vol. xix. p. 351) of a true *alligator* in the Yang-tse-kiang, will, if confirmed, add a still more remarkable case to those mentioned above.

Leibnitz and the Royal Society

PROF. TAIT and myself ought not to be at issue on this question. I suppose we both want to get at the facts; and, for my part, I have no more desire to whitewash a foul reputation than he can have to blacken a fair one. Where we differ appears to be, as to how far Leibnitz's reputation can stand the test of facts. The question, however, is not whether Leibnitz acted disingenuously in respect to Gregory's series, or any other subordinate matter, but whether he was indebted to something of Newton's, surreptitiously imparted to him, for his differential calculus. If the grounds upon which that charge was made are swept away, there is an end of it. But if, on the other hand, that is not found feasible, and evidence to character becomes a factor in the final decision, then it is right to examine into those subordinate matters. Till then, I, for one, decline to touch them. At the same time let me say that I never undertook to be bail for Leibnitz's impeccability. All I said or say is, that on the published facts I believe that Leibnitz was led to the calculus by his own honest speculations, and *had not the means* of stealing from Newton, had he been that way disposed. But there are so many relative papers still unpublished, but publishable, that it is impossible to arrive at a true decision till at least some of them have been submitted to an authorised tribunal.

Prof. Tait recommends me to repeat the fruitless attempt of Dr. Slowman. I decline to follow the example of that ominously surnamed *savant*; for it is contrary to precedent that the pursuer should ask the defender to show his hand; and I am quite sure that "the proper authorities" abroad have too much sense to take the initiative. So I appeal to the Council of the Royal Society of 1879 (not that of 1712, as Prof. Tait gives it), and I do so for these two reasons:—

1. The so-called *Commercium* of 1712, which was merely a statement, arriving at no decision on the principal question, contained several allegations (apparently inconsistent with known facts) which give colour to the charge against Leibnitz; it is then an obvious duty on the part of the Royal Society, who were on the occasion represented by the Committee, to give the proof, or make the reparation.

2. The first-published charge against Leibnitz, which was made by Wallis in 1695, was based on allegations said to have been derived from papers and letters in the possession of the Royal Society; it is but fair, then, that those papers and letters should be published.

I therefore once more respectfully urge upon the Royal Society to reopen the main question, and publish such of the relative papers, &c., in their possession as directly bear upon the original charge. C. M. INGLEBY

Athenæum Club, February 8

Ear Affection

THE experience of "P." as given in NATURE, vol. xix. p. 315, is physiologically interesting, and by no means usual. Before attempting an explanation it may be as well to assume that only one of "P.'s" ears was affected by the disorder, as by this hypothesis we get the greatest possible divergence from the healthy state. It would have been easy to ascertain which was the faulty organ at the time by requesting a musical friend to listen while "P." vocalised the note of the tuning-fork as conveyed to him by each ear separately. The discordant ear would then have been revealed.

The fault of hearing must have been due either to some mechanical misadjustment of the auditory apparatus, by which a wrong sensation was conveyed to the brain, or else to some deep-seated brain or nerve lesion, which led to a faulty conception of the original sound. Let us consider briefly the first of these cases.

From the exceedingly scanty description of his disorder given by "P." I gather that the discord was mostly conspicuous when the note was high pitched (such as when whistled). Now it sometimes happens from paralysis of the chorda tympani nerve, or even from occlusion of the Eustachian tube, that the tension of the ear-drum is preternaturally increased. Such affections, as aurists well know, frequently intensify to a distressing degree the hearing of high pitched notes, whilst they correspondingly diminish the sound of the lower tones of the chromatic scale. This result is probably obtained by the fact that the tense membrane responds more readily to the rapid vibrations of the higher tones than it does to those of a slower rate. We must also remember that the power of lessening the tension of the membrane is in such cases very seriously impaired, and, as a consequence, the power of adjustment also. I do not suppose that in "P.'s" case there was any actual paralysis of the tympanic muscles, but it is just possible that there may have been a certain degree of misadjustment of the drum of the affected ear due to a feeble and imperfect contraction of one or the other of the muscles referred to. If the disorder was, as I surmised, accompanied with great tenseness of the membrane, the laxator tympani would be the faulty muscle. We might, I believe, under such circumstances, expect the ear-drum to vibrate discordantly in response to a note, for Helmholtz's experiments with stretched strings would suggest that this is feasible within certain limits. As a matter of fact this discordance is rare, and therein rests the interest of "P.'s" case.

I can scarcely believe that in his case any of the deeper structures of the ear were seriously implicated, otherwise he would hardly have made such a rapid and complete recovery as he did.

Brighton, February 10 W. AINSLIE HOLLIS

YOUR correspondent "P." (NATURE, vol. xix. p. 315) desires an explanation of the phenomenon of alteration in the pitch of sounds, which he has experienced in his own person whilst suffering from temporary deafness. Your second correspondent on this subject, Dr. Wallich (p. 340), was under my observation at the time of his experiencing the same peculiar and comparatively rare aberration, and I was able myself to verify his statements.

I propose with your permission to give an explanation which appears satisfactory to myself, and hope it may be so to your correspondent "P."

Persons suffering in this way find that sounds heard by the affected ear appear to be sharper or flatter than their true pitch as heard by the other ear, and hence a sound may even appear double.

The internal ear, or labyrinth, must be the part affected, and in all probability it is the cochlea which is at fault. Now most authorities are agreed that the pitch of a sound is appreciated by the cochlea in the following manner. Each tone, or division of a tone, has its corresponding portion on the spiral lamina of the cochlea, which under ordinary circumstances can only be affected by that tone. So that the sound-wave produced by a certain tone passes along the keyboard (as it were) of the spiral lamina until it reaches its own key, which it strikes or so affects as to cause an impression to be sent from that portion of the lamina to the brain. Hence the appreciation of variation in the pitch of sounds.

This theory being accepted, for an explanation of the aberration in question we have only to suppose some slight physical alteration in the contents of the cochlea, which would cause the sound wave to strike or affect the wrong portion of the lamina spiralis, and thus a false impression would be carried to the brain. URBAN PRITCHARD

Now attention is drawn to the above allow me to give another experience.

On two separate occasions while playing the English concertina, and more particularly when single notes or simple chords were struck, I noticed that each was followed by a loud and distinct note an octave lower which appeared to be that of its fundamental tone. The musical tones of the voice of any person addressing me, also, had their deeper reverberations in a similar manner, these being numerous and of rapid succession; the confusion arising was very like that which is heard in a hall unsuitably constructed for sound.

The nuisance, for such it amounted to, I was troubled with for a couple of days each visitation, the abnormal state of hearing being peculiar to the left ear only. JOHN HARMER

Wick, near Arundel

Intellect in Brutes

THE following case will perhaps interest those who believe that the reasoning faculty in man and animals differs in degree only, and is essentially the same in kind. Some years ago a plumber told me that he had, on several occasions, been called in to examine into the cause of leakage of water-pipes under the flooring of houses, and had found that the rats had gnawed a hole in the leaden pipe to obtain water, and that great numbers of them had made it a common drinking-place, as evidenced by the quantity of dung lying about. The plumber brought me a piece of leaden pipe, about $\frac{3}{4}$ inch in diameter and $\frac{1}{8}$ inch in thickness, penetrated in two places, taken by himself from a house on Haverstock Hill. There are the marks of the incisors on the lead, as clear as an engraving; and a few hairs and two or three of the rats' vibrissæ have been pinched into the metal in the act of gnawing it. This crucial proof of brute intelligence—a rat will not drink foul water—interested me so much, that I ventured to send an account of it to Dr. Chas. Darwin, asking his opinion on the means by which the rats ascertained the presence of water in the pipe. To this he replied: "I cannot doubt about animals reasoning in a practical fashion. The case of rats is very curious. Do not they hear the water trickling?" It may be conceded that this explanation is the most probable, and if it be the true one we have an example of an animal using his senses to obtain the data for a process of reasoning, leading to conclusions about which he is so certain that he will go to the trouble of cutting through a considerable thickness of lead. Obviously man could do *no more* under the same conditions. ARTHUR NICOLS

OUR ASTRONOMICAL COLUMN

THE COMPANION OF ALGOL.—There are grounds for suspecting that the light of the small star about 80″ distant from Algol in the S.P. quadrant is also variable. Schröter in his letter to Bode, wherein he first drew attention to this object, mentions that he detected it with a 7-feet reflector on October 12, 1787, and although small it was distinctly seen. Soon afterwards he estimated its distance from Algol at 1′ 30″. On April 9, 1788, the star was not to be found, and he therefore concluded that it must be variable. In 1792, when he was in possession of a 13-feet reflector, which he describes as the most powerful instrument then available in Germany, he re-examined the vicinity of Algol, and on March 9 saw the companion much brighter than before, and compares its distinctness in the larger telescope with its faintness in the smaller one with which he had discovered it. But on April 5, in a state of atmosphere at least as favourable as on March 9, with the same instrument and magnifying power, not the slightest trace of the companion could be perceived; on increasing the power to 370, with the utmost straining of the eye, the faintest glimmering was now and then suspected in its position. Schröter then, in this second communication to Bode, expresses himself more confidently as to the variability of the small star.

In the early part of the year 1874 the writer of these lines made several ineffectual attempts to observe the companion, using various powers on a 7-inch refractor; though the skies were favourable enough, nothing could be glimpsed in its place. It was not therefore without surprise that upon re-examining the vicinity under similar conditions on September 9 of the same year, the companion was caught at once, and seen with great distinctness. It was measured with Mr. J. G. Barclay's 10-inch refractor at Leyton, by Mr. Talmage, on October 2 following, when the angle was found to be 194°·4 and the distance 79″·02; the magnitude was estimated 11·12. An observation by Smyth in 1835 is recorded, but his distance is much too small; it is not stated whether he found the companion himself or whether his knowledge of its existence was due to Schröter's communications to Bode. It does not occur amongst the objects in the "Bedford Cycle," which were re-measured by Secchi.

While upon the subject of variable stars we may just mention that ι Andromedæ, to which attention is directed in the last number of the *Monthly Notices* of the Royal Astronomical Society as "a new variable star," is no novelty: we referred to the star as almost certainly entitled to insertion in the catalogues of such objects, four years since (NATURE, vol. xi. p. 308).

"A MISSING STAR."—From a letter addressed by Prof. C. H. F. Peters, Director of the Observatory, Clinton, New York, to the Superintendent of the Naval Observatory, Washington, which Admiral Rodgers has communicated to the *Astronomische Nachrichten* (No. 2240), it appears that he has strangely misinterpreted a note with the above heading, which was lately printed in this column. We referred to an object observed at Washington, with *Hygeia* in 1850, and afterwards sought for at that observatory and elsewhere on the assumption that it might possibly have been a trans-Neptunian planet, and in view of the failure of a careful search on this hypothesis, we remarked: "the only likely explanation appears to be that there was a variable star in this position, and that the observations in right ascension were affected with greater error than might be expected, considering that on two of the days of observation several comparisons were made." Prof. Peters, however, explains the difficulty by referring several transits to the first instead of to the second wire of the movable plate of the micrometer employed, in which case the star is identified with Lalande 36613, and Prof. Hall has found, on examining the original observing-books, that Mr. Ferguson had altered several correct observations to correspond with erroneous ones, and Admiral Rodgers accepts the explanation as satisfactory. But Prof. Peters is alarmed about the matter now that NATURE "stirs it up again," and writes to the Superintendent of the Washington Observatory "in order that nobody thereby might be induced to spend months and years upon a renewed search," and to "stop any further perpetuation of the credence, that a trans-Neptunian planet is revealed by the Washington Observations." It will be seen that our suggestion was that a variable star might exist in the observed position, and was in no way connected with a renewed search for a trans-Neptunian planet. Prof. Peters must entertain rather odd notions as to the probable knowledge of his astronomical *confrères* respecting the contents of the ecliptical region of the sky, if he believes that any one would be induced, by remarks that we might offer, to undertake in these days a search for a distant planet close to the ecliptic amongst stars of the *ninth* magnitude!

COMET 1871 V.—Dr. B. A. Gould, with his usual energy, has secured an excellent series of post-perihelion places of the comet discovered by Dr. Tempel on November 3, 1871, which in a fortnight's time sank below

the European horizon. The discussion of these observations in conjunction with those made in the northern hemisphere, will lead to a much more precise knowledge of the orbit than we have at present.

Olbers' Comet of 1815.—In a recent note upon this comet it should have been stated that, acting upon the wish expressed by Olbers at the time, Triesnecker printed his observed differences of right ascension and declination between the comet and comparison-stars in *Zeitschrift für Astronomie*, vol. ii. The Vienna observations, therefore, admit of a new reduction, in addition to those previously named.

DIURNAL OSCILLATIONS OF THE BAROMETER

IN the "Meteorological Notes" which appeared in Nature, vol. xviii. p. 198, some interesting results are referred to, which show marked differences in the diurnal variations of the barometer at places quite near to each other, as Greenwich, Kew, Oxford. It is remarked especially that the forenoon maximum in the months of May to July occurs near 9 A.M. at Greenwich, and near 8 A.M. at Kew; while at Falmouth and Valentia it is delayed to 11 A.M., or noon, and occurs in June as late as 2 P.M. at Helder.

Having made several investigations relatively to these questions (which I have not been able to publish as yet in detail), I think it may not be without advantage to give at present conclusions relating to the results above noticed.

It is obvious that it is of the highest importance with relation to the research as to the cause or causes of the remarkable semi-diurnal oscillations of the barometer, that we should have only real variations of atmospheric pressure to deal with, and not instrumental irregularities; and that, if there is any part of the mean diurnal variations which is due to local causes, we should be able to separate that part from any other which may be due to general or cosmic causes.

When it is remembered that the range of the mean diurnal variation with us is from two to three hundredths of an inch of mercury, and that the epochs of maximum or minimum may be shifted an hour by a difference of one or two thousandths of an inch, it will be seen how essential it is that the instruments, the observations, and the corrections shall be the best, in order to be sure that we have real variations of atmospheric pressure before us.

In order to obtain the best possible results, my investigations have been limited to observations made in first-class observatories with standard instruments. From observations made during several years at Makerstoun, Dublin, Greenwich, and Brussels, I have sought by the harmonic analysis the functions of sines which represent them most accurately. I give here the equations for the means of the three months in question—May, June, and July. The variation, v, is in ten-thousandths of an inch of mercury; the origin for each of the four stations M, D, G, and B, is mean midnight ($\theta = 0$):—

$$\begin{aligned}
\text{M},\ v &= 56 \sin(\theta + 355^\circ) + 68 \sin(2\theta + 143^\circ) + 21 \sin(3\theta + 171^\circ)\\
\text{D},\ v &= 51 \sin(\theta + 358^\circ) + 72 \sin(2\theta + 144^\circ) + 25 \sin(3\theta + 153^\circ)\\
\text{G},\ v &= 63 \sin(\theta + 346^\circ) + 88 \sin(2\theta + 143^\circ) + 25 \sin(3\theta + 154^\circ)\\
\text{B},\ v &= 43 \sin(\theta + 354^\circ) + 92 \sin(2\theta + 140^\circ) + 24 \sin(3\theta + 170^\circ)
\end{aligned}$$

The terms on the right of each equation represent the oscillations, whose superposition completes the whole diurnal variation. We find—

From the 1st term that the epochs of the maximum and minimum were the same within a few minutes at M, D, and B (as shown by the arguments 355°, 358°, and 354°), differing at Greenwich from the others by about 40m.

From the 2nd term, that of the semi-diurnal oscillation, that the epochs were the same at all the stations within a few minutes.

From the 3rd term, that they agreed at D and G and at M and B, those for the former being about 23m. different from those for the latter.

When we consider the coefficients of the different terms, which represent half the ranges of the oscillations, slight differences are found for the 1st and 3rd terms; for the 2nd the *range diminishes regularly as the latitude increases* at the rate of 0·00101 inch for each degree of latitude.

The exact agreement in the epochs of maxima and minima and the regularity of the variation of range with latitude in the semi-diurnal oscillation show that this oscillation obeys a general law. Dr. Lamont has supposed that the 1st term, or single oscillation, is due to variation of temperature; this, I believe, is not the case. When we compare the terms for different *seasons* of the year, we find that for the *same place* the epochs of maximum and minimum may vary twelve hours in the single oscillation, while the epochs deduced from the same term for the temperature variations do not differ one hour. Not only so, I have found on the South Indian Ghats that the epochs deduced from the 1st term of the barometric equations vary seven hours in ascending 6,000 feet; while those shown by the 2nd term are absolutely constant.

For all these reasons I conclude that the semi-diurnal oscillation of the atmospheric pressure is due to a cosmic cause, independent of local influences, while the single diurnal oscillation shows that part of the solar action which is modified by atmospheric conditions yet to be determined. The results for the four stations just given are a few links in a long chain of facts which tend to prove that the semi-diurnal oscillation of the barometer is due to an action of the sun, which is repeated equally, twice in each day, like the solar oceanic tide.

It will be seen, I think, from the results obtained from the Brussels, Greenwich, Dublin, and Makerstoun observations that the differences noticed at the beginning of this article cannot be allowed to enter as data into the domain of meteorology without much greater study of all the circumstances on which they depend. The facts of atmospheric variations are very difficult of explanation, but if we begin to admit results which may be purely instrumental among these facts explanation will become impossible.

It is a fact that the true temperature of the mercurial columns has not always been obtained, and when we have to discuss observations with self-registering instruments, many sources of error, including those of temperature on the apparatus itself, have to be cared for.[1] At stations near the sea, such as Helder, Valentia, and Falmouth, we have also to remember that in the varying height of the partial base of the atmosphere, through the *solar* oceanic tide, there is a real cause of diurnal barometric oscillation whose amount and epochs should be ascertained and deducted before exact comparisons can be made with observations inland. At the same time I would remark that of the stations here considered Dublin is near the sea, while the others are more or less distant from it.

John Allan Broun

MAROCCO AND THE ATLAS[2]

THE expedition of which an account is given in this most interesting volume was undertaken by Sir Joseph Hooker and Mr. Ball in the spring of 1871, and lasted a little over two months. Many causes com-

[1] The observations here studied at the four observatories are all made by the eye.

[2] "Journal of a Tour in Marocco and the Great Atlas." By Joseph Dalton Hooker, K.C.S.I., C.B., Pres. R.S., Director of the Royal Gardens, Kew; and John Ball, F.R.S., M.R.I.A. With an Appendix, including a sketch of the Geology of Marocco, by George Maw, F.L.S. (London: Macmillan and Co., 1878.)

bined to hinder the publication of this journal until now, but the delay will, we feel sure, not in the least detract from either the novelty or value of its contents.

The coast-line of the territory of Marocco extends from the frontier line of Algeria on the Mediterranean, along through the Straits of Gibraltar to the Atlantic Ocean, nearly opposite to the Canary Islands. Inland it would seem to have no absolutely well-defined boundary-line except towards Algeria, but it is supposed to stretch far into the Great Desert on the southern side of the Great Atlas range. Of this great region, possibly as large in extent as Spain, and the chief ports of which are within a few days' steaming of Southampton, almost nothing, if we except the mere coast-line, is known; and a journey across the dark continent itself seems easier of accomplishment than one to the town of Tarudant, in the southern valley of the Great Atlas.

The distance from Gibraltar to Tangiers is scarcely thirty-five miles. The five peaks of the Beni Hassan are seen from the Rock, yet we have no records of their having been investigated, and when the authors earnestly desired Sir John Drummond Hay, the British Minister Plenipotentiary in Marocco, to assist them in exploring their recesses, he was reluctantly obliged to pronounce against the feasibility of any such excursion, and even when they started for a short excursion to Cape Spartel it was considered prudent to give them an escort of two soldiers.

Argan Trees.

With this excursion the botanical interest of this journal begins. Plants of many sorts were seen and collected. Where a little slender stream ran through some damp meadows they were charmed by the delicate tint of a pale blue daisy that enamelled the green turf. It was merely a variety of the little annual daisy (*Bellis annua*) so common in many parts of Southern Europe; but the blue tint does not seem to have been noticed elsewhere. The botanical district to which this northern corner of Marocco belongs has been called that of the cistus and heath. One very interesting plant to be often seen living in the Botanical Gardens of Kew and Dublin is thus described:—

"The most singular of these is the *Drosophyllum lusitanicum*, a plant of the sun-dew tribe, whose branched stem bears several large yellow flowers. The numerous slender strap-shaped root-leaves, nearly a foot in length, that are gradually contracted to the thickness of whipcord, are beset with pellucid ruby-tipped glands, and present a peculiarity that appears to be unique in the vegetable kingdom. Any one who has remarked the growth of ferns must have seen that in the young state the leaves are rolled or curled inwards, so that in the process of unfolding the face or upper side of the leaf, which was at first concealed, is gradually opened and turned to the light. A similar process occurs in many other plants; but in *Drosophyllum* alone, so far as we know, the young leaf is rolled or curled the reverse way, so that the upper side of the leaf is that turned outwards. It appears to grow in many parts of Southern Portugal; reappears on the north side of the Straits of Gibraltar near Tarifa and Algeciras, and on the southern side about Cape Spartel and on the hills above Tetuan, where it commands a view of the opening of the Mediterranean, but extends no farther eastward."

Tetuan was visited from Tangiers. The season (April 10) was scarcely far enough advanced for the flowering of many of the sea-side plants, "but there was more than enough to rejoice the heart of a botanist, especially one escaping from the ghastly spring season of the north, where, when the days grow longer, they become only the more dreary, and where the bitter east winds parch and blast the young leaves and blossoms that are tempted to unfold themselves to their own destroying by the mildness of the winter weather." At Tetuan, Beni Hosmar was ascended. It was about 3,000 feet high, and had not been ascended by any European since Barker Webb's time. The season was still too little advanced, and the botanist who will follow the travellers' footsteps about the beginning of June is promised a much richer harvest. Ceuta, about thirty miles from Tetuan, was next visited, and in order to catch the steamer to Mogador, our authors had to cross over to Gibraltar, from whence they again departed on April 20, in the *Vérité*, landing for a brief moment at Casa Blanca. They were in the port of Mogador on the 26th.

Arrangements were soon made, through the goodness of the late Consul Carstensen, for the excursion to the Great Atlas. Mules were bought, the question of costume was decided, the interpreter of the Consulate, Abraham by name, was lent for the trip. The necessary escort consisted of four soldiers under the command of a captain. The necessary dinner at the Governor's was eaten, and about 7 A.M. on the morning of April 29 the cavalcade took its departure from Mogador for Marocco. Instead of following the direct road, a detour nearly at right angles was made, to enable the botanists to gain a fuller acquaintance with the great Argan Forest. Their course was first through a sandy soil, but as it rose and receded a little from the coast, the tertiary calcareous rock that underlies the sand cropped out here and there, and the first Argan trees appeared.

"As we advanced, the trees grew larger and nearer together, and as we approached our intended halt, at a place called Douar Arifi, they formed a continuous forest.

"The Argan tree is in many respects the most remarkable plant of South Marocco; and it attracts the more attention as it is the only tree that commonly attains a large size, and forms a conspicuous feature of the landscape in the low country near the coast. In structure and properties it is nearly allied to the tropical genus *Sideroxylon* (Iron-wood); but there is enough of general resemblance, both in its mode of growth and its economic uses, to the familiar olive tree of the Mediterranean

region to make it the local representative of that plant. Its home is the sub-littoral zone of South-western Marocco, where it is common between the rivers Tensift and Sous. A few scattered trees only are said to be found north of the Tensift; but it seems to be not infrequent in the hilly district between the Sous and the river of Oued Noun, making the total length of its area about 200 miles. Extending from near the coast for a distance of thirty or forty miles inland, it is absolutely unknown elsewhere in the world. The trunk always divides at a height of eight or ten feet from the ground, and sends out numerous spreading, nearly horizontal branches. The growth is apparently very slow, and the trees that attain a girth of twelve to fifteen feet are probably of great antiquity. The minor branches and young shoots are beset with stiff thick spines, and the leaves are like those of the olive in shape, but of a fuller green, somewhat paler on the under side. Unlike the olive, the wood is of extreme hardness, and seemingly indestructible by insects, as we saw no example of a hollow trunk. The fruit, much like a large olive in appearance, but varying much in size and shape, is greedily devoured by goats, sheep, camels, and cows, but refused by horses and mules; its hard kernel furnishes the oil which replaces that of the olive in the cookery of South Marocco, and is so unpleasant to the unaccustomed palate of Europeans. The annexed cut, showing an average Argan, about twenty-five feet in height, and covering a space of sixty or seventy feet in diameter, with another, where goats are seen feeding on the fruit, exhibits a scene which at first much amused us, as we had not been accustomed to consider the goat as an arboreal quadruped. Owing to the spreading habit of the branches, which in the older trees approach very near to the ground, no young seedlings are seen where the trees are near together, and but little vegetation, excepting small annuals; but in open places, and on the outer skirts of the forest, there grows in abundance a peculiar species of Thyme (*T. Broussonnetii*), with broadly ovate leaves and bracts that are coloured red or purple, and the characteristic strong scent of that tribe. It is interesting to the botanist as an endemic species, occupying almost exactly the same geographical area as the Argan. As we afterwards found, it is replaced in the interior of the country by an allied, but quite distinct, species. Its penetrating odour seems to be noxious to moths, as the dried twigs and leaves are much used in Mogador, and found effectual for the preservation of woollen stuffs."

Stopping at Shedma, Aïn Oumast, Sheshaoua, Misra ben Kara, Marocco was at last in view. From whatever side it be approached, this city presents an imposing appearance. The western side presented an outline about a mile and a half in length. Massive walls some thirty feet in height, with square towers at intervals of about 170 yards, completely inclose it, and on two sides at least it is girdled by a wide belt of gardens in which the date palm, the olive, and fig, are conspicuous objects. We must refer the reader to the volume for an account of the sojourn in Marocco. Some difficulties with the Governor were got over by the quiet determination of Sir Joseph Hooker, whose knowledge of the Oriental character acquired in Asia here stood him in good service.

The outline of the Great Atlas range was quite visible from the terraced roof of the house in Marocco occupied by the travellers, though owing to the prevalence of clouds they failed to secure a satisfactory sketch of these. Through the kindness of Sir J. D. Hay, they are, however, enabled to insert a copy of a drawing made in 1829 by Mr. William Prinsep, the correctness of which they endorse. On May 8 Marocco was left for the mountains. The cavalcade was a large one, consisting of thirty-seven souls and thirty-three horses and mules. The baggage formed a good load for nine mules. The route lay south-east, but the upward slope became hardly perceptible, when before sunset they were compelled to stop for the night at the house of the Kaïd of Mesfioua, at an elevation of about 2,400 feet over the sea. The next morning they were off pretty early, and soon began to ascend, often riding along hollow ways between high banks or lofty hedges formed of tangled shrubs and climbing plants, in which were mingled some familiar forms with several altogether new.

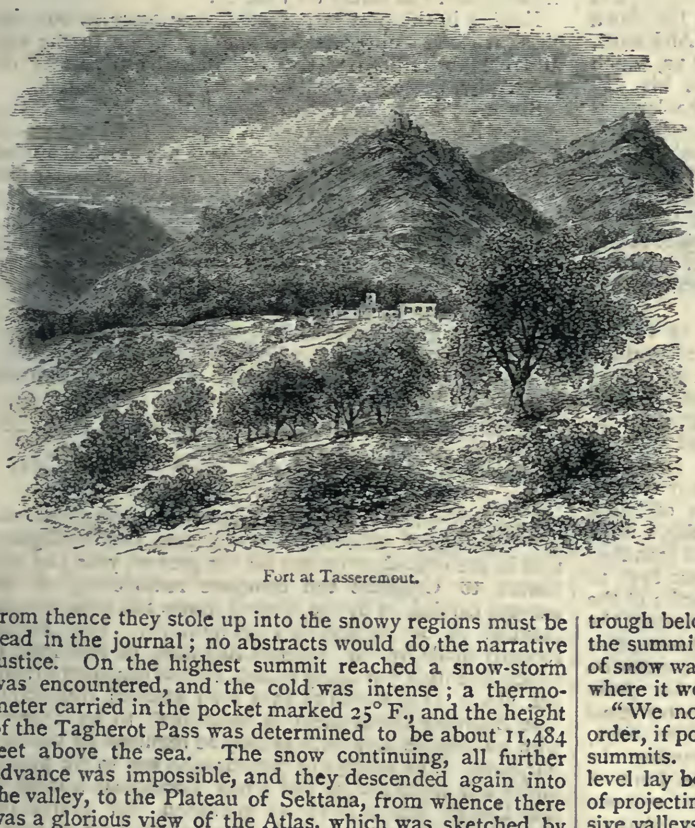
"The date-palm had disappeared soon after we entered the hills; here, and elsewhere on our route, it seems to be confined to the lower region, rarely attaining the level of 3,000 feet above the sea. Its place was here supplied by the palmetto (*Chamærops humilis*), which seldom forms a trunk, perhaps because it is not allowed to attain a sufficient age. As we advanced, the vegetation constantly offered a more varied and attractive aspect; and one of our first prizes was a new species of thyme (*Thymus maroccanus*, Ball), somewhat like the species of the Argan zone, but with oblong leaves and uncoloured bracts. Of comparatively familiar forms there were *Cistus monspeliensis* and *C. polymorphus*, the first species of that genus that we had seen in South Marocco, the pretty little *Cleonia lusitanica*, with many other Labiatæ. Of plants new to our eyes by far the most interesting was the curious *Polygala balansæ*. To those who know only the milkworts of Europe and North America it must seem strange to hear of a large shrubby Polygala, with branches that end in a sharp point, few small leaves, so quickly deciduous that it generally appears quite leafless, and large flowers of a showy purple-red colour. In truth, although there is great variety of form in this large genus, the species which is common throughout the lower valleys of the Great Atlas is very distinct from all its congeners. In Arabia and South Africa there are some species forming dwarf bushes with spinescent branches, but in other respects very different. When full grown this is six or eight feet in height; and the round, green, almost leafless stems give it, when the flowers are absent, much the appearance of *Spartium junceum*, the large broom of Southern Europe.

"After riding some way up a rather steep stony track, we reached a grove of very fine olive trees, and our escort came to a halt. We had reached Tasseremout. For some time we had seen a large pile of solid masonry which crowned the hill immediately above the olive grove. This seemed to deserve a visit; but, on the other hand, the attractions of the surrounding vegetation were irresistible to botanists. The matter was settled by Hooker proceeding to visit the castle with the Kaïd, while Ball botanised, and Maw secured living specimens of some of the more interesting plants."

The castle of Tasseremout is one out of a large number of similar buildings standing on the northern outworks of the Great Atlas chain that will afford interesting matter for inquiry to future travellers when the country becomes more accessible, and the lessened jealousy of the natives will make a thorough examination of them less impossible than it would be at present. The natives vaguely attribute their construction to Christians or Romans, the same word conveying either meaning; but the Jews often explain this to mean Portuguese. The general character of these buildings, as far as our information goes, is tolerably uniform. The walls are of great thickness and built of rough hewn stone: the arches are always rounded, and the lower chambers vaulted; and they are evidently places of defence. There is little reason to believe that the Portuguese, who held at one time or other most of the Atlantic coast of Marocco, ever established a firm footing inland, and still less that they had such a hold on South Marocco as would be implied by the erection of a chain of forts along the foot of the Atlas. On the other hand, the history of Mauritania during the long period of the decline of Rome, and preceding the Saracen conquest, is an almost complete blank, save for a few apocryphal stories.

It is certain that the lower country was once completely subject to Roman power and Roman institutions, and it remains to be ascertained how far an organised government survived the weakening of the central authority. That the independent tribes of the Atlas may have been inconvenient neighbours to the half-Romanised inhabitants of the plain is more than probable, and that the forts should have been erected to hold the former in check seems the most likely conjecture as to their origin. Excavation, whenever that may be practicable, will scarcely fail to tell something of the original occupants of these buildings, and to diminish our ignorance of a dark period of past history.

At sunrise on April 10 (a misprint for May 10) the thermometer stood at 60°, and the travellers were in the best of spirits for undertaking the work that seemed ready cut out for them. They would explore the fine valley that led directly from their camp station to the heart of the great mountain chain, up until they reached the snow; but here comes the exciting portion of the narrative, and the record of how their progress was checked, how they were forced to return, what troubles they had with their escort, how they camped in Ait Mesan Valley, and how from thence they stole up into the snowy regions must be read in the journal; no abstracts would do the narrative justice. On the highest summit reached a snow-storm was encountered, and the cold was intense; a thermometer carried in the pocket marked 25° F., and the height of the Tagherot Pass was determined to be about 11,484 feet above the sea. The snow continuing, all further advance was impossible, and they descended again into the valley, to the Plateau of Sektana, from whence there was a glorious view of the Atlas, which was sketched by Mr. Ball. Amsmiz was reached about May 19; the position of the town reminded them of some of the villages in Piedmont that stand at the openings of some of the interior valleys of the Alps, or still more of similar places on the Apennines of Central and Southern Italy; and from it they reached the poor village of Imintelli, where they sojourned for two days. From Imintelli a desperate and fortunately successful attempt was made to climb to the summit of Djebel Tezah.



Fort at Tasseremout.

"Hooker reached the summit about 2 P.M., and was rejoined by Ball nearly half an hour later. Excepting some light fleecy cumuli floating over the low country to the north, at a lower level than the eye, the sky was cloudless; but in some directions a thin haze obscured the details of the vast panorama. Our first glance was inevitably directed towards the unknown region to the south, and there, at a distance of fifty or sixty miles, rose the range of Anti-Atlas, showing a wavy outline, with rounded summits, and no apparent deep depression, rising, as we estimated, to a height of from 9,000 to 10,000 feet above the sea. The highest portion within our range of view, and the only part with a somewhat rugged outline, bore a few degrees west of due south, and corresponded in position with the Djebel Aoulouse of the French map. A somewhat darker shade traceable at some places on the flanks of this dimly seen range, possibly indicated the existence of forests, or at least of shrubs covering the slopes.

"When the first impulse of curiosity was partially satisfied, we began to take more careful note of our position, and to study in detail a view which had been so long denied to us. The first fact that struck us, was that the peak on which we stood lies a considerable way north of the watershed. The axis of the main chain, which here subsides into undulating masses from 2,000 to 3,000 feet lower than Djebel Tezah, lay between us and the central portion of the Sous valley, and, even if the prevailing haze over the lower districts had not veiled the details, would probably have cut off the course of the stream and the rich tracts that are said to fringe its banks. The higher strata of the atmosphere, above the level of about 7,000 feet, were, however, delightfully clear towards the east and west, and every feature of whatever portion of the main chain lay within our range was easily traced even at distances of thirty or forty miles. An extraordinary change had occurred during the three days since we had viewed the chain from Sektana, covered in deep snow down to the level of about 7,000 feet, and showing only a few crests of precipitous rock here and there protruding. The white mantle had now completely disappeared, and only long streaks of snow filling the depressions of the surface now seamed the flanks of the higher mountains, leaving the summit ridges everywhere bare. During the ascent of the northern face of the mountain, we had kept close to one of these long and comparatively narrow snow-slopes that extended through a vertical zone of over 2,000 feet, with a breadth of some 300 to 400 feet, and we now saw a still longer and wider strip of the same character, filling a shallow trough below us, on the east face of the peak. Near to the summit, and on the ridges leading to it, not a trace of snow was to be seen, even in the crevices of the rocks, where it would find partial shelter from the sun.

"We now proceeded to survey the field of view, in order, if possible, to fix the positions of any conspicuous summits. Looking due west, nothing approaching our level lay between us and the dim horizon. A succession of projecting spurs of the Atlas, dividing as many successive valleys, subsided into the plain; the most prominent, and that extending farthest from the main chain, being the mountain above Seksaoua. Turning the eye a little to the left, about west by south, we saw crowded together many of the higher summits of the western portion of the main range, which was here seen foreshortened, so that it was impossible to judge of their true relative position. The highest of these, seamed with snow, we judged to be about twenty-five miles distant, and higher than Djebel Tezah by 600 or 800 feet. In nearly the same direction, but only about ten miles distant, was a rugged projecting peak, rising some 300 feet above our level, and very many more of somewhat lower elevation were discernible in the space between us and the more distant points. Between

S.W., and S.S.E., the range of Anti-Atlas, rising behind the broad Sous valley, bounded the horizon.

"At our feet, and cutting off from view the course of the river Sous, the mountain mass that here forms the axis of the main chain presented the appearance of a troubled sea of a light ferruginous colour, declining gradually in elevation from W. to E. At a distance of about eight miles E.S.E. of Djebel Tezah it sinks to an estimated height of little over 7,000 feet, at the head of the main branch of the Oued Nfys, and offers the only apparently easy pass over the main chain which we had yet seen.[1] The rocky sunburnt flanks of the mountains were dotted with trees of dark foliage, doubtless some form of the evergreen oak, up to a height of about 8,000 feet above the sea, for the most part solitary, sometimes in clumps, but nowhere forming a continuous forest. The numerous feeders of the Oued Nfys had cut deep ravines in the flanks of the mountains, and were lost to sight, except where gleams of silver light shot upwards from the deeper valleys amid the walnut trees that fringed their banks. Numerous hamlets were seen, some perched upon projecting ridges, some lying in hollows and girdled with a belt of emerald-green crops.

"It was impossible not to speculate on the condition of these primitive mountaineers, who have since the dawn of history preserved their independence. Leo Africanus, speaking of the very district now overlooked by us, which he calls Guzula, says that the people were in his day molested by the predatory Arabs and by 'the lord of Marocco;' but they successfully resisted all encroachments, and no attempt is now made to assert the Sultan's authority among them, or to enforce tribute. Something they have doubtless gained in material, and still more in moral, welfare by stubborn resistance to alien rule; but the prosperity that is sometimes attained by tribes subject to the semi-feudal rule of chiefs, and among whom intestine feuds are rooted in immemorial tradition, is usually short-lived.

"Our hope of getting further knowledge as to the eastern extremity of the Sous valley, and the orographic relations between the Atlas and Anti-Atlas ranges was not to be satisfied. Djebel Tezah, as we found, stands some way north of the axis of the chain, while the great mass that rose over against us between E.N.E. and E.S.E., extending to the head of the Aït Mesan valley, sends out massive buttresses to the south, and by these our view of Anti-Atlas was cut off to the S.E. On one of these western projecting buttresses we could distinguish a large village belonging to the district of Tifinout, and standing at an elevation of nearly 7,000 feet. Turning our eyes to the north of true east, many of the higher summits of the chain were seen rising above the intervening ranges, the most distant probably belonging to the Glaoui group, east of Tasseremout. Once more we came to the conclusion that throughout the portion of the Great Atlas chain visible from the city of Marocco, between the easternmost feeders of the Oued Tensift and those of the Oued Nfys, there are no prominent peaks notably surpassing the average level. Many of them must surpass the limit of 13,000 feet above the sea, but it is not likely that any one attains the level of 13,500 feet. The last object that attracted our attention in the panorama, in a direction about east by north, was an isolated mass, forming a bold promontory on the northern side of the chain, of which a rough outline is here given.

"When the engrossing interest of the distant view had so far subsided as to let us pay attention to nearer objects, we were struck by the unexpected appearance of considerable remains of dwellings on a platform of level ground, only a few feet below the actual summit of the mountain. About a dozen rude stone dwellings, all in a ruinous condition, with chambers sunk a couple of feet below the level of the ground, and the roofs fallen in, had at some former period been here erected; but we saw no traces of recent occupation. It seemed most probable that they were intended as shelter for herdsmen, who had driven their flocks in summer to this lofty station.

"As we lingered on the topmost point of the mountain, the intense silence of the scene was broken by the distant scream of a large grey eagle that soared over our heads, and then sailed away southward over the Sous valley, making the deep stillness still more sensible than before."

No wonder that after the excitement of such a day, which only ended when their quarters were reached at half-past eight at night, the travellers let their collections rest in the collecting boxes and portfolios for the night, and after supper fell themselves to sleep.

Wars among the native tribes at last drove our authors to the necessity of returning to Mogador, passing through the wonderfully singular defile of Aïn Tarsil, which is like a trench some thirty to fifty feet wide and the same deep, running for a length of nearly three miles. Four days were spent on the occasion of the second visit to Mogador, and Sir Joseph Hooker reached London with his collections in safety on June 21.

Having in this notice exceeded the space at our disposal, we can only quite incidentally allude to the very valuable appendices to this volume, which treat of the geography, geology, and flora of the districts visited.

Since we some years ago closed the pages of "Palgrave's Personal Narrative of a Year's Journey through Central and Eastern Arabia," we have not perused a more delightful or instructive book of travels than this account of a tour in Marocco. From the well-known acquirements and great experience of the authors we expected much, and we have not been disappointed. The journal is, without doubt, especially pleasant reading to a botanist, but the geographer will find in it much to interest him, the politician will find in the description of the state of things now existing in such a country material for some serious thought, while the literary taste of every reader will be gratified by the excellent manner in which the narrative is written.

[1] This is apparently the pass spoken of by Leo Africanus as leading from near Imizmizi (Amsmiz?) to the region of Guzula (the northern branch of the Sous valley). He says it is called Burris, that word meaning downy, because snow frequently falls there.—See "Ramusio," vol. i. p. 17, B.

REORGANISATION OF THE AMERICAN SURVEYS

ALL well-wishers of the progress of geographical and geological research will welcome the intelligence that in the official estimates for the present year just presented to Congress the complete remodelling of the surveys carried on by the United States has been recommended to be immediately undertaken. The Report recently made by the National Academy of Sciences, to which attention was lately called in these pages (NATURE, vol. xix. p. 213), seems to have been adopted *simpliciter*. The Engineer Department is henceforth to be charged with no surveys save such as may be required for military purposes. The surveys of mensuration are to be placed under one organisation, and a new Geological Survey of the United States is to be instituted. Of course the changes are at present only recommended for adoption by the Committee on Appropriations, and there may be a struggle over some of the proposals. We hear indeed that the Engineers are leaving no point in their defence unguarded and are preparing for what is called a "heavy fight." For their own sakes as well as for the cause of scientific progress we cannot wish them success.

They object to the constitution of the Academy's Committee on the ground that only one of the members of it knows anything practically of surveying. And this objection will no doubt be urged with force and persistence in the debates in Congress. But surely they can hardly expect to throw dust in the eyes of the legislature by such a flimsy argument. The Committee, as we formerly pointed

out, was a thoroughly competent one. They might as well insist that nobody is competent to pass a judgment on poetry unless he has written an epic, or to criticise fine art if he has not painted a picture, or modelled a statue. The Bureau of Engineers has done such admirable work and deserves such thorough respect, that its best friends can only hope that it will not embitter a fruitless struggle against the inevitable. We have no fear that its scientific *prestige* will in the least be diminished by the projected revolution. The nature of the change will be best understood from the following extracts from the Bill of the Committee on Appropriations, which has been read twice and "committed to a committee of the whole House on the state of the Union and ordered to be printed":—

"For the salary of the Superintendent of the Coast and Interior Survey, 6,000 dollars: *Provided,* That the present coast and geodetic survey, with supervisory and appellate powers over the same authorised by law, is hereby transferred from the Treasury Department to the Department of the Interior, and shall hereafter be known as the Coast and Interior Survey, and shall have charge of all surveys relating to questions of position and mensuration of the coast and interior, except the special survey necessary for geological purposes, the survey of the northern and north-western lakes now under the direction of the War Department, and local surveys required for the improvement of rivers and harbours and surveys necessary for military purposes immediately connected with the operations of the army, in accordance with the plan reported to Congress by the National Academy of Sciences, under the Act of June 20, 1878, entitled 'An Act making Appropriations for Sundry Civil Expenses of the Government for the Fiscal Year ending June 30, 1879, and for other purposes:' *And provided further,* That the offices of surveyors-general are hereby abolished, to take effect on June 30, 1879; and the compensation of said surveyors-general, and all employees under them, shall cease on that day; and the duties pertaining to the offices of surveyors-general shall thereafter be performed by the Superintendent of the Coast and Interior Survey; and the parcelling surveys of the public lands shall hereafter be made by employees of the Coast and Interior Survey: *And provided further,* That the rectangular method with township and sectional units shall be retained wherever it can be appropriately and economically applied, but all surveying by contract shall be prohibited; and the Superintendent of the Coast and Interior Survey is hereby authorised to adopt such additional surveying methods as he may deem most economic and accurate; but the surveys of mineral claims shall be made by deputy surveyors, as now provided by law. And such of the archives and records now in the offices of the surveyors-general as may not be required for the office of the Superintendent of the Coast and Interior Survey shall be turned over to the governors of the several States and Territories, upon the same terms and conditions and in the same manner that the archives have heretofore been delivered to the State authorities in States where the public surveys have been completed and the offices of the surveyors-general closed: *And provided further,* That the Secretary of the Interior shall direct the archives and records of the surveyor-general's office of any State or Territory to be kept in the place where they are now located, if thereby the interests of the people of said State or Territory will be best subserved, such archives and records to be placed under the charge of an employee of the Coast and Interior Survey: *And provided further,* That hereafter surveys of public lands shall, at the discretion of the Secretary of the Interior, be made under the deposit system, on petition of not less than five persons for the survey of a township; the sum of money to be deposited for the survey of the township shall equal the cost of the survey at the present rates allowed for the several classifications of the land to be surveyed, including such sum as shall be estimated for office-work: *Provided,* That the excess of any deposit over and above the aforesaid cost shall be returned to the depositor; and all moneys so deposited and actually required for said survey and office-work, for the amount of land for the survey of which the petition is filed, shall be applicable, either in the hands of the depositor or his assignee, to pay for lands to which the said depositor or others may be entitled under the law. It shall be the duty of the Commissioner of the Land Office to make all needful rules and regulations necessary for carrying into effect the detail of this law, so far as relates to the new conditions established by it in reference to the public lands.

"For the salary of the Director of the Geological Survey, which office is hereby created, who shall be appointed by the President by and with the advice and consent of the Senate, 6,000 dollars: *Provided,* That this officer shall have the direction of the geological survey, and the classification of the public lands and examination of the geological structure, mineral resources, and products of the national domain, in accordance with the plan reported to Congress by the National Academy of Sciences under the act of June 20, 1878, entitled 'An Act making Appropriations for Sundry Civil Expenses of the Government for the Fiscal Year ending June 30, 1879, and for other purposes;' and that the director and members of the geological survey shall have no personal or private interests in the lands or mineral wealth of the region under survey, and shall execute no surveys or examinations for private parties or corporations; and the Geological and Geographical Survey of the Territories, and the Geographical and Geological Survey of the Rocky Mountain Region, under the Department of the Interior, and the Geographical Surveys west of the 100th meridian, under the War Department, are hereby discontinued, to take effect on June 30, 1879; and all collections of rocks, minerals, soils, fossils, and objects of natural history, archæology, and ethnology, made by the Coast and Interior Survey, the Geological Survey, or by any other parties for the Government of the United States, when no longer needed for investigations in progress, shall be deposited in the National Museum.

"That all laws, parts of laws, and all departmental regulations relating or having reference to the coast and geodetic survey now in force and effect are hereby continued in force and effect, and made applicable to the Coast and Interior Survey until changed by competent authority.

"For the expense of a commission on the codification of existing laws relating to the survey and disposition of the public domain, and for other purposes, 20,000 dollars: *Provided,* That the commission shall consist of the Commissioner of the General Land Office, the Superintendent of the Coast and Interior Survey, the Director of the United States Geological Survey, and three civilians, to be appointed by the President, who shall receive a per diem compensation of 10 dollars for each day while actually engaged, and their travelling expenses; and neither the Commissioner of the General Land Office, the Superintendent of the Coast and Interior Survey, nor the Director of the United States Geological Survey, shall receive other compensation for their services upon said commission than their salaries, respectively, except their travelling expenses, while engaged on said duties; and it shall be the duty of this commission to report to Congress within one year from the time of its organisation: first, a codification of the present laws relating to the survey and disposition of the public domain; second, a system and standard of classification of public lands as arable, irrigable, timber, pasturage, swamp, coal, mineral lands, and such other classes as may be deemed proper, having due regard to humidity of climate, supply of water for irrigation, and other physical characteristics;

third, a system of land-parcelling surveys adapted to the economic uses of the several classes of lands; and, fourth, such recommendations as they may deem wise in relation to the best method of disposing of the public lands of the western portion of the United States to actual settlers.

"The publications of the Coast and Interior Survey shall consist of the annual report of operations, such geographic and topographic maps, and geodetic and coast-charts, and such discussions and treatises connected therewith, as the superintendent shall deem of value. The publications of the Geological Survey shall consist of the annual report of operations, geological and economic maps illustrating the resources and classification of the lands, and reports upon general and economic geology and palæontology. The annual report of operations of the Coast and Interior Survey and of the Geological Survey shall accompany the annual report of the Secretary of the Interior. All special memoirs and reports of both surveys shall be issued in uniform quarto series. The style and scale of the cartographic publications shall be determined by the head of each organisation, so as to express the scientific results in the most effective manner. Three thousand copies of each shall be published for scientific exchanges by the heads of the surveys and for sale at the price of publication; and all literary and cartographic materials received by the heads of the surveys in exchange shall be the property of the United States, and form a part of the libraries of the two organisations; and the money resulting from the sale of such publications shall be paid into the Treasury of the United States."

HEINRICH GEISSLER

A YEAR since we were called upon to chronicle the death of Ruhmkorff, whose name is so closely identified with the history of electricity. In recording the death of Dr. Heinrich Geissler at Bonn, January 24, we regret the loss to the world of science of an equally important and esteemed worker. He was born in the village of Igelshieb, in Central Germany, in the year 1814. At an early age he mastered the art of glass-blowing—an industry which has long flourished in his native duchy of Sachsen-Meiningen—and for a number of years he led the life of a German *Handwerksbursch*, rambling from one place to another, accepting employment wherever it was offered. The German University towns offered to him the chief attraction, the preparation of the articles requisite for scientific research having for him a peculiar fascination; and his wandering life finally ended in a permanent settlement at Bonn. Here he developed rapidly. In the treatment of glass before the blowpipe he attained a degree of perfection hitherto unknown, and in his day unsurpassed. Despite the disadvantages of his early life, and the demands of his occupation, he succeeded in making rapid acquisitions in various departments of the natural sciences; and favoured by association with numerous leading celebrities, in physics and chemistry, he soon attained a remarkably comprehensive and intimate familiarity with scientific facts and principles. The union of this knowledge with his constructive ability and manual accomplishments was productive of the happiest results, and the past thirty years have witnessed a constant succession of novel and ingenious devices for the furtherance of scientific discovery, issuing from his *atelier*. Not only was he able to accomplish the practical realisation of the designs submitted to him, but in a multitude of cases, when simply the end in view was proposed to him, Geissler planned and produced apparatus of the most delicate construction, and exact precision, involving a mastery of physical laws to be expected only in one who had devoted his life to the solution of scientific problems. The impulse thus given by him to the march of original investigation is not easy to measure, for his name is rarely associated with the numerous discoveries where his fruitful ideas have contributed in a greater or less degree to the successful result. One of his earliest direct investigations was in companionship with the distinguished physicist, Plücker, in 1852. By means of a delicate apparatus, in which the expansion of the glass was exactly compensated by the introduction of mercury, they made a series of accurate observations on the expansion of water, and established the maximum of density at 3·8°. With an equal degree of accuracy the coefficient of expansion for 1° of ice between $-24°$ and $-7°$ was established at 0·0001585, and the coefficient of expansion for water when freezing at 0°, was ascertained to be 0·09195. In 1869, Geissler, in company with Vogelsang, demonstrated in an ingenious manner the presence of liquid carbonic acid in the cavities of topaz and quartz. The minerals were decomposed by means of a galvanic current, the resultant gases were collected in a vacuum, and the presence of CO_2 was shown by the electric arc. They succeeded, likewise, in producing a precipitation in lime-water, and established beyond doubt the character of the liquid present. Shortly after Geissler succeeded in changing ordinary phosphorus into the amorphous state by the action of the electric current.

The apparatus with which Geissler's name is most popularly associated consists in the famous *tubes* arranged for the exhibition and study of the phenomena accompanying the electric discharge in various gases and vapours. Their ingenious disposition has contributed much to the progress of research on the nature of the electric light and the condition of matter in the gaseous state. Scarcely less important are his inventions of the vaporimeter, the mercury air-pump, as well as the balances, normal thermometer, and normal areometer, and other instruments of precision devised by him, which have rendered such incalculable service to those engaged in exact research. A few years since the University of Bonn rendered a fitting tribute to the varied merits of Geissler by bestowing on him the honorary title of Doctor of Philosophy.

The career of Geissler was in many respects similar to that of Ruhmkorff. Both advanced from the lowest ranks of life to positions of honour in the scientific world, both gave, in a quiet and almost unrecognised manner, an important impulse to the cause of physical investigation, and both have left their names as "household words" in the nomenclature of the science to which they were so faithfully devoted.

T. H. N.

GEOGRAPHICAL NOTES

LIEUT. WEYPRECHT informs us, with reference to his proposed scientific expedition to Novaya Zemlya that the statements which have appeared are very inaccurate, nothing having as yet been decided. He and Count Wilczek certainly intend to go there and make one year's thorough scientific observation in some place on the northern coast; special attention will be given to cosmical physics. They would prefer, however, if in other places of the Arctic and Antarctic regions, others would make observations simultaneous with them. Before the war broke out they had the best hopes of seeing their proposals nearly everywhere accepted, but the disturbed state of Europe during the last two years has prevented them from taking further steps. In April the International Meteorological Congress, which was to have met in 1877, will meet in Rome. One of the questions to be decided there will be, in what manner the Congress can contribute to the realisation of the proposals of Count Wilczek and Lieut. Weyprecht. On the decision come to at that meeting will theirs mainly depend. The programme which it is intended to carry out will be found described in NATURE, vol. xvii. p. 29.

THE commemoration of the centenary of Cook's death by the Paris Geographical Society on Friday seems to have been thoroughly successful. Several addresses were given, showing the services done by Cook to geography, to humanity, to navigation, and to science. M. Huber gave an account of Cook's career, showing how he dispelled the tenacious notion of an Antarctic continent, gave England New Zealand and Australia, discovered a multitude of islands, simplified nautical astronomy, studied oceanic currents, the variations of the compass, and the "Aurora Australis," laid down principles of hygiene, still of value, and opened new horizons to civilisation and religion. He paid a tribute to Cook's care of his crews, his mildness and forbearance towards the natives, his resort to reprisals only when the interests of the expedition or the lives of his men were imperilled, his intrepidity and inventiveness. In connection with Cook's death, M. Huber briefly adverted to the native account of the tragedy, as published four years ago by M. de Varigny, fourteen years a member of the Sandwich Islands Government. It is in the shape of a poem on Captain Cook, attributed to Kupa, an eye-witness of what happened. It describes how two forests were seen gently floating on the waters; how Kupa and others were ordered to swim up to them; how they found Goords, who breathed out fire and smoke from their nostrils and mouths, had dazzling white skins and sparkling eyes, wore skins of various colours, with holes in the sides, into which they plunged their hands, and which appeared full of treasures. A god with his thunderbolt kills Kupa's father, whereupon the other swimmers take to flight. The priest declares that the floating island is the war vessel of the god Lono, who, after murdering his wife through jealousy, left Hawaii long ago to explore the seas, and had now returned, according to his promise, after six generations. He bids the natives take them bananas, cocoa-nuts, and oranges, which are accepted. At night Lono and his fellow-gods shoot hissing arrows of fire at the stars, making some of them fall into the sea. Flames of strange colours descending from the trees of the floating islands and extraordinary sounds alarmed the natives. Next morning Lono lands; is treated as a god, with sacrifices and prostrations; but whether from anger, or from having forgotten the language, he makes no answer. Several of his inferior gods seize on sacred fish destined for the altar. Others begin pulling up the palisades surrounding the sacred inclosure, the Morai, where the elders meet. King Kalaimano remonstrates, but they laugh and persist. Lono comes up, crosses the sacred boundary, and is about to enter the Morai. Kalaimano intercepts his progress, but Lono rudely pushes him away. Kalaimano takes Lono up in his arms, and, on his struggling to free himself, presses him lightly. Lono cries out with pain. "He cries, so he is not a god," exclaims Kalaimano, and kills him. The other gods, who were pulling up the stakes, fly, but the natives fall on them, and, strange to say, their blood flows like that of mortals. Kalaimano, however, while launching arrows from the shore is killed by the invisible fire. Thus your fathers, concludes Kupa, saw the death in one day of their god and their chief. In this song the two visits of the vessels are fused into one. This confirms the story we referred to last week, and the impression that it was not treachery but vexation and disappointment that led to Cook's death. Much surprise, the *Times* correspondent states, was expressed in conversation at the inaction of the London Geographical Society. In Paris not only was a special festival organised with collections, which remained on view till Monday, but the Society has inserted in its *Bulletin* Mr. James Jackson's catalogue of the 300 works published in various languages relating to Cook. Dr. Hamy referred to Cook's observation of the transit of Venus at Tahiti, the rivalry and attacks of Dalrymple, and the fate of Cook's collections in being buried in an Austrian museum. He described Cook as ranking with Columbus and Magellan.

WE understand that the forthcoming number of the *Monthly Record of Geography*, published by the Royal Geographical Society, will contain a full bibliography and cartography of Zulu Land.

THE Russian Geographical Society proposes to give its great gold medal to Prof. Nordenskjöld.—The Berlin Geographical Society has given its gold medal to M. Prjvalsky.

WE hear that Capt. Henry Sengstacke, who had intended to accompany Dr. Otto Finsch in his projected scientific expedition among the islands of the Pacific, is shortly about to proceed to Behrings Straits for the relief of Prof. Nordenskjöld. Capt. Sengstacke took a leading part in recent German Arctic expeditions, and had but lately returned from the west coast of America. At his special request the Council of the Royal Geographical Society have, we understand, undertaken to furnish him with copies of the sailing directions for, and the latest and best charts of, the part of the world which he is now about to visit. The latest information, however, with regard to the relief of Nordenskjöld's party seems to be contained in the following telegram received by the Russian Government from the Governor-General of East Siberia:—"Irkutsk, January 28.—Sibiriakoff telegraphs to me from Zurich that a steamer belonging to Bennett will, immediately after the opening of the navigation, proceed from San Francisco to Behrings Straits to assist Nordenskjöld. It is therefore not necessary to send a steamer from Nicolajefsk."

AT the last meeting of the Berlin Academy of Sciences an account was given of the programme of Dr. Finsch's journey, the cost of which will be defrayed by the Humboldt Fund, and which is estimated at about 13,000 marks (650*l.*). Dr. Finsch will direct his principal attention to Polynesia. He will proceed to Honolulu *viâ* New York and San Francisco; thence he will visit the Marshall and King'smill group, the Caroline, Mary Anne, and Bonin Islands, and he intends to return *viâ* Japan, China, and the Philippine Islands.

MOVING OF HEAVY ORDNANCE

MODELS of the poop and topgallant forecastle decks of H.M.S. *Iris*, and midship main deck of H.M.S. *Dwarf*, are now exhibited by Mr. George Fawcus at the floating dock, North Shields, to explain how naval ordnance can be traversed and trained round elliptical or circular sterns and parabolic bows of vessels, from side to side amidships, or from a point blank or direct broadside, to a fore and aft range of barbette or over all fire "all round" without any changing of pivots; and how muzzle-loading guns can be turned round to load in board, to avoid the inconvenience of loading in front, and thus obtain all the presumed advantages of breech-loading ordnance.

A simple and compact mechanical motion has been developed from the action of the trammel or elliptograph, and is communicated rapidly along the diameters or minor and major axis of an ellipse or oval, as a shorter road than slowly round the circumference, with a small elliptical circuit instead of a circular segment of a larger circle. Two moving pivots replace a single central one. These pivots mutually assist each other to produce a reciprocal compound lever movement, one good graceful turn being succeeded by another, and are kept each in its own track of two intersecting straight lined grooves, which may be adjusted, by various angles of intersection and varied distances of the centres apart from each other, to obtain any imaginable curvilinear movement, so that guns of all kinds can be worked in less space with greater ease of movement, and therefore with less labour and waste of time, than has ever yet been previously effected.

NOTES

MR. PREECE and Mr. Stroh, who have been working for the past twelve months upon the acoustic properties of the phonograph, have completed their labours as far as the vowel sounds are concerned, and their paper on the synthetic examination of these sounds will be read before the Royal Society probably on the 27th inst. Several new instruments of great novelty and marvellous ingenuity will be exhibited, including a new phonautograph, an automatic phonograph, a compound curve-tracer, a new syren, and a new musical instrument.

WE record with deep regret the death, at Luxor, in Egypt, on the 1st inst., of Dr. C. E. Appleton, the founder and editor of *The Academy*. Dr. Appleton was under forty years of age, and had been in declining health for the past two years. His name will be familiar to many of our readers in connection with the Endowment of Research, on which subject he frequently wrote, and a volume of essays on which he edited a year or two ago. Dr. Appleton was himself mainly a student in metaphysics, but he clearly perceived the value of physical science, and the immense advantages likely to accrue to its progress, to our universities, and to the country, by the appropriation of part of the great wealth of the universities, and of the funds of the state, to the encouragement of original research. He laboured earnestly to advance these views, believing that it was the country's duty and interest to encourage the discovery of new truths. He will be greatly missed by his many friends.

WE have to record the death of Mr. Bennet Woodcroft, F.R.S., which happened on the 7th inst. at his residence in Brompton. Mr. Woodcroft will be best remembered in connection with the Patent Office, which he may be said to have originated, and the working of which he so ably and zealously superintended from the time of its establishment down to within the last two years. He was born at Bennet Grange, near Sheffield, in December, 1803, and was consequently in his seventy-seventh year when he died. Early in life he studied science under Dalton, of Manchester, and in course of time joined his father in his business, which was that of a Manchester manufacturer. After a while Mr. Woodcroft came to London, and was appointed Professor of Machinery at University College, London, in 1847; he held that appointment until 1851, when he resigned it. Next year witnessed the passing of the Patent Law Amendment Act, and the then Lord Chancellor, Lord Cranworth, appointed Mr. Woodcroft as superintendent of the specifications, for which post his great experience in patent matters especially qualified him. He retired from office in March, 1876, and during his administration of affairs he carried out the provisions of the Act with efficiency and liberality. The establishment of the library in connection with the Patent Office was mainly due to Mr. Woodcroft, as was also the formation of the Patent Office Museum at South Kensington, to which he was a very liberal contributor, and which was made a free institution solely through his exertions. Among other mechanical improvements effected by Mr. Woodcroft was that of giving to the screw-propeller what is known as an increasing pitch. He was the means of rescuing from oblivion the first marine steam-engine ever made. Mr. Woodcroft was the author of several scientific treatises, and wrote a series of biographical sketches of inventors. He was elected a Fellow of the Royal Society about twenty years since. An excellent notice of Woodcroft appears in the *Engineer* of February 14.

WE regret to announce the death at Berlin on January 15 of Prof. Philipp Spiller, one of the most eminent of German philosophers. Prof. Spiller was born on September 26, 1800, at Einsiedel, near Reichenberg, in Bohemia, and has enriched scientific literature by many valuable publications. His recent work, "Die Urkraft des Weltalls nach ihrem Wesen und Wirken auf allen Naturgebieten" (Berlin: Stuhr, 1876), is a work of the greatest importance and worthy of the attention of all interested in philosophy.

RUSSIA has lost one more of her mathematicians, Prof. Popoff, of Kazan. His works on the integration of differential equations, on hydrodynamics, on the waves which arise from the motion of a body, on definite integrals, on the calculus of variations, &c., have given to the late professor an eminent place among mathematicians.

MR. COWPER'S new "Writing Telegraph" will be brought before the Society of Telegraph Engineers at their next meeting, on the 26th inst., at the Institution of Civil Engineers.

THE Anthropological Institute has just received a legacy of 1,000*l*., bequeathed by the late Mr. Sydney Ellis of Nottingham.

M. CHEVREUL, who although about ninety years of age, enjoying good robust health, has resigned the administration of the Jardin des Plantes. M. Jules Ferry, the new Minister of Public Instruction, has written him a letter eulogistic of his career, and appointing him Honorary Administrator. M. Jules Ferry has appointed to the post, for a term of five years, M. Fremy, the eminent Professor of Chemistry, Director of the Laboratory at the Gardens, the practical School of Chemistry in Paris.

THE people of Penzance have been attempting to celebrate in a mysterious, hole-and-corner way, the centenary of the birth of their great townsman, Sir Humphry Davy, two months after the actual date. What their notion of the "adjacent" world is we do not know, but we doubt if they have any adequate appreciation of the greatness of Davy, whose only merit in their eyes seems to be that he was born in Penzance. Why, if they wanted worthily to honour one of England's greatest scientific worthies, did they not take the Royal and Chemical Societies into their confidence? or how is it that the Royal Society, being aware of the occurrence of this important centenary (they seem to have contributed to the exhibition), have made no efforts to take part in the celebration officially? We leave it to a foreign nation to honour the memory of one of our greatest explorers, and to a petty provincial town to commemorate the birth of one of our greatest chemists. There are surely several screws loose in our scientific organisation.

THE Russian Physical and Chemical Society is now discussing the means of a thorough study of the surface of the moon, especially by means of spectrum analysis.

PROF. FAMINTZIN, of St. Petersburg, has been elected member of the Russian Academy of Sciences in the place of the late Prof. Geleznoff.

A MEETING of the General Committee of the Hanbury Memorial Fund was held in the rooms of the Pharmaceutical Society yesterday. The Sub-Committee reported that the nett proceeds of the one-guinea subscriptions collected from all parts of the world amount, after payment of the cost of the die for the medal, &c., to about 350*l*. The Sub-Committee have to recommend:—1. That the proceeds be invested in consols; the interest to be expended in defraying the cost of a gold medal to be awarded biennially (or otherwise) "for high excellence in the prosecution or promotion of original research in the natural history and chemistry of drugs." 2. That trustees be appointed, who, from time to time, shall request the following gentlemen to award the medal:—The presidents for the time being of the Linnean Society, the Chemical Society, the Pharmaceutical Society, and the British Pharmaceutical Conference, and one pharmaceutical chemist, who shall be nominated by the two presidents last-named.

A Royal Commission, consisting of Mr. Warington W. Smyth, F.R.S., Sir George Elliot, M.P., Mr. F. A. Abel, C.B., Mr. Thomas Burt, M.P., Mr. Robert Bellamy Clifton, F.R.S., Prof. Tyndall, F.R.S., Mr. Lindsay Wood, and Mr. William Thomas Lewis, has been appointed for the purpose of inquiring and reporting whether, with respect to the influence of fluctuations of atmospheric pressure upon the issue of fire-damp from coal, to the adoption and efficient application of trustworthy indicators of the presence of fire-damp, and generally to systematic observation of the air in mines, to improved methods of ventilation and illumination, to the employment of explosive agents in the getting of minerals, and to other particulars relating to mines and mining operations, the resources of science furnish any practicable expedients that are not now in use and are calculated to prevent the occurrence of accidents or limit their disastrous consequences.

Mr. R. McLachlan, F.R.S., writes us that he is informed from two independent sources that Italy has lost its head from dread of the visitation of the *Phylloxera*. The restrictions on the importation of plants of any kind whatever, *and from any quarter*, are most rigid. A consignment of the newly-discovered gigantic Aroid, from Sumatra, received in Genoa, was subjected to formalities and delays in permission to be delivered, of a nature that seriously compromised the welfare of the tubers. In some places gentlemen must dispense with the ordinary floral decorations in their button-holes. On the French frontier no one is allowed to gather a bouquet of wild flowers on foreign soil and take them across the border, for fear that the much-dreaded pest should exist in it. All scientific reasoning seems to be at an end in the minds of the Italian Government officials. But let us not forget that in 1877 we ourselves were almost in the same condition, owing to the panic spread among us with regard to the Colorado beetle. A knowledge of the rudiments of phytological entomology appears to be so universally deficient that it only requires some agitator to raise a panic in order to bring about the most absurd restrictive enactions. No one can blame the Italians for endeavouring by all means in their power to prevent the introduction of the *Phylloxera* into their vineyards; but they might show a little common-sense discrimination. A restriction on the importation of foreign vines would be sensible enough, and they might go further, and prohibit the discharge of earth-ballast taken in by vessels at ports in districts known to be infected. To stop the introduction of *all* vegetables and flowers is quite unnecessary.

On January 4, at 11 P.M., and on the following day at 9 A.M. a strong earthquake was felt at Maikop (Russia); there were five shocks, at intervals of about fifteen minutes.

The installation of objects sent in for the Anthropological Exhibition at Moscow will begin in the end of March. The interesting collections from Samarcand have already arrived, as well as very interesting objects sent by the East Siberian branch of the Russian Geographical Society. Those of stone implements and of quaternary mammals especially draw the attention of the organising committee, as well as several numismatic collections.

We notice a communication made by M. Kontkevitch, at the last meeting of the St. Petersburg Mineralogical Society, on the recently explored iron mines in the provinces of Kherson, Ekaterinoslav, and Taurida. At the confluence of the Saksagon and Ingulda rivers there are no less than forty layers of iron from 35 to 200 feet thick and several miles wide, containing 58 to 70 per cent. of iron, and representing a store of two and a half milliard cwts. of iron.

The Aosta section of the Italian Alpine Club proposes to celebrate this year the centenary of Saussure's travels in the Alps, which opened up quite a new world for science and for travellers. In 1779 he stayed for the first time in the Valley of Annecy, and the Club proposes to put a commemorative marble plate on the house he inhabited in the village of Dolonne, near Courmayeur. An inscription will probably be placed also on the Grammont Mountain, whence Saussure made his famous observations on Mont Blanc, the first ascent of which he made in 1787.

The Indian Government *Gazette*, we learn from the *Times of India*, contains papers on the proposed Presidency Botanic Gardens, including a Government minute and the report of the Committee. The Committee's consideration was invited to the question whether Puna or Bombay should be chosen as the place for the principal botanic garden of the Presidency. They decided in favour of Ganesh Khind. They recommend, however, that a small branch garden, consisting of four or five acres, be established in Bombay, and that the Grant College compound be selected for the purpose. The Government highly approved of all the recommendations, which will be carried out whenever financial means may permit. The main scientific garden, which will embrace about forty acres, is to be laid out in the irregular picturesque style, with special reference to landscape effect, and the planting of the ground will be done gradually and without any undue haste. It may be mentioned here that the chief resources of the garden are to be devoted to the bringing together of the indigenous plants of Western India, and until this is satisfactorily accomplished no pains will be taken, except in special cases, to introduce foreign plants. An extraordinary expenditure of Rs. 22,037 will have to be incurred for the purpose of constructing roads and footpaths, excavating a ground, erecting houses and sheds, providing iron piping, &c., for water supply, fitting up rooms for the herbarium, library, and class-room, and for the purchase of botanical books and diagrams. The estimated annual expenditure is, in round numbers, Rs. 12,000.

At the last meeting of the French Geographical Society a letter was read from the Abbé Desgodins, dated Yerkalo, August 27, 1878, in which he states that, contrary to the common assertion which represents the sheep as the beast of burden most used in Thibet, this function belongs in preference to the yak (*Bos grussiens*); the mule, ass, and horse are also made use of. The sheep, he says, is only employed as a beast of burden at one period, viz., when the parties of Thibetans quit the high plateaux to descend into the valleys at the approach of winter. The Buddhist pilgrims are sometimes to be met with sheep and goats carrying their baggage, but, as the Abbé Desgodins remarks, there is a wide difference between that and representing the sheep as the beast of burden of Thibet.

The first fascicule of the sixth volume of the "Repertorium für Meteorologie," published by the Russian Central Physical Observatory, contains a memoir, by Prof. Wild, on the temperature of the soil at St. Petersburg and Nukus (Amu-darya); geographical, magnetic, and hypsometric observations, by M. Fritsche, made during his journeys from St. Petersburg to Peking in 1866 and 1877; photochemical measurements of the intensity of daylight in St. Petersburg, by M. Stelling; determinations of the coefficients of anemometers, and magnetic observations on the Amu-darya, by the late M. Dorandt; and researches, by M. Frölich, into the temperature of space.

The German *St. Petersburger Zeitung* states that the cost of the bronze monument to be erected at Dorpat, in memory of Carl Ernst von Baer, is estimated at 15,000 roubles (about 2,300*l*.), and solicits subscriptions towards this sum.

The use of a paper dome for an astronomical observatory is a novelty in modern architecture, although, according to Prof. Greene, of Troy, U.S., under whose supervision this has been constructed, it promises to answer a satisfactory purpose. The

dome is a hemisphere with an outside diameter of twenty-nine feet. The framework is of pine properly seasoned, and the covering is of paper, such as is used by Messrs. E. Waters and Sons for the construction of paper boats. The entire weight of the dome and appurtenances, as completed, is about 4,000 pounds. It can be easily revolved by a moderate pressure without the aid of machinery.

THE director of the Postal Telegraph Service of the French Republic has been made a member of the Cabinet and placed on the same footing as the Postmaster-General of the British Government. The present holder of that office is M. Cochery.

THE Royal Institute of Sciences at Venice offers three prizes of 3,000 lire each (about 115*l.*) for three monographs containing (1) an account of the advantages which the application of physics to medical science has brought about; (2) a summary of the most recent investigations made in the field of theoretical hydrodynamics, as well as a statement of the true and essential progress made by this branch of scientific mechanics; (3) a treatise on the commercial and industrial conditions of the city of Venice. Further particulars may be learnt by applying directly to the "Istituto Reale Veneto" at Venice.

THE petrified remains of a Dinotherium belonging to the miocene period have just been discovered at Schöneg, near Salmhausen (Swabia), at a depth of 13 metres in a sand-hill.

WE recently referred to the all-embracing scientific agency of Friedländer and Son of Berlin, and this week we have received the first three parts of a new fortnightly publication from that house, likely to be of the greatest service to students in all departments of science. It is entitled *Naturæ Novitates*, and is a fortnightly bibliographical list of current literature of all nations, methodically arranged, in the various departments of science. The publication deserves encouragement; it may be had through Messrs. Williams and Norgate.

THE first part has reached us of an important German undertaking, an Encyclopædia of the Natural Sciences, constructed somewhat after the method of the old "Encyclopædia Metropolitana." It is to consist of methodical treatises in the various departments of science, followed by an index, which will give it all the advantages of an alphabetical cyclopædia. Each department has a separate editor, and some of the best men in Germany are engaged upon it. The first part is an instalment of a "Handbuch der Botanik," edited by Prof. A. Schenk, and contains a treatise on "Fertilisation of Flowers," by our friend Dr. H. Müller, and another on "Insectivorous Plants," by Dr. O. Drude. Trewendt of Breslau is the publisher.

THE Rev. W. A. Leighton has nearly completed the printing of the *third* edition of his "Lichen-Flora of Great Britain, Ireland, and the Channel Islands," which, it is expected, will be ready for issue early in March. This new edition is rendered necessary by the recent important discoveries in the west of Ireland, the north of Scotland, and the author's own researches in North and South Wales, whereby the number of our lichens, in the former editions amounting to 1,156, has been raised to 1,706, thus rendering our lichen-flora quite equal in number, rarity, and novelty, to that of any country in Europe.

AMONG recent deaths is that of M. Chauffod, Professor of Zoology at the Paris School of Medicine. M. Chaufford during his whole career opposed M. Claude Bernard's determinism, and advocated the existence of a vital principle and final causes in a number of books largely circulated.

FOR some time past the well-founded fear of trichina has led to a microscopic examination of much of the meat, especially pork, sold in Berlin. Recently the occurrence of this pest there has been more frequent, and Dr. Luedtge (who claims the invention of the microphone) has consented to give a course of instruction in this branch of microscopy, which began February 17. The course, with practical exercises, will occupy five hours, and is open to ladies and gentlemen at the price of 5*s*. The instruction is to be given in the old Mint, at the Microscopic Aquarium, of which Dr. Luedtge is the director.

DR. AUB, one of the oldest Rabbis in Berlin, recently received from the University of Munich a new doctor's diploma, commemorative of his having received that degree there fifty years ago. It was conferred by Dr. Steinthal in the name of the philosophical faculty.

IT is stated in the *Diario de Manila* that a mine of amianthus, or earth flax, has been discovered in the Island of Luzon. Several specimens of the mineral have been taken to Manila, and have been pronounced by competent judges to be of excellent quality.

IN December last a convention between Spain and China was signed in Spanish, French, and Chinese at Peking relating to the treaty which regulates the emigration of Chinese to the Island of Cuba.

A CORRESPONDENT to the *Times of India*, who lately rode through the Kohat Pass, gives a somewhat curious description of an Afridee village, or, rather, an Afridee family home. The first thing seen is a mud wall oddly slit and pierced, and over its summit rise a number of mud and generally round-shaped projections, on the tops of which may be seen a few children and women. These projections are the roofs of the little rooms or mud inclosures in which the family live during the day; but what immediately strikes the attention on approaching is the loopholed mud tower overshadowing the mud inclosure. The house proper is reached by passing through a very narrow entrance between the family fort and the mud inclosure. Inside are winding lanes between high mud walls, loop-holed at every turn. The writer found the inhabitants of the village he visited exceedingly hospitable. "The men around him," he says, "had a curiously frank, inquiring, and manly look. Nothing in their demeanour as they stood examining me and watching me eat, could have embarrassed the most sensitive stranger; but as I attentively watched some of their countenances, I could not help observing how often their expression changed, and how often there flitted across their faces a look that made one insensibly shudder." It is worth noting that the women of the Afridees, although Mohammedans, do not cover their faces.

WE understand that Mr. J. R. Gregory, the well-known mineral dealer, has several specimens of that extremely rare mineral, *Percylite*, of which the only known specimen, till quite recently, was the example in the British Museum; he also has, we hear, specimens of another rare mineral, named *Schwartzenbergite*, both from the same locality—a new one for these minerals—in Bolivia.

THE additions to the Zoological Society's Gardens during the past week include a Ring-tailed Lemur (*Lemur catta*) from Madagascar, presented by Mr. Thos. G. Mann; a Cape Hyrax (*Hyrax capensis*) from South Africa, presented by Mr. A. H. Jamrach; two Black-headed Gulls (*Larus ridibundus*), a Common Gull (*Larus canus*), European, presented by Mr. Harry W. Preston; a Wood Owl (*Syrnium aluco*), European, presented by Mrs. George Blagden; a Garnett's Galago (*Galago garnetti*) from South-East Africa, two Yellow-billed Sheathbills (*Chionis alba*) from Antarctic America, purchased; a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*), born in the Gardens.

SIR JOHN LUBBOCK ON ANTS

SIR JOHN LUBBOCK read two papers on ants at the Linnean Society on February 6. The first gave an account of their anatomy; but from the extreme complexity of these interesting little creatures, it would be impossible to make this communication intelligible without the figures. The second paper was a continuation of his observations on the habits of ants. He mentioned that he had at first isolated his nests by means of water. This was effectual enough, but, especially in summer, the water required to be continually renewed. Kerner, however, had suggested that the hairs of plants served to prevent ants from obtaining access to the honey, and it accordingly occurred to him that strips of fur arranged with the points of the hairs downwards might answer his purpose. He had tried this, and finding it successful, he thought a similar arrangement might perhaps be found useful in hot countries.

It is generally stated that the queen ants alone lay eggs, but Sir John has found that in most of his nests some few of the workers are capable of doing so. It appears, however, that these eggs always produce males. In the case of bees we know that the queen is fed on a special kind of food. In ants it is not feasible to make observations similar to those by which in bees this has been established. It is, however, rendered more than probable by the fact that while males and workers have been bred by hundreds in his nests, no queen has yet been produced.

It is well known that ants keep other species of insects in their nests, which they use just as we do cows, &c.

The *Mat. p. l'Hist. prim. de l'Homme* for 1869 contains a short but interesting account by M. Lespes of some experiments made by him on the relations existing between ants and their domestic animals, from which it might be inferred that even within the limits of a single species some communities are more advanced than others. He found that specimens of the blind beetle, *Claviger duvalii*, which always occurs with ants, when transferred from a nest of *Lasius niger* to another which kept none of these domestic beetles, were invariably attacked and eaten. From this he infers that the intelligence necessary to keep *Clavigers* is not coextensive with the species, but belongs only to certain communities and races, which, so to say, are more advanced in civilisation than the rest of the species.

Sir John Lubbock, however, removed specimens of the curious blind *Platyarthrus* from one nest to another, but they were always amicably received. He even transferred specimens from a nest of *Lasius flavus* to one of *Formica fusca*, with the same result.

As regards the longevity of ants he has now two queens of *F. fusca*, which seem quite in good health and which have lived with him since 1874; they are therefore probably five years old. He has also workers of *Lasius niger*, *Formica sanguinea*, *F. fusca*, and *F. cinerea*, which he has had under observation since 1875.

In his previous papers he had given various instances which seem to show that ants do not exhibit such unvarying kindness to their friends as has been usually supposed. He wished to guard himself, however, against being supposed to question the general good qualities of his favourites. In fact, ants of the same nest never quarrel among themselves; he had never seen any evidence of ill-temper in any of his nests. All is harmony. He had already in previous papers given various instances of tender kindness. Again, in one of his nests of *Formica fusca*, was a poor ant which had come into the world without antennæ. Never having previously met with such a case, he watched her with great interest, but she never appeared to leave the nest. At length one day he found her wandering about in an aimless sort of manner, and apparently not knowing her way at all. After a while she fell in with some specimens of *Lasius flavus*, who directly attacked her. He then set himself to separate them; but she was evidently much wounded, and lay helplessly on the ground. After some time another *Formica fusca* from her nest came by. She examined the poor sufferer carefully, then picked her up tenderly, and carried her away into the nest. It would have been difficult, Sir John thinks, for any one who witnessed this scene to have denied to this ant the possession of humane feelings.

It is clear from the experiments recorded in the present and in Sir John's former papers, that the ants recognise all their fellows in the same nest, but it is very difficult to understand how this can be effected. The nests vary very much in size, but in some species 100,000 individuals may probably be by no means an unusual number, and in some instances even this is largely exceeded. Now it seems almost incredible that in such cases every ant knows every other one by sight; neither does it seem possible that all the ants in each nest should be characterised from those of other nests by any peculiarity. It has been suggested in the case of bees that each nest might have some sign or password. The whole subject is full of difficulty. It occurred to Sir John, however, that experiments with pupæ might throw some light on the subject. Although the ants of every separate nest, say of *Formica fusca*, are deadly enemies, still if larvæ or pupæ from one nest are transferred to another, they are kindly received and tended with, apparently, as much care as if they really belonged to the nest. In ant warfare, though sex is no protection, the young are spared—at least when they belong to the same species.

Moreover, though the habits and dispositions of ants are greatly changed if they are taken away from their nest and kept in solitary confinement, or only with a few friends, still under such circumstances they will often carefully tend any young which may be confided to them. Now if the recognition were effected by means of some signal, or password, then, as it can hardly be supposed that the larvæ or pupæ would be sufficiently intelligent to appreciate, still less to remember it, the pupæ which were intrusted to ants from another nest would have the password, if any, of that nest, and not of the one from which they had been taken. Hence, if the recognition were effected by some password, or sign with the antennæ, they would be amicably received in the nest from which their nurses had been taken, but not in their own.

He therefore took a number of pupæ out of some of his nests of *Formica fusca* and *Lasius niger*, and put them in small glasses, some with ants from their own nest, some with ants of another nest of the same species. The results were that thirty-two ants belonging to *Formica fusca* and *Lasius niger*, removed from their nest as pupæ, attended by friends and restored to their own nest, were all amicably received. What is still more remarkable, of twenty-two ants belonging to *Formica fusca*, removed as pupæ, attended by strangers and returned to their own nest, twenty were amicably received. As regards one Sir John was doubtful; the last was crippled in coming out of the pupæ case, and to this, perhaps, her unfriendly reception may have been due. Of the same number of *Lasius niger* developed in the same manner from pupæ tended by strangers belonging to the same species, and then returned into their own nest, seventeen were amicably received, three were attacked, and of about two Sir John felt doubtful.

On the other hand, fifteen specimens belonging to the same species, removed as pupæ, tended by strangers belonging to the same species and then put into the strangers' nest, were all attacked.

The results may be tabulated as follows:—

	Pupæ brought up by friends and replaced in their own nest.	Pupæ brought up by strangers. Put back in own nest.	Pupæ brought up by strangers. Put in strangers' nest.
Attacked	0	7[1]	15
Received amicably	33	37	0

Sir John intends to make further experiments in this direction, but the above results seem very interesting. They appear to indicate that ants of the same nest do not recognise one another by any password. On the other hand, if ants are removed from the nest in the pupæ state, tended by strangers, and then restored, some at least of their relatives are certainly puzzled, and in many cases doubt their claim to consanguinity. Strangers, under the same circumstances, would be immediately attacked; these ants, on the contrary, were in every case—sometimes, however, after examination—amicably received by the majority of the colony, and it was often several hours before they came across one who did not recognise them.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Medical Faculty of the University of Zurich gave, last week, the degree of M.D. to Miss Draya Sjocie, from Shabats, in Servia, and the Countess Vilma Hugonai, from Teteny, Hungary.

THE success of the high classes for ladies at Odessa has exceeded all expectations. On the opening day, January 21, instead of the expected sixty or seventy students, 215 ladies were inscribed. The University has offered its rooms for the classes.

[1] Of about three of these Sir John did not feel sure.

We learn from the Annual Report of the Moscow University that the number of students at the University was, during 1878, 1,643, with 108 professors; 318 of them were in the Jurisprudence Faculty, 131 in the Philological, 240 in the Physico-Mathematical, and 954 studied Medicine. No less than 62 medical students have taken part in the last war; the majority of students are very poor, and 417 of them received pecuniary help which has reached, during the year, the sum of 11,500*l*.

SCIENTIFIC SERIALS

American Journal of Science and Arts, January, 1879.—Prof. Loomis's important paper in this number on storms on the Atlantic, &c., has been noticed elsewhere. Prof. Marsh (in an appendix) describes a new order of extinct reptiles (*Sauranodonta*) from the Jurassic formation of the Rocky Mountains; they closely resemble Ichthyosaurus (of which no remains have hitherto been found in America), but are without teeth. The same author continues his "Principal Characters of American Jurassic Dinosaurs."—Prof. Greene, of Troy, New York, describes a paper dome constructed from his plans for an astronomical observatory. The paper covering is in sixteen equal sections, the framework of each section consisting of three ribs of pine meeting at the apex. There are also a circular sill at the base and two parallel semicircular arch girders spanning the dome (all of pine). The entire structure weighs about 4,000 lbs. The dome is supported on six 8-inch balls rolling between grooved iron tracks by direct pressure.—Mr. Edison describes his tasimeter as applied to measuring the heat of the stars and of the sun's corona.—Mr. Fontaine writes on the mesozoic strata of Virginia, and Mr. Holden on the brightness and stellar magnitude of the third Saturnian satellite.—A list of fifty species of east coast fishes (many of them new to the fauna) is supplied by Messrs. Goode and Blan.—In the "Miscellaneous Intelligence" will be found the report of the committee appointed to consider the scientific surveys of the United States territories.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 23.—"On the Microrheometer." By J. B. Hannay, F.R.S.E., F.C.S., lately Assistant Lecturer on Chemistry in the Owens College, Manchester. Communicated by H. E. Roscoe, LL.D., F.R.S., Professor of Chemistry in Owens College, Manchester.

In this paper the author reviews the work done by chemists and physicists in determining the relation between the chemical composition of a liquid and its rate of flow through a capillary tube. Poiseiulle [1] ascertained, in a very accurate manner, all the physical laws relating to the rate of flow, as regulated by temperature, pressure, and dimensions of the tube; but on examining saline solutions he could make nothing of the numbers presented, because he used percentage solutions instead of solutions proportional to the equivalent of the body dissolved. Graham,[2] noticing that Poiseiulle had discovered a hydrate of alcohol by running various mixtures of alcohol and water through the tube, examined mixtures of the various acids with water, and found that the hydration proceeded by distinct steps of multiple proportions. Several others, notably Guerout,[3] have since worked on the same subject, but as they have only worked on organic liquids, and have done all the rates at the same temperature, the results throw no light on the phenomena. Thus water runs about five times as quickly at 100° as at 0°; and in a series of alcohols, such as Guerout experimented upon, the differences between their boiling points were very great, so that, their vapour tensions or molecular mobilities being quite incomparable while at the same temperature, the experiments do not admit of any real interpretation. The author reserves the organic part of the investigation, which requires the determination of vapour tensions, till a future paper, and in the present deals with saline solutions.

The phenomenon of the flow of liquids through capillary tubes has been called in this country transpiration, while in other countries no distinct name has been adopted; and as the English word is already in use in French for another purpose, and properly applies to gases (the laws relating to which are quite different), the author proposes to use for liquids the term "Microrheosis," from μικρός and ῥέω, the instrument being called the microrheometer. The form of apparatus which the author finally adopted is figured in the paper, and is so arranged that when the liquid is introduced, as many experiments as may be desired may be tried, and the pressure and temperature, as well as the atmosphere in which the experiment is conducted, may be varied, while the thermometer indicating the temperature is at the mean point of the system. The author gives a curve for water from 0° to 100°, the differences of rate being smaller as the temperature rises.

[1] *Ann. de Chim. et de Physique*, [3], t. vii. 50.
[2] *Phil Trans.*, 1861, p. 373.
[3] *Comptes Rendus*, lxxix. p. 1201; lxxxi, p. 1025.

Various salts are then examined, being dissolved to form "normal" solutions; but as the solubility of some salts is too low for such solutions, the effect of the amount of salts dissolved is determined. This is found to be directly proportional to the amount of salt in solution. Values for many salts in solution are then given, each number being the mean of ten experiments, and the probable error of the mean is calculated in each case. The conclusions arrived at are these. The rate of flow does not depend on any of the "mechanical" features of the salt, such as crystalline form, specific volume, solubility, &c.; but upon the mass of the elements forming the substance and the amount of energy expended in its formation. Each element has a value of its own, which is continued in all its compounds. Thus all the salts of potassium and sodium formed by the same acids have a constant difference. In like manner each metalloid and acid radicle has a value which is continued in all its combinations. Then the greater the combining value of an element the quicker is its microrheosis; thus potassium has a higher rate than sodium, barium than strontium, strontium than calcium, and so on. The microrheosis also varies with the amount of energy in the compound; thus nitrates stand highest, as they contain most energy; then chlorides; and, lastly, sulphates, which are exhausted compounds.

The instrument, bringing to light as it does the fundamental relations of combining weight and energy in chemical action, will be of the utmost importance in chemical physics, as by its use not only will the amount of energy evolved in reactions be determined, but the mass combined; or, in other words, the chemical equivalent of the elements involved will be found.

February 6.—"On certain Dimensional Properties of Matter in the Gaseous State." By Osborne Reynolds, F.R.S., Professor of Engineering at Owens College.

Mathematical Society, February 13.—C. W. Merrifield, F.R.S., president, in the chair.—Sir J. Cockle, F.R.S., was admitted into the Society.—Mr. R. Hargreaves and Prof. W. E. Story were proposed for election.—Dr. Hirst, F.R.S., communicated a paper by M. Halphen on the number of conics which satisfy five independent conditions.—Sir J. Cockle spoke upon a construction for making magic squares. Messrs. Cayley, Harley, Henrici, Roberts, Hart, and other gentlemen took part in a discussion on the subject. Prof. Henrici, F.R.S., gave some properties of frames.—Prof. H. J. S. Smith, F.R.S., read two papers on a modular equation and on the formula for four Abelian functions.—Mr. J. J. Walker communicated a quaternion proof of Minding's theorem.

Linnean Society, February 6.—Prof. Allman, F.R.S., president, in the chair.—Mr. J. R. Jackson exhibited specimens from the tombs of ancient Thebes. Among these were fruits of the Doum Palm (*Hyphæne thebaica*) and of *H. aigun*, formerly, but wrongly, described as an *Areca*. Small berries also obtained were identified as those of *Juniperus phœnicea* as against those of *J. excelsa*.—Mr. J. G. Baker showed dried bulbs of *Buphane toxicarea*, which furnish a principal ingredient of the poison the Bushmen of South Africa tip their arrows with. Structurally, the numerous tunics of the bulb are a peculiarity. The range of this plant has been found to be as far north as Lake Tanganyika. In Sir C. W. Strickland's hothouse a plant flowered last year, and this for the first time in England.—Mr. W. T. Thiselton Dyer shortly described specimens of, and pointed out the special characters and probable advantages of, a new fodder grass, *Euchlæna luxurians*, and he also exhibited and made remarks on curious instruments used for weaving fibre of *Curculigo latifolia* by the natives of Borneo.—Mr. T. Christy drew attention to a sample of tea grown in Natal, and to a bottle of the milky secretion of the African Rubber Tree (*Landolphia*), the same having been freshly drawn from the living plant and immediately thereafter forwarded to this country; slight coagulation of the juice had nevertheless occurred.—The Rev. G. Henslow passed round for examination a specimen of female

mistletoe bearing male shoots. The botanists present expressed opinion of its being an androgynous condition rather than a male parasitic on a female plant, as had been supposed.—Mr. R. Irwin Lynch exhibited and made remarks on parts of the Bull's Thorn Acacia (*A. sphærocephala*), the Imbauba Tree (*Cecropia peltata*), and on a couple of Orchids (viz., *Espidendron bicornutum*, and *Schomburghii tibicinus*), as exemplifying their economy in affording protection to, and food for, ants. Mr. F. Darwin has already described the two former (*L. S. J. Bot.*, xv. p. 398), but that orchids should furnish a nidus for ants is apparently a new fact.—A short paper on the position of the genus *Sequenzia* among the Gasteropoda was read by Dr. J. Gwyn Jeffreys. His opinion differs from that lately promulgated by the Rev. R. B. Watson, believing that it belongs to the Solarium group rather than to the Trochus family, where placed by the latter naturalist. —There followed two papers on the anatomy and on the habits of ants, the gist of which we give elsewhere.

Physical Society, February 8.—Annual meeting.—The President (Prof. W. G. Adams) read the Report of the Council, which showed that the papers had been more numerous during the past than in any previous year, and that their value and interest had been well sustained.—A copy of the collected papers of the late Sir Charles Wheatstone was laid on the table, and the work will shortly be issued to the members of the Society.—The President then gave a brief review of the physical work of the past year, dwelling more especially on the papers read at the meetings.—Votes of thanks were then passed to the president, to the Lords of the Committee of Council on Education, to the demonstrator, treasurer, secretaries, and auditors, and the following were elected as Council and Officers for the ensuing year:—President—Prof. W. G. Adams. Vice-Presidents—Prof. G. C. Foster, Prof. R. B. Clifton, Lord Rayleigh, Dr. Spottiswoode, Sir Wm. Thomson. Secretaries—Prof. A. W. Reinold, Mr. W. Chandler Roberts. Treasurer—Dr. E. Atkinson. Demonstrator—Prof. F. Guthrie. Other Members ot Council—Capt. W. de W. Abney, Dr. Warren de la Rue, Major E. R. Festing, Prof. Fuller, Dr. Huggins, Prof. A. B. W. Kennedy, Prof. McLeod, The Earl of Rosse, Mr. G. Johnstone Stoney, Dr. Wormell. Honorary Members—Prof. G. R. Kirchhoff, Dr. J. Plateau.—The meeting was then resolved into an ordinary one, and Dr. O. J. Lodge read a short paper on a method of calculating the course of temperature in a rod along which heat is being conducted.—Mr. Shoolbred gave an account of electric lighting illustrated by diagrams of the most recent magneto- and dynamo-electric machines and examples of the lamps in vogue. The only surviving magneto-machine is that of De Meritens, which is incomparably superior to the older ones of Nollet and Holmes. The dynamo-electric machines described were the continuous-current machines of Siemens, Gramme, Wallace-Farmer, and the alternating current machines of Wilde, Gramme, and Lontin. Wilde's machine is the first of these, or parent machine, and Lontin's so resembles it that the latter cannot be used in England. In these machines the current from a continuous machine is passed through a second machine, which yields the alternating currents. In Lontin's machine also a number of distinct currents are generated in separate circuits, each of which is capable of feeding several lights. There is now one in use on the Western Railway of France which gives three distinct currents, each of which supplies four different lamps, making a total of twelve lights. The American Brush machine was also mentioned. The Dubosq lamp, which was the first regulator, is well adapted for laboratory purposes, but for practical purposes the Serrin is preferable. Rapieff's lamp is used in the *Times* office. The De Mersanne, which was highly spoken of at the Paris Exhibition, moves the carbons by bevelled gearing. The Wallace-Farmer lamp, though durable, is unsteady, perhaps because only inferior gas carbon has yet been used. Jablochkoff's candle was found to be defective from the solid insulator, such as plaster, used between the carbons. This made it very expensive also. Experiments in Paris had shown that whereas Jablochkoff's system cost 10*d*. per hour per light, the other systems only cost one half of that. In Wilde's candle the solid insulator was dispensed with, air taking its place, the arc always tending to keep at the tip of the candle by electro-dynamic repulsion. In the De Meritens' candle three strips of carbon were used, the intermediate one being a stepping-stone to the arc, which passes between the two outer ones. Werdermann's and Regnier's so-called incandescent lamps were also shown. Mr. Shoolbred, after alluding to the fact that the upper positive carbon takes a crater form, and hence becomes a reflector, shedding the light downwards, stated that experiments had proved the line of maximum intensity of light to pass downward at an angle of 60° to the axis of the vertical carbons. By giving the positive carbon a horizontal displacement behind the lower negative one, Mr. Douglass, of the Trinity House, had been able to raise this line till it became horizontal, an advantage in lighthouses. He also pointed out that whereas in Paris the Jablochkoff waxed for a period, short compared to that in which it waned, in London it waxed for longer than it waned, which was, of course, an improvement, and Mr. Shoolbred suggested that it might be due to the fact that the engine worked at speed nearer to that of the machine, and that the machine was founded more solidly in London than in Paris. Mr. Werdermann said that it was a mistake to call his lamp an incandescent one, the fact being that all carbon lamps gave light from the incandescence of the positive carbon, and that a small arc was formed in his lamp between the two electrodes, which could be varied by the pressure between them. He maintained that it was as easy to produce 500 lights as 10 from the electric current by subdivision, as he hoped soon to show; and stated that the size of the carbons greatly controlled the intensity of the light. Prof. Ayrton held that the obstacle to the subdivision of the electric light was not an electrical one, but was due to the fact that the amount of light produced by the current is not in direct proportion to the amount of the heat produced. In contradiction to Prof. Ayrton, Mr. Werdermann stated that in the electric arc the opposing electromotive force was proportional to the original electro-motive force. Prof. Silvanus P. Thomson pointed out that residual magnetism in the cores of the bobbins of dynamo-electric machines lowered their efficiency, and hence short cores, as in the Wallace-Farmer machine, were an improvement.

EDINBURGH

Royal Society, January 20.—Prof. Kelland, president, in the chair.—Prof. Crum Brown gave the third part of a paper by him and J. Adrian Blaikie, B.Sc., on the action of heat on salts of tri-methyl-sulphine. They find that—I. the aqueous solution of tri-methyl-sulphine does not yield crystals when evaporated over sulphuric acid in vacuo, but only a thick syrup was obtained. On heating this at 100° it decomposes—water acetate of methyl and sulphide of methyl being given off.

$$CH_3{-}COO{-}S(CH_3)_3 = CH_3{-}COO{-}CH_3 + (CH_3)_2S.$$

II. The aqueous solution of benzoate of tri-methyl-sulphine cystallises in small thin plates which it is difficult to separate from the thick mother-liquor. On heating, the imperfectly dried salt yields at 110° water, sulphide of methyl and benzoate of methyl; the later boiling at 198°.

$$C_6H_5{-}COO{-}S(CH_3)_3 = C_6H_5{-}COO{-}CH_3 + S(CH_3)_2.$$

III. The dithionate of tri-methyl-sulphine is obtained by neutralising free aqueous dithionic acid with the hydrate. It crystallises readily in small clear cubes with one molecule of water of crystallisation. It is not hygryscopic. On heating at 220° sulphurous acid begins to come off, and afterwards along with it sulphide ot methyl, leaving methyl-sulphate of tri-methyl-sulphine.

$$\{(CH_3)_3S\}_2S_2O_6{-}H_2O = \{(CH_3)_3S\}CH_3SO_4 + SO_2 + S(CH_3)_2 + H_2O.$$

—Prof. Tait then gave the result of some experiments he had been making to determine the electro-motive force of the Gramme magneto-electric machine at different speeds. The experiments were not completed but they seemed to show that the electromotive force varied approximately in the duplicate ratio of the rate of turning. He explained that the Gramme machine with an electromotive power of about 37 Bunsen cells could give as powerful an electric light as a battery of 60 Bunsen cells, for the resistance in the Gramme is very much less than that in the battery of Bunsen.—Prof. Tait gave a note on the law of cooling of bars. He mentioned that in his continuation of the experiments of the late Principal Forbes on the condition of heat in bars, he had been able to reproduce all Forbes's results with one exception. This was that the rate of cooling of the bar when exposed after being heated to a high temperature, was found by Forbes to be rather slower at first than afterwards. Or if we express the temperature of the bar in terms of its excess of temperature above the air, he found that at first it grows till it reaches a maximum and then falls off. Prof. Tait found this phenomenon always at the beginning of his experiments, but by heating the bar to temperatures higher than was required for his experiments, this peculiarity vanished before

the temperatures were reached upon which the results were based. On trying to explain this phenomenon by Fourier's conduction of heat, he found that the difference between the true and the false law of cooling should not last more than a fraction of a second, whereas it lasted more than 10 minutes. He accordingly concluded that it must be due to the fact that the thermometers did not acquire at once the temperature of the bar, but that some minutes must elapse before the whole of the thermometer has the proper temperature. He tested this by heating a bar and placing a thermometer in it as soon as it was allowed to cool. The phenomenon appeared. After a few minutes, when the thermometer was indicating the true rate of cooling of the bar, a second thermometer was placed in the bar close to the first. The second showed the same phenomena, though the first was now following the usual law of cooling. In 5 or 6 minutes the first and second thermometers gave identical results, but a third thermometer gave the old phenomenon when newly inserted alongside the others. Other experiments confirming these were also made. —Mr. J. Y. Buchanan read a note on the distribution of temperature under the ice in Linlithgow Loch. He had made these experiments while testing a new deep-sea thermometer by Negretti and Zambra. His results showed that with the depths for abscissæ and the corresponding temperatures for ordinates, the curve so plotted was one of contrary flexure—the contrary flexure being at the temperature of 37·6° F., which he expected further experiments would show was the temperature of maximum density for the water of the loch. One remarkable fact was that the temperature rose as the bottom was reached, being about 40·1° there. This he thought was due to the oxidation of matter at the foot, and this idea was confirmed by the fearful stench of the water.—Dr. Alexander Macfarlane continued his paper on the principles of the logical algebra. He showed that $x^2 = x$ is of the nature of a condition imposed on x and that a more general form is $x^2 = \pm x$. He applied the principles of the logical algebra to deduce the general conclusion from data of certain common forms, and gave theorems on the number and nature of such general conclusions.

Paris

Academy of Sciences, February 10.—M. Daubrée in the chair.—The following papers were read:—Last reply to M. Pasteur, by M. Trecul.—Fourth reply to M. Berthelot, by M. Pasteur.—On the existence of an apparatus prehensive or complementary of adherence, in parasitic plants, by M. Chatin. This apparatus is most commonly furnished by the parasite, sometimes by the sustaining plant, or both. More often the form is that of a bell, of which the sucker corresponds to the tongue. Sometimes the tissue of the supporting plant rises round and embraces the sucker; and sometimes both plants furnish uniting growths round the point of attachment of the sucker. The author shows how the prehensive arrangement is more or less strong according to circumstances, and he gives histological details.—M. de Lesseps read a letter from M. Roudaire, giving news of boring operations in the region of the isthmus of Gabes, which are so far encouraging. One fact stated is that fresh water was found at 4 metres depth at the highest point; this would be important in the case of cutting through the tongue of land.—Observations on the project of forming an interior sea in Eastern Sahara, by MM. Martins and Desor. Having visited the region in 1863 they disapprove of the project, on the score of mirage possibly falsifying observations, the water of the new sea and any change of climate ruining the date palm cultivation, on which the natives so largely depend, &c.—Researches on the formation of latex and laticiferous vessels, during germinative evolution, in the embryo of *Tragopogon porrifolius*, by M. Faivre.—On the determination of imaginary roots of algebraic equations, by M. Farkas.—Remarks on differential linear equations and those of the third order, by M. Combescure.—On a simple way of presenting the theory of potential, and on the differentiation of integrals in cases where the function under the sign $\int$ becomes infinite, by M. Boussinesq. —Hydroelectricity and hydromagnetism; experimental results, by M. Bjerknes. These relate to actions of two pulsating or two oscillating bodies with each other, or a pulsating with an oscillating body. In one set of experiments air columns in two bell jars immersed in water, were varied through tubes, by means of pumps, causing pulsations.—On green and phosphorescent light from molecular shock, by Mr. Crookes.—On the dissociation of hydrate of chloral (new method) by MM. Engel and Moitessier. The dissociation is effected at about 61° in an atmosphere of chloroform. (A mixture of hydrate of chloral and chloroform is submitted to distillation.)—Researches on the yeast of beer, by MM. Schützenberger and Destrim. They compare the modifications of yeast in presence and absence of sugar. Simple digestion of yeast at 30° for twenty-four hours made it lose 1·77 per cent. of solid matter. With yeast and sugar there was an increase of solid matter; about 11·3 per cent. of yeast, or 5·7 per cent. of sugar.—On the homologues of oxyheptic acid, by M. Demarçay.—Analysis of a honey of Ethiopia, by M. Villiers. This honey, called *tazma*, is gathered (without wax) in subterranean cavities by an insect resembling a large mosquito. It differs from other honeys by absence of cane-sugar.—On the banana, by M. Corenwinder. He points out variations in its composition. In the fruit, sound and ripe, the total proportion of sugar may rise to 22·06 per cent.—On a process of enriching phosphates of carbonated gangue, by M. L'Hote. He effects decarbonatation of the phosphate, by heating it to near a cherry red, and making steam act on it. The quick lime is separated by means of weak hydrochloric acid.—On various epizooties of diphtheria of court-yard fowls at Marseilles, and on possible relations of this disease with human diphtheria, by M. Nicati. Inoculation of a rabbit succeeded; and the appearance of the disease in the fowls seemed to occur along with an increase of human diphtheria.—On the sensibility of the eye to action of coloured light more or less diluted with white light, and on photometry of colours, by M. Charpentin. The chromatic sensibility remained constant provided the white light added did not exceed a certain pretty high maximum (in the case of red, ten or twelve times the intensity of the red). A very simple element of comparison (of white and colourless lights) is had in determining for each light used, the minimum quantity capable of causing the original sensation of colourless light.—Researches on the physiological properties and the mode of elimination of methyl-sulphate of soda, by M. Rabuteau.—On sub-periostic ossification and especially on the mechanism of formation of Haversian systems in the periostic bones, by M. Laulanié.—Researches on the liver of cephalopod molluscs, by M. Jousset de Bellesme. The liver in these animals is not analogous in function to the liver of vertebrates. It is a digestive gland, merely transforming albuminoid matters, and without action on fatty or amylaceous matters.—Observations on a shower of sap, by M. Mussel. This was observed in autumn from the leaves of *Abies excelsa*.—The death of M. Gervais was announced.

Göttingen

Royal Academy of Sciences, December 7, 1878.—On the ponderomotor elementary law of electrodynamics, by Herr Riecke.—The mean depth of the ocean and the proportions of land and sea, by Dr. Krümmel.

January 4.—The meteorite collection of Göttingen University on January 2, 1879, by Herr Klein.—Electrolytic friction compared with capillary friction, by Herr Kohlrausch.

CONTENTS

THURSDAY, FEBRUARY 27, 1872

THE GROWTH OF THE STEAM-ENGINE

A History of the Growth of the Steam-Engine. By Robert H. Thurston, A.M., C.E., Professor of Mechanical Engineering, Stevens' Institute of Technology, &c. (London: C. Kegan Paul and Co., 1878.)

WE have before us a very striking instance of the value of a popular knowledge of mechanical philosophy.

The book is essentially divided into two parts. The first six chapters contain a history of the steam-engine as a machine; while Chapters vii. and viii. are devoted to the philosophy of the steam-engine, its history, and application.

As regards the first part we have but little to say. In two points this part of the book is distinctly commendable; the author gives ample reference to the numerous sources from which he has derived his information, and he has cast aside all national partiality. The literature of the history of the steam-engine is now so considerable that it can have been no small task to condense an abstract into something less than the limits of a small volume and at the same time make it interesting; and the success which has been attained must be largely attributed to the excellent illustrations. Much of the matter has doubtless come ready to the author's hand, requiring but little modification. In places the style of the author very closely resembles that of Mr. Smiles. The plan adopted is that of awarding unlimited praise to the various inventors instead of distinguishing between their various claims. Throughout this part of the book little or no attempt is made to explain the physical questions involved or to record the steps by which a knowledge of the physical properties of steam has been acquired.

Had, however, the first six chapters constituted the book, it must have been allowed to pass as a fairly full and very interesting account of the development and application of the mechanism of the steam-engine; much in the same style as the work of Dr. Lardner but in many respects much better and containing considerably more matter.

The history of the philosophy of the steam-engine contained in Chapter vii. might also be allowed to pass, although the assurance with which commendations are distributed to such men as Carnot, Joule, Mayer, Rankine, Regnault, and Thomson could only have been justified by evidence of the possession on the part of the author of a very unusual appreciation of the highest theory of his subject—an appreciation for which the earlier part of the work had in no way prepared us, and which is certainly not shown by the merit of the author's comments in this chapter. These comments, however, are of the most cursory kind, and although for the most part unintelligible, they did not prepare us for the disclosure of the last chapter, in which we have the author's own exposition of the philosophy of the steam engine and its application.

A suspicion that the author's physics are not what they should be began to dawn upon us when we came to the paragraph in which he describes the essentials of a good furnace, which suspicion was strengthened at the following sentence—"A pound of carbon has been found to be capable of liberating by its perfect combustion resulting in the formation of *carbonic oxide* 14,500 British thermal units—" but we still reserved our judgment, as carbonic oxide might be a misprint for carbonic acid. However, on turning over the page we find the following erroneous deduction:—

"The laws of thermo-dynamics teach, as has been stated, that the proportion of the heat-energy contained in the steam or other working fluid which may be transformed into mechanical energy is a fraction, $\frac{H_1 - H_2}{H_1}$, of the total, in which H_1 and H_2 are the quantities of heat contained in the steam at the beginning and at the end of its operation, measuring from the absolute zero of heat motion. In perfect gases,

$$\frac{H_1 - H_2}{H_1} = \frac{\tau_1 - \tau_2}{\tau_1} = \frac{T_1 - T_2}{T_1 + 461{\cdot}2^\circ \text{ Fahr.}};$$

but in imperfect gases, and especially in vapours which, like steam, condense, or otherwise change their physical state, this equality does not exist, and $\frac{H_1 - H_2}{H_1} > \frac{\tau_1 - \tau_2}{\tau_1}$; and the fluid is more efficient than the perfect gas as a working substance in a heat-engine. In any case it is seen that the efficiency is greatest when the whole of the heat is received at the maximum and rejected at the minimum attainable temperatures."

This paragraph shows how completely the author has misapprehended the second law of thermo-dynamics. And this is not nearly all; the theory of the steam-engine is not so simple but that a slip of this sort might have been pardoned, but when we come to the author's application of the theory to the deduction of the economy of the "possible engine of the future," we have page after page of perfect nonsense, which not only shows that the author does not understand what he is writing about, but also shows that his erroneous views of, and imperfect acquaintance with, the theory of the steam-engine have led him into absurd errors from which the earlier inventors on whose work he has so boldly pronounced had emancipated themselves. How far this is the case will appear from the following quotations:—

"Heat-engines may be divided, for present purposes, into three principal classes, according to their disposition of rejected heat:

"I. Those which restore all heat rejected from the working cylinder to the reservoir from which it was derived.

"II. Those which restore a part of the unutilised heat of the working fluid, discharging the remainder from the system and allowing it to be wasted.

"III. Those which waste all heat rejected from the working cylinder.

"No existing type of steam-engine belongs to the first of the classes specified. Some forms of air and gas-engines are theoretically assignable to that class, as, by means of some form of 'regenerator,' they store up rejected heat and restore it to the succeeding charge of working fluid as it enters the cylinder. . . .

"There are two forms of engines of Class I., in which—were it possible to fully avail ourselves of them—all this waste of energy may be avoided:

"Type A. The working fluid, if expanded from the temperature and pressure of the boiler or reservoir quite down to the absolute zero, would have all its heat-energy transformed into mechanical work, and there would be no waste. The efficiency would be perfect.

"Type B. All heat rejected from the cylinder unutilised may be gathered up and restored to the boiler, there to serve as a basis upon which to pile a new stock of transformable heat-energy, instead of being, as now, rejected from the system entirely and lost. This done, there could be no loss, as all heat leaving the machine would be transmitted to exterior bodies as mechanical energy. Nothing being lost as heat, the efficiency of the engine would be unity and its economy a maximum.

"Forms of steam-engines may be conceived in which these methods (of saving heat now wasted) may be applied. Practically, however, it is evident, the first form of these two ideal engines can never be made successfully, since it would require to be made of such immense size that all the power derivable from it would be insufficient to move it."

An instance of the Type B is given. It is proposed to expand the steam in the cylinder until it is half liquefied, then separate the water from the steam and return them separately to the boiler, whereupon it is concluded.

"Under the conditions now assumed, it is evident that only that portion of the heat entering the engine which is surrendered by the condensation of steam doing work can be utilised. It is also evident that, in this form of engine, no heat can be lost; and, consequently, that the engine of Type B, which is operated as just indicated, will have yielded the exact equivalent of the net amount of heat expended upon it. *All heat rejected from the working cylinder, unutilised, being returned to the boiler*, there to form 'a basis on which to pile up a new stock of utilisable energy,' the engine is a 'perfect engine' in a broader sense than that adopted by Carnot. It is further evident that perfect efficiency is given for all ranges of temperature, and that what working fluid shall be adopted, and what temperatures shall be chosen, will be determined simply by practical conditions to be ascertained by experiment. . . .

"When this possible 'engine of the future' is likely to be introduced, if at all, can be scarcely even conjectured. It seems evident that its success is to be secured, if its introduction is ever attempted, by the adoption of high steam-pressures, of great piston-speeds, by care and skill in design, by the use of exceptionally excellent materials of construction, by great perfection of workmanship, and by intelligence in its management. There seems no tangible obstacle to its introduction." [1]

KINAHAN'S GEOLOGY OF IRELAND

Manual of the Geology of Ireland. By G. St. Kinahan, M.R.I.A., &c. (London: C. Kegan Paul and Co., 1878.)

THE appearance of another volume on Irish Geology, so soon after that of Prof. Hull, seems to show that the geologists of the Emerald Isle can be as active with their pens as with their hammers. Mr. Kinahan, indeed, has just claims to be heard when he treats of the rocks of his native country, for we suppose there is hardly any other living Irishman who has worked so long and so continuously among them. His volume, of course, coming before us as it does, cannot but challenge comparison with that of his director, Mr. Hull; and in truth it would almost appear as if this had been, consciously or not, in his mind. The two books, however, are on two very different plans. The general reader who wishes a pleasantly-written sketch of Irish geology, and of its relation to the scenery of the island, will find what he needs in Prof. Hull's chapters. Mr. Kinahan's work is more suited for professed geologists who propose to visit Ireland, and want to have some idea of the best districts to visit for their special purpose.

The volume is divided into five sections. In the first the author describes the sedimentary formations of Ireland, going over the country district by district, and pointing out in a useful, if somewhat prolix manner, the peculiarities of each. In the second he enters upon more speculative matter in an account of metamorphic and eruptive action as displayed among the Irish rocks. The third is devoted to the superficial accumulations, including the drift, the proofs of glaciation, changes of level, peat-bogs and prehistoric remains, and extinct mammalia. In the fourth section a brief description of the physical features is given from the writer's own point of view. The remaining division treats of the economically useful minerals of the country.

One cannot read Mr. Kinahan's pages without recognising his sturdy force of character. He takes up a position and holds it, one might almost think sometimes against his better judgment. It is satisfactory, however, to find him willing to yield sometimes, as, for instance, in his relinquishment of the absurd *yte* termination of rocks, though he takes care to inform us that while he yields to the entreaties of publishers and friends, he remains of the same opinion still. What a labour it must have been originally to change all his i-s into y-s! There are some other matters of orthography or even of grammar, which, when he feels himself to be in a compliant temper, he may take into his consideration. Why does he so constantly speak of "Cambrians" and "Silurians?" These are adjectives, and not nouns, and though in the field-slang of the Geological Survey, or of geologists generally they may be tolerated, they ought not to appear in any grave published work.

In the first section of the book it will probably occur to most readers that there is a want of breadth in the treatment of the subject. The local details are valuable as guides to the places and for the scattered facts they contain. But they lack that connecting thread which would have strung them all together and have carried the reader intelligently along, instead of leaving him, as he can hardly help feeling, struggling after the seven-leagued strides of Mr. Kinahan as he hops from district to district. Brief summaries for the separate regions, and a general connected summary of the whole for the system or formation, would have given the reader the key to the details. And as regards these details we fear they are often too vague to be of as much service to the field-geologist as he would wish them to be. The kind of detail he needs is supplied by the valuable "Explanations of the Geological Survey of Ireland." Mr. Kinahan often, indeed, refers to these "Explanations," but he would have given his volume a more practically useful character had he inserted in the account of each district a reference to the particular explanation in which a fuller description may be found. Such references, for the purposes of the student and the geologist who wants to study the matter on the ground, cannot be too precise and frequent. As it is, Mr. Kinahan's chapters on the rocks and struc-

[1] "The idea first suggested itself to the mind of the writer in 1858–59, when at Brown University, and while designing a peculiar form of 'drop cut-off engine,' which was intended to work with exceptional economy. The plans then conceived have gradually assumed a different shape, but still embody the essential principles here outlined. Mr. C. E. Emery, in 1868, proposed a similar plan."

ture of Ireland are full of suggestive matter, and will be consulted by all who wish to learn what has been done in Irish geology.

The section which will provoke most criticism is, no doubt, the second—on Metamorphic and Eruptive Rocks. The author himself hints something to this effect in his preface. There are objections to the terminology he has invented, the words themselves being unfortunately selected. For Daubrée's term "regional metamorphism" he substitutes *metapepsis*, and, speaks of *metapeptic* rocks. A dyspeptic geologist—and we suppose such beings exist in some number—will shudder at the very sound of these words. Then the old term "contact metamorphism," long ago so elaborately illustrated by Delesse, is replaced by *paroptesis*, and its rocks are called *paroptetic*. Another term, *methylosis*, is applied to a local kind of metamorphism, "due to the introduction and action of chemical substances from without;" and the rocks affected by it are named *methylotic*—a word which many a learner will at once surmise to be connected in some way with the methylated spirit he used to spill and smell of in the days of his practical chemistry.

But accepting these terms, there will be graver objections to some of Mr. Kinahan's metamorphic notions. It is specially unfortunate that he does not give any adequate grounds in this volume for enabling the reader who has not perused his other writings to judge on what detailed evidence his conclusions have been based. For example, he treats quartz-rock as one of the intrusive granitic rocks, and distinguishes it from quartzite or quartz-schist. But we have been unable to discover any passage which explains how he could distinguish these rocks, and what are their relative mineralogical and petrographical characters. Nay, while in one place he includes quartz-rock among the highly siliceous intrusive granitic rocks, he elsewhere speaks of it as having been again and again deposited by springs connected with volcanic action. Surely he does not wish us to believe that even a tyro in petrography would confound siliceous sinter with any form of granitic rock? Again, without giving any data, he speaks of "protrusions of limestone and dolomite." How does he imagine they were protruded? Were they thrust out as solid masses, or like the quartz-rock of his springs? He mentions them in connection with "a sheet of quartz-rock," and speaks elsewhere of having himself observed intrusive quartz-rock in many places. It is evident, however, that it would lead to the most hopeless confusion if the term quartz-rock, which has for generations included hardened siliceous sandstones, sometimes even with traces of organisms, were applied also to any member of the granitic family. Mr. Kinahan should invent another name for his intrusive quartz-rock. He has no timidity in names, and might hit upon one quite as euphonious as those already referred to.

Probably the most valuable part of the book is that which treats of the prehistoric remains. Mr. Kinahan is an authority on *crannoges*; and the digest therefore which he has given of known facts in this subject, besides its interest to the general reader, will be welcomed by geologists to whom the scattered papers in the Transactions of the Irish Societies are not familiar.

The illustrations are singularly poor, and seem all the more so by contrast with the sketches of the lamented Du Noyer, which have made the geology and scenery of Ireland familiar to many eyes all over the world. Could not Mr. Kinahan have availed himself of some of the drawings, published or unpublished, of his friend? Any additional publicity he could have given them would have been another tribute to the memory of a true artist.

While the author recognises the debt of gratitude owed by Irish geologists to Griffith and Jukes, there are some names which he passes over in strange silence. Why, for instance, could he find no room for the honoured name of Harkness? Surely, when he was writing about the metamorphic rocks of Donegal, he might have made grateful allusion to the geologist who, more than any one else, has thrown light upon these rocks. He quotes two or three times an opinion of Prof. Hull only to reject it, and these are all the direct references he deigns to make to the labours of one who has already done and is still doing so much for the cause of Irish geology; there being not the least allusion anywhere to the previously published volume by that writer. This may have arisen from mere inadvertence, and in that hope we take leave of Mr. Kinahan and his book, wishing for both that appreciation from geological readers which they deserve.

OUR BOOK SHELF

Studies in Comparative Anatomy. No. I. *The Skull of the Crocodile.* By L. C. Miall. No. II. *Anatomy of the Indian Elephant.* By L. C. Miall and F. Greenwood. (London: Macmillan and Co., 1878.)

PROF. MIALL has given in the first of these "Studies" a careful and systematic description of the Skull of the Crocodile, his object being to furnish to students a more complete account of the skull in this family of reptiles than is found in the usual treatises on the Comparative Anatomy of the Vertebrata. He commences by giving a general view of the crocodilian skull, and then sketches its mode of development, pointing out at the same time the relation of the cranial nerves to the post-oral clefts and arches. The individual bones of the skull are then described in detail. An elaborate account is given of the tympanic cavity and of its communications with the several Eustachian passages, which, together with the external auditory meatus, represent the cleft between the mandibular and hyoidean arches. Mr. Miall gives in an appendix a translation, with annotations, of Rathke's account of the development of the skull of the crocodile. The essay will be of great service to those desirous of acquiring a knowledge of the crocodile's skull.

In the second of these "Studies" Prof. Miall writes, in conjunction with Mr. Greenwood, an account of the anatomy of the muscular, vascular, digestive, and genito-urinary systems of the Indian elephant, together with some observations on the organs of special sense. This essay appeared originally in the *Journal of Anatomy and Physiology* for 1878, and in reprinting it the authors have reproduced the plates and woodcuts employed in illustrating their description as it appeared in that *Journal.* Throughout the essay frequent reference is made to the previous literature of the subject, and the authors point out any discrepancies between their observations and the descriptions of the other anatomists who have examined this species of elephant. The part of this essay which contains the greatest number of new facts is the description of the muscular system, which is very carefully done, and forms an important contribution to the myology of this huge animal.

Seconda Contribuzione morfologia e sistematica dei Selachi. Del Prof. Pietro Pavesi. (Genoa, 1878.)

IN 1874 Prof. Pavesi, of Pavia, described in the *Annali del Museo Civico* of Genoa a shark which had been captured at Lerici, in the Gulf of Spezzia, in 1871. It belonged to the genus *Selache*, but, from a peculiarity in the conformation of the rostrum, Pavesi considered it to be a distinct species from the great basking shark, *Selache maxima*, and named it *Selache rostrata*. The specific difference of this specimen has, however, been called in question by Canestrini, Steenstrup, and other ichthyologists, who were inclined to regard it as a monstrous form of the *Selache maxima*. In June, 1877, a male shark, also belonging to the genus *Selache*, was caught in the harbour of Vado, near Savona, and, being examined by Prof. Pavesi, forms the subject of this second communication to the *Annali del Museo Civico*, vol. xii. Its length was between ten and eleven feet. It had been eviscerated before coming into his possession, so that the memoir does not give an account of the abdominal viscera, but the external characters, the skeleton, the pectinated appendages, the brain and cranial nerves, and the vascular system, are described. The shark from Vado is almost identical, says the author, with that previously caught at Lerici. He then carefully reconsiders the systematic position of these specimens. He is strongly of opinion that the view that the specimen originally described was a monstrous form of *Selache maxima* is quite untenable. But his examination of this second specimen has convinced him that these sharks can no longer be regarded as a distinct species, and that they are young examples of the great basking shark, *Selache maxima*. The memoir is illustrated by a lithographic plate and by twenty-seven woodcuts.

Das Leben. Naturwissenschaftliche Entwickelung des organischen Seelen- und Geisteslebens. Von Philipp Spiller. (Berlin: Stuhr'sche Buchhandlung, 1878.)

THIS work may be said to be but an enlarged reproduction of a division of an earlier and more important work: "Die Urkraft des Weltalls nach ihrem Wesen und Wirken auf allen Naturgebieten," by the same author. Prof. Spiller, whose death it was our painful duty to announce last week, is the originator and founder of a philosophical theory on the first cause of all things. According to his view the world-ether is the architect of the universe as well as the fundamental cause of gravitation. In his works, particularly in the one just mentioned, the learned professor treats this world-ether theory in a most masterly manner, and whatever view we may take as to the correctness of his views—a question which we certainly do not wish to decide—it is only justice to point out that his explanations and definitions are all written in such a spirit of firm conviction of the truth of his theory, that an attentive reader cannot refuse his admiration and respect.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Leibnitz's Mathematics

I UNDERSTOOD Dr. Ingleby to say that he was prepared to make good his assertions, and to respond to Mr. Nelson's "call" as soon as I retracted, or justified, my former statement.

In 1871 Dr. Ingleby said it was exactly twenty years since the last vestige of presumption against the fair fame of Leibnitz was "obliterated."

Dr. Ingleby is evidently unacquainted with the work of Dr. Sloman (Leipzig, 1858; in English, Macmillan, 1860), else he would not have spoken of the "last vestige of presumption."

Kant's opinion of Leibnitz, which is *far more favourable* than that of Dr. Sloman, compares him to chemists "who gave themselves out to be possessed of secrets, when they had really nothing but a persuasion and a conviction of their capacity for acquiring such." This verdict, from a *true* metaphysician, ought to have much weight with Dr. Ingleby.

P. G. TAIT

Guthrie's "Physics"

SOME weeks ago (p. 311) you published in NATURE a review by Prof. Maxwell of a little book of mine on Practical Physics. It is not my intention to complain in any way of the review, partly because it would be a profitless trespass on your space, but mainly because, while the tone is unfavourable, the instances adduced by the reviewer go a long way to confute his own statements in all cases where there is any connection between the two.

Some well-meaning friend has composed and sent me a copy of the inclosed. There appear to be various opinions as to the authorship. It has even been suggested that Prof. Maxwell, with that sense of humour for which he is so esteemed, and with a pardonable love of mystification, is himself the author.

February 24 FREDK. GUTHRIE

REMONSTRANCE TO A RESPECTED DADDIE ANENT HIS LOSS OF TEMPER

Suggested by Prof. CLERK MAXWELL'S *review of* GUTHRIE'S "PHYSICS".

WORRY, through duties Academic,
It might ha'e been
That made ye write your last polemic
Sae unco keen:

Or intellectual indigestion
O' mental meat,
Striving in vain to solve some question
Fro' "Maxwell's Heat."

Mayhap that mighty brain, in gliding
Fro' space tae space,
Met wi' anither, an' collidin',
Not face tae face.

But rather crookedly, in fallin'
Wi' gentle list,
Gat what there is nae help fro' callin'
An ugly twist.

If 'twas your "demon" led ye blindly,
Ye should na thank him,
But gripe him by the lug and kindly
But soundly spank him.

Sae, stern but patronising daddie!
Don't ta'e 't amiss,
If a puir castigated laddie
Observes just this:—

Ye 've gat a braw new Lab'ratory
Wi' a' the gears,
Fro' which, the warld is unco sorry,
'Maist naught appears.

A weel-bred dog, yoursel' must feel,
Should seldom bark.
Just put your fore paws tae the wheel,
An' do some Wark.

$d\sqrt{\frac{m}{n}}$.

Unscientific Art

IN *Punch's* series of cartoons, "the man at the wheel" turns up now and again. The most recent example is that of date February 22: John Bull and Punch are strenuously holding a steering-wheel between them, in a tempestuous scene. I have a second example before me in the series of cartoons of Beaconsfield recently issued, No. 61: Disraeli has one hand on a steering-wheel, while the other holds a pistol directed to the powder magazine below; and he threatens to blow up the ship if Gladstone and Bright (climbing over the bulwarks behind) step on board. Other cases will be remembered. Now (neglecting here the political meaning of the pictures) these steering-wheels are wonderful productions, and how they serve for steering is a mystery. The wonder, remarked on by St. James, of "a very small

helm" turning great ships, is here outdone. The wheel stands, in all simplicity, between two uprights, or a slitted upright, fixed on the deck (or a raised platform); there is nothing behind or before the outer surfaces of the uprights. But an essential part of ordinary steering-wheels is the drum or axle extending generally a little way behind (and covered, it may be), on which are wound ropes or chains passing round pulleys to the tiller. A more modern form well known is a screw shaft with levers, &c. The omission in question in these cartoons leaves the scientific mind decidedly "at sea," and with little confidence in the steersman. I suspect the artistic type of mind is rather apt to neglect such details.

It is remarkable, indeed, how many matters belonging to simple observation escape notice by artists. I may perhaps be allowed to note a few points which have occurred to me in glancing over *Punch* from November to the present time, and the three books of cartoons of the *Punch* series.

The electric machine sometimes makes its appearance in *Punch*. In No. 53 of the Beaconsfield cartoons, that gentleman (as a professor) is arranging a circuit between an aristocrat and a working man for a shock. The electric machine behind is evidently meant for one of the Ramsden type, but the brass-work with points to collect the electricity is wanting, and the glass plate seems to have great concealing power. Again, the clever and fantastic sketch at the beginning of the Almanack shows an electrical machine of quite indescribable type, unless it be a Holtz, but it defies all mechanical conception. Perhaps it is not allowable to apply scientific rules to the brilliant insanity of such drawings, but I think there should be more basis of real existence than this one presents.

From many pictures we might be led to infer that left-handedness is much more common than it really is. Thus, a *pince-nez* is held in the *left* hand by Mr. Bright (in No. 19 of the Bright cartoons), by a church dignitary speaking to his daughters (*Punch*, December 21, p. 282), and by an old gentleman who receives a letter on the road on a snowy day (*Punch*, February 1, p. 39). In the Almanack (p. 5) a workman holds a cup in his left hand and a saucer in his right. Reins are frequently held in the right hand (which, I understand, is wrong)—one example is the cartoon of *Punch*, December 14, "Post Equitem." If something might be said for these cases, it is difficult to see how an artist can be justified in putting a quill pen behind the *left* ear, as in the case of Gladstone, when meeting Bismarck (last of Gladstone series), unless, indeed, the right ear were already occupied with one (which is not here the case). A similar remark seems applicable to a caricature of Ruskin by Sambourne (*Punch*, December 7, p. 254).

In an ingenious sketch (*Punch*, February 1, p. 37), in which a complex pocket-knife or sort of *multum in parvo* is made to take the aspect of a formidable animal, the spiral of the corkscrew turns the wrong way.

In one of the Bright cartoons (No. 33), that gentleman appears in court costume before a mirror which slants away from him upwards, but the image, I think, hardly corresponds to this.

One word more, and of a somewat different order of criticism. Heat of certain intensities and in certain circumstances may, of course, be very unpleasant. But, as we have had good reason to know lately, heat may be very welcome and agreeable. Therefore I venture to indict the cartoon of *Punch*, February 15, "Hot water, sir!" as flagrantly at fault. Beaconsfield is bringing in the morning's hot water to John Bull in bed. In the session of 1879 John Bull may very likely find himself "in hot water;" but in the connection to which the picture refers, hot water is a pleasant mitigation to the inevitable discomfort of washing. So John Bull's horrified look could not possibly refer to that. If he were being awoke, as I have been, in a hydropathic establishment, about 6 A.M., by a fiend in human shape, who showed a cynical determination to pack him in a cold wet sheet, the man's implements *might* arouse some horror. In the Beaconsfield cartoon, No. 90, "The Turkish Bath," the metaphor is, of course, all right: "You made it so confoundedly *hot* for me!"

Some of the foregoing are little points, but they prove this much, that there is room for improvement among artists of this class as regards correctness of observation and strict fidelity to fact. A. B. M.

Intellect in Brutes

In Mr. Nicols' instance of intellect in brutes (Nature, vol. xix. p. 365) he tells us that a plumber "had on several occasions been called in to examine into the cause of leakage of water-pipes under the flooring of houses," and then records *a single instance* of rats having knawed through a pipe. It is important to know whether the plumber knew of another case: for the idea at once suggests itself that the pipe had cracked through frost, and the rats then discovering the leakage gnawed it to get more water.

It has always seemed to me that brute reasoning is always *practical* but never *abstract*. They do wonderful things suggested by the objective fact before them; but, I think, never go beyond it. Thus, a dog left in a room alone rang the bell to fetch the servant. Had not the dog been taught to ring the bell (which on inquiry proved to have been the case) it would have been abstract reasoning, but it was only practical. The Arctic fox—too wary to be shot like the first who took a bait tied to a string, which was attached to the trigger of a gun—would dive under the snow and so pull the bait down below the line of fire. This is purely practical reasoning; but had the fox pulled the string first out of the line of fire *in order* to discharge the gun, and *then* to get the bait, *that* would have been abstract reasoning which he could not attain to.

This practical reasoning is just what young people do, before they can reflect. A boy the other day found the straps of his skates frozen. The fact only suggested *cutting* them. Not one of his schoolfellows reflected upon the abstract fact that the ice would melt if he sat upon his foot a few minutes. Hence brutes and boys are just alike, in that nothing occurs to either beyond what the *immediate* fact before them may suggest. The one kind I call purely *practical* reasoning, which both have; the other, *abstract*, which brutes *never* acquire; but the boy *will* as his intelligence develops. George Henslow

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In Central Park one very hot day my attention was drawn to the conduct of an elephant which had been placed in an inclosure in the open air.

On the ground was a large heap of newly-mown grass, which the sagacious animal was taking up by the trunkfull, and laying carefully upon his sun-heated back. He continued the operation until his back was *completely thatched*, when he remained quiet, apparently enjoying the result of his ingenuity.

It seems to me that *instinct* should have prompted the elephant to eat the grass, and that it was *reason* which caused him to use it for the purpose of diminishing the effect of the sun's rays.

New York, February 8 James J. Furniss

Bees' Stings

Will you allow me, as possessor of a couple of score of hives, to say a word respecting the discussion in your columns as to the effect on *Apis mellifica* of the loss of its sting and appendages.

As far as my observations go, the bee is not seriously injured by the loss, for though imprisoned and watched for some hours, as soon as released it flies back to its hive, and apparently resumes its work as before. However, any one sufficiently painstaking can settle the question finally by marking some such bees, and watching for their departure, and return laden with honey or pollen.

May I ask if any of your readers have yet determined the identity of bee poison and formic acid. The former is said, on exposure to the air, to solidify to a white crystalline mass, but formic acid requires, I believe, a temperature of 0° C. to effect this modification. J. P. Jackson

Bull's Mill Apiary, Hertford, February 18

P. LE NEVE FOSTER

A very numerous body of friends will have heard with regret of the sudden death of Mr. Le Neve Foster, the secretary of the Society of Arts. Though not himself an original worker in science, there were few men better known in scientific circles, or so universally liked where he was known, as Mr. Foster. His connection with the Society of Arts threw him amongst men working in nearly all lines of research, and there are probably few recent instances of the practical application of any new scientific discovery to industrial purposes in which he did not take some interest. Coming up to London with a fellowship from Trinity Hall, he was called to the bar in 1836, and practised for some fifteen or sixteen years

as a conveyancer. The natural bent of his mind, and some association of his relatives with the Society of Arts, led him to join it, and he soon became a member of its council. The Society was then in anything but a flourishing state. The necessity for such work as it had usefully done at the beginning of the century had passed away, and those who then controlled its destinies were hardly capable of striking out for it a fresh line of action. It was, however, just beginning to revive a little, when the proposal to hold the first great exhibition was taken up by Mr. (now Sir) Henry Cole, Mr. (afterwards Sir) Wentworth Dilke, and some others of the more enterprising spirits who were then gaining the upper hand in the Society. With this reforming party Mr. Foster was associated, and it tells much of the character of the man, of his freedom from self-seeking tendencies, that while other members of the little body worked their way upwards to honours and high positions, he was content to remain without reward, either pecuniary or titular, as a worker for the whole of his life. When Mr. Grove gave up the secretaryship of the Society of Arts in 1853, Mr. Foster, resigning the post he then held on its Council, was appointed to the office, an office which he held till the day of his death, last Thursday. Since then his career has been associated with that of the Society. This grew in numbers and influence, and so the Secretary's office increased in importance. During the twenty-five years in which he guided it, it did a great deal of good work, and, it may be owned, some which was not of much value. The trivialities soon passed away and were forgotten, the good work endures. In undertakings such as that of a public society most of the work and but little of the credit falls to the executive officer, and probably, if the truth were known, many of the crude ideas first launched into the world at the Society of Arts owed their ultimate success to their having been hammered into a practical form by the secretary. Ever ready with advice, the fruit of long experience, never bored even by the most importunate of inventors, ready to find something good, something to praise in the most impracticable of schemes, he won the friendship, even the affection, of all who knew him.

At many scientific gatherings his genial presence will be missed. His was a well-known figure at the British Association meetings. For thirteen years he acted as secretary to Section G (Mechanical Science), and from 1863 to 1866 he served on the Council of the Association. Taking an intelligent interest in several branches of science, it was to photography that he principally devoted himself. He was one of the earliest amateur photographers, and continued to work energetically at his favourite pursuit down to the time of his death. One of his last bits of out-door work, before his camera was laid aside for the winter, was to take a view of the Obelisk on the Embankment, a day or two before it was swung from a horizontal into a vertical position. He wrote a good deal on photographic subjects, mainly in the pages of the *British Photographic Journal*, and similar periodicals. He also wrote the article on photography in the series of volumes on "British Industries," published by Mr. Stanford. He was an occasional contributor to several of the scientific and technical journals, and wrote a good deal in the *Journal* of his own society, which, though not founded by him, was published from the beginning under his auspices, for he was on the Committee of Publications when it began, and his secretaryship commenced before the completion of its first volume. The older series of *Transactions*, it will be remembered, ceased some few years before the *Journal* was started. Mr. Foster was one of the founders of the Photographic Society, and served till a few years ago on its council. He was also President of the Quckett Club for a year. The manner of his death was startlingly sudden. Returning home after his day's work, he sat down to read the newspaper before dinner, when one of his family coming into the room after he had been by himself only a few minutes, found that he had fallen back from his chair dead. The cause of death was fatty degeneration of the heart. He died as we might all wish to die, at a ripe old age (nearly seventy), quietly and easily, after a good life's work, and in harness till the end.

He did not live to receive a testimonial which his friends had just collected for him, and these same friends now propose to do what they can to increase the amount for the benefit of those he has left behind.

DR. APPLETON

WE were only able last week to note briefly the loss which learning and science have sustained in the death of Dr. Appleton at the early age of thirty-eight years. Dr. Appleton was born at Reading, where, and at St. John's, Oxford, he received his education. His special bent lay more in a literary and philosophical than in a scientific direction; but, as we indicated, his services to the advancement of science in this country have been very great. He may, indeed, be regarded as the originator of the movement for the endowment of scientific research; and it is greatly owing to his unceasing activity and influence that anything has been done in this direction by Government. To quote the words of the memoir in the *Academy*, of which he was the founder, and which, for the sake of sound criticism and accurate knowledge, we trust will be a lasting monument to his energy, and the breadth of his culture:—"With an enthusiasm which nothing could chill, and a belief no opposition could shake, he endeavoured to inspire his countrymen with the same zeal for learning and science that he felt himself, and to rekindle among them the well-nigh extinguished love of disinterested study and research. Where others talked, he acted; where others wavered, he continued firm. Through good report and evil report he struggled on towards the goal he saw clearly before him, and the confidence he felt himself was communicated to those who worked with him. Gifted with the power of organisation, with boundless energy, and with the art of influencing others, he was marked out as the leader of a forlorn hope. Defeat was impossible for him, and disappointment only increased his courage and activity. It was in Dr. Appleton's hands that the movement in favour of the endowment of research took solid shape and organisation. His exertions brought about the meeting at the Freemasons' Tavern in 1872, which first drew public attention to the fact that the universities exist for something higher than the examination of young men. From that time forward Dr. Appleton took an active share in the agitation that resulted in the passing of the Universities Act of 1877, and none of the opportunities which his editorial position gave him were allowed to be lost. Besides letters in the *Times*, the *Pall Mall Gazette*, and elsewhere, he wrote two elaborate articles on 'The Economic Character of Subsidies to Education' and 'The Endowment of Research as a Form of Productive Expenditure,' both republished in the volume of 'Essays on the Endowment of Research,' of which Dr. Appleton was editor."

We are pleased also to be able to refer in this connection to an article in the *Athenæum*. Although there are some parts of this article which we certainly could not quote. In this it is acknowledged that Dr. Appleton had raised a great question, and given it a hold on public interest, "and there can be no doubt that the movement which he, more than any other single man, had set on foot had considerable effect on several of the provisions of the Universities Act passed by the present Government." . . . "For a man who died before he was thirty-eight it is no slight achievement to have forced such a question as this on the sluggish attention both of the public and of Parliament."

We are pleased not only that our contemporary should bear such hearty testimony to the energy and success of Dr. Appleton, in promoting the cause which he had at heart, but that it should be able to refer to the subject of endowment of research not only without bitterness, but with even some slight measure of approval. A cause which had so pure-minded, clear-sighted, and widely-cultured a man as Dr. Appleton on its side, must surely have some solid reasons in its favour.

Dr. Appleton is acknowledged by all who knew him to have been one of the most even-tempered of men. He was always cheerful and complaisant; opposition and even rudeness did not ruffle him; he returned to the charge smiling at every blow. He was a very quick and ready manager in such work as that of an editor, being full of suggestion, and prompt at meeting difficulties. He was a genuine philosopher, though he professed keen interest in all departments of knowledge, he did not make the mistake of over-estimating his own knowledge, or of pretending to an encyclopædiac mind. His great merit is that he really gave time and strength for "ideal" ends.

PRISON BREAD

IN two former papers[1] I discussed the dietetic value and chemical composition of brown bread and of aerated bread. The recent report[2] of the Committee appointed to inquire into the dietaries of the prisons in England and Wales having called public attention to the subject of the nourishment contained in different varieties of bread, the suggestions made in that report may be suitably considered at the present time.

On pages 21, 22, 23, 38, and 39 of the report will be found the statements and figures as to the bread question, which I propose to criticise on the present occasion. The committee begin by stating that "the flesh-formers in white bread amount to 7 or 8 per cent., according to the quality of the wheat of which it is made. In bread containing the envelopes (of the grain) they amount to about 10 per cent." Two or three years ago little fault could have been found with these statements; indeed, the Committee appear here as in other parts of their report, to have drawn some of their facts and figures from a work of my own on Food, published in 1876 for the Science and Art Department. Still I am not now prepared, in the light of the most recent analyses of wheat and its mill-products, to endorse the statement that brown bread, whether made so as to include all the flour, middlings, sharps, pollard, and bran, produced from a given weight of wheat, or with all these materials minus the coarse bran, will contain on the average 10 per cent. of flesh-formers. Why, there are some varieties of wheat, beautiful, plump, soft, white, floury wheats, which do not contain more than 8 per cent. of total nitrogenous matters of all kinds, including veritable flesh-formers. How then can a wheat of this kind, if simply ground up (whether the 4 per cent. of long bran it yields be included in or excluded from the bread), be made to yield a brown bread or whole meal bread containing more flesh-forming matter than 5½ per cent.? For the meal will have taken up nearly one half its own weight of additional water, and will now be proportionately more dilute as to all its nutrients.

And, again, I have previously pointed out that the coverings of the wheat grain contain, in varying, yet considerable proportions, nitrogenous compounds to which the flesh-forming property cannot be rightly attributed. Thus, it may easily happen that the inclusion of the 14 or 15 per cent. of mill products, usually rejected in bread-making (excluding the long bran), may not appreciably influence the proportion of flesh-formers in the loaf. These two considerations do not, in my opinion, lessen the desirability of substituting whole meal bread for white bread in our prisons, but they invalidate some of the Committee's calculations as to the amounts of flesh-formers supplied in the new prison-dietaries, and they further suggest a method of adjusting the nutrient ratio which should subsist between the nitrogenous and carbonaceous constituents of the day's ration. I will briefly discuss these two points.

We are told (on page 38) that 7 lbs. per week of bread will furnish the prisoner with 9·072 ounces of flesh-formers. Now, if the bread referred to be that recommended by the reporters, 7 lbs. should furnish, according to their own showing, no less than 11·2 ounces of flesh-forming nitrogenous matters. For they affirm such bread to contain on the average 10 per cent. of flesh-formers, and so the weekly allowance of 7 lbs. or 112 ounces of bread would furnish 11·2 ounces of these nutrients. They appear, however, to have assumed the bread in use to contain not 10, but only 8·1 per cent. of flesh-formers—at least, in the absence of direct analytical data, I deduce this figure from the calculated amounts of nitrogenous substance tabulated in the report. Indeed, I conclude that they have not made the fresh calculations rendered necessary by the altered composition of the proposed bread, but have adopted the old figures of Playfair and other writers on this subject. But taking average whole meal bread made as directed by the reporters, and from ordinary wheats, it would not be safe to reckon upon it containing as much as 8·1 per cent. of true flesh-forming material—my own experiments put it at a little above 7. But granted the higher figure, we then find that the prisoners with hard labour (with 7 days' confinement) receive no more than 14 ounces of flesh-formers to 96 of heat givers, reckoned as starch, during a week. The ratio here is 1 to 6·8, which differs too widely from the normal ratio (1 : 4½) to afford satisfactory sustenance to men expected to do hard work.

There are, however, two ways out of this difficulty. Why should not a part of the fine flour be excluded from the constituents of the meal for prison bread? Or again, why should not biscuit flour, tailings, and middlings be added to it from other sources? And the same result might be ensured, and the flesh-formers be at the same time more adequately represented in the bread, if care were taken to choose for prison meal the hard, horny and tail wheats, which are always more nitrogenous than the white, opaque and soft grains. It is true that some of these hard, translucent wheats, especially when they owe their character to unripeness or a wet season, contain a larger proportion than usual of non-albuminoid nitrogen, but in spite of this their percentage of true flesh-formers is always high. It would be quite easy, by chemical analysis of the samples of grain offered by contract to the authorities, always to secure a wheat containing 13 to 14 per cent. of true flesh-formers, and therefore capable of producing a bread with at least 9 per cent.

There are two other remarks suggested by reading the part of this report on Prison Dietaries which relates to bread. The Committee is clearly right when it urges the desirability of including most of the coarser mill products of wheat in the meal on account of the phosphates thus secured. And the proposed plan is a good one, of making the dough of the finer mill products only at first, and then introducing, when the dough is nearly ready for the oven, the middlings, sharps, and pollard; again, kneading the mass as quickly as possible, and then baking it. The excessive solidity and stickiness of most whole meal bread is thus avoided, since the ferments present in the seed coats of the grain have but little time to exert their action upon the starch of the flour. A. H. CHURCH

[1] NATURE, vol. xviii. p. 229. and vol. xix. p. 174.
[2] Report of Committee on Dietaries in Prisons, 1878

ISOMORPHISM

AT the regular meeting of the Berlin Chemical Society on February 10, Prof. Hermann Kopp, of Heidelberg, delivered an address upon "Isomorphism." Prof.

Kopp first proceeded to sketch the methods employed for determining the molecular and atomic weights of the elements. When an element can be volatilised conveniently, so that we can obtain its vapour-density, its molecular weight is readily decided. Those elements which enter with a large number of volatile, or gaseous bodies, like carbon, present but little difficulty. Those, like zinc, which form but one class of volatile compounds, leave much to be desired, for a series of homologous bodies are no better than a single member of the series. In this case, however, the specific heat of the element comes to our aid, and we can usually take such a multiple of its equivalent as will give, when multiplied by the specific heat, a product not far from six. Naumann's law also enables us to make use of the specific heat of salts as well as that of the elements, the product of the specific heat by the sum of the atomic weights being nearly equal for similar compounds, and usually six times that of the number of atoms in a molecule. But this fails in some cases, probably, because we cannot take the specific heat at a sufficiently high temperature, as in the case of ice. In many cases where the above tests fail, isomorphism holds good. But totally unlike bodies, containing an unlike number of atoms in the molecule, have the same crystalline form. To avoid this dilemma, Prof. Kopp proposes to limit the term isomorphous to those compounds which possess the same crystal-forming power, as proved by their ability to crystallise together, or, if unequal in solubility, the ability of one crystal to grow in a solution of the other. Both of these cases were beautifully illustrated by alums. If a trace of a chromalum solution be added to a solution of potash-alum, each crystal that forms will contain both, as shown by the reddish tinge, and the colour deepens as the quantity of chromalum added is increased. On the other hand, when a crystal of one sort of alum is placed in a solution of the other kind, it continues to grow. Fine specimens of such crystals were exhibited by the speaker, who is remarkably skilful in this matter of growing and nursing crystals. Many other isomorphous salts were exhibited, such as the sulphates of magnesia and nickel; in some cases two different salts had been deposited alternately over the crystal of a third salt. Most interesting were rhombohedra of calxspar covered with nitrate of sodium, thus proving these two bodies isomorphous. The professor acknowledged that he had had much difficulty in obtaining these, and had utterly failed to make a crystal of arragonite grow in a solution of nitrate of potash. Prof. Kopp said, in conclusion, that unlike number of atoms could not replace each other in a molecule of two isomorphous bodies. Sulphate of cadmium will crystallise with eight equivalents of water to three of the anhydrous salt. Sulphate of didymium crystallises with eight equivalents of water. Both have the same crystalline form, but two atoms of didymium seem to replace the three of cadmium:—

$$Di_2S_3O_{12} + 8H_2O;$$
$$Cd_3S_3O_{12} + 8H_2O.$$

But these salts will not crystallise together, and crystals of the latter, from a mixed solution, contain no pinkish tinge of didymium.

HER MAJESTY'S ASTRONOMER AT THE CAPE

MR. DAVID GILL has been gazetted successor to Mr. E. J. Stone in the direction of the Royal Observatory, Cape of Good Hope. The discrimination exercised by the First Lord of the Admiralty in this appointment, we are confident will be appreciated and applauded by astronomers generally. Obtaining his first experience in practical astronomy in the Observatory at Aberdeen, and in a private observatory which he erected in the same place, Mr. Gill was so fortunate as to be associated with Lord Lindsay in the designs and details of the large observatory founded by this nobleman at Dun Echt in 1870, taking the position of chief of the staff. He thus became engaged in the organisation of the expedition to the Mauritius fitted out by Lord Lindsay for the observation of the transit of Venus, on which occasion advantage was taken of the circumstance of a heliometer forming part of the equipment to determine the sun's distance by measures of the planet Juno, being the first trial of the method, and attended with satisfactory results; the details of this work were published by Lord Lindsay as the joint work of himself and Mr. Gill. In connection with the same expedition, Mr. Gill arranged and personally conducted the whole of the chronometric and telegraphic longitude determinations connecting Berlin, Malta, Alexandria, Suez, Aden, Bombay, Seychelles, Reunion, Mauritius, and Rodriguez. It was while engaged upon these operations that he undertook, at the request of the Khedive, the measurement of the first base line of the geodetic survey of Egypt. In 1877 Mr. Gill laid before the Royal Astronomical Society a proposal to determine the sun's distance by heliometric observations of the planet Mars about the very favourable opposition of that year, Lord Lindsay lending his heliometer for the purpose. The proposal met with the support of the Astronomer-Royal and Council of this Society, and was further aided in its execution by a grant from the government funds in the hands of the Royal Society. The Island of Ascension was fixed upon as a favourable station for these observations, and Mr. Gill proceeded to Ascension in June, being occupied there about six months in the necessary preparations and carrying out of the scheme. The reductions are still proceeding, but in proof of the importance attached to this attempt to obtain a reliable value of the solar parallax and the interest felt by the leading astronomers of different nations in his work, it may be mentioned that on asking for aid in the accurate determination of the positions of the stars observed with Mars, his request was cordially acceded to at the following observatories:—Greenwich, Oxford, and Liverpool, Albany, U.S., Berlin, Cambridge, Mass., Cordoba (the national establishment of the Argentine Republic), Königsberg, Leipsic, Leyden, Melbourne, Paris, Pulkova (the Imperial Observatory of Russia), and Washington.

We will express the hope that Mr. Gill may carry to his new sphere a continuance of the great energy he has hitherto shown and repeat our conviction that his nomination by the First Lord to the important position of "Her Majesty's Astronomer at the Cape," will be hailed with great satisfaction in the astronomical world. It is understood that Mr. Gill leaves England early in May, arriving at the Cape in good time to confer with Mr. Stone upon the future work of the Observatory.

OUR ASTRONOMICAL COLUMN

THE NAVAL OBSERVATORY, WASHINGTON.—The Report of Admiral Rodgers, superintendent of this great astronomical establishment, for the year 1878 has just been issued. The operations of the institution have been more than usually extended, involving expeditions for the observation of the transit of Mercury on May 8, and the total solar eclipse of July 29. The 26-inch refractor has been in charge of Prof. Asaph Hall, with Prof. Holden as assistant, and has been constantly employed in the observation of satellites, double stars, and nebulæ, and occasionally of comets. Admiral Rodgers mentions that many foreign astronomers visiting the United States on the occasion of the eclipse, took the opportunity of inspecting this instrument, expressing very generally an opinion that the mounting was too light, and in this opinion the superintendent to a certain degree coincides,

although it is pointed out that during the five years that the equatorial has been mounted, "the position of the pole of the instrument has changed only a fraction of a minute of arc." The observations of the satellites of Saturn refer mostly to Japetus, Hyperion, and Titan. The disappearance of the ring took place February 6; Bessel's elements were verified by observations of its angle of position on thirty-six nights by Prof. Hall, and on twenty nights by Prof. Holden. There are also observations of the satellites of Uranus and Neptune, besides the fine series on the two satellites of Mars which were placed in the hands of astronomers some time since. A good series of measures of the companion of Sirius was obtained, and the six stars in the trapezium of Orion have been measured in connection with observations of Mr. Otto Struve's selected list of stars for determining the personal errors of observers. Prof. Holden observed the Orion nebula on twenty-eight nights, also six others of the more interesting of this class of objects.

The transit-circle and the 9·6-inch equatorial have been in charge of Prof. Eastman; 3,450 observations were made with the former instrument during the year, while the equatorial has been occasionally employed for a very necessary auxiliary purpose when it is desired to observe the fainter or less accurately computed minor planets on the meridian, viz., in determining previously the approximate correction of the ephemerides; for want of this necessary preliminary observed at Washington, a considerable number of observations on the meridian have been put upon record as observations of faint minors, which have been found to belong to small stars, to the equal vexation of observers and computers.

During the transit of Mercury, seventy-two photographs of the planet upon the solar disk were made at Washington by Mr. Rogers, with one of the photoheliographs used for the transit of Venus. Prof. Harkness proceeded to Texas for the observation of this transit of Mercury, succeeding better with the later than the earlier half of the phenomenon. The compilation and discussion of the observations is proceeding under Prof. Eastman, and will soon be ready for publication.

With regard to the total solar eclipse, it is stated that the liberal appropriation authorised by Congress allowed of a number of separate expeditions being organised, and the co-operation of the leading astronomers of the United States was invited and cordially responded to; but, while the Observatory of Washington was enabled to assist in a financial point of view, the heads of expeditions were left free to arrange their own plan of observation. The report enters briefly into particulars of the stations and success of the observers, to which space will not allow further reference here. With respect to the search for an intra-Mercurial planet or planets, it is mentioned that the following, in addition to Prof. Watson, were so occupied, at least during a part of the time that the sun was obscured:—Prof. Asaph Hall at La Junta, Colorado, with a 5-inch Alvan Clark equatorial, power 150 diameters, sweeping south and following the sun to about 10° distance; Mr. O. B. Wheeler at the same place, with a similar instrument, sweeping below and preceding the sun; Prof. Newcomb at Separation, Wyoming, and Professors Holden and Pritchett at West Las Animas, Colorado, also conducted unsuccessful sweeps for an intra-Mercurial planet.

The Washington Observatory has made arrangements for dropping a time-ball in New York city, at noon daily, which took effect from September 10, 1877; there have been a few failures, the cause of which is explained. The volume of observations for 1875 was daily expected to be delivered from the press at the time the Report was drawn up: we presume there are few real astronomical workers who have not experience of the liberality with which the handsome volume annually issued has been distributed by the United States Naval Observatory.

TEMPEL'S COMET, 1867, II.—Since our last note referring to this comet, M. Gautier has published sweeping-ephemerides, in the calculation of which he first assumes the perihelion passage to occur May 10·9416 Berlin mean time (that being the epoch fixed by his calculations after taking into account the action of Jupiter during the present revolution, which has delayed the comet less than three days), and then varies this date by ∓ 4 days; he believes the error of his computation will not exceed these limits. The following are the positions taking T = May 10·9416 for midnight at Berlin, or roughly for 11h. Greenwich time, during the next period of absence of moonlight, or rather beyond it:—

1879.	Right Ascension. h. m. s.	North Polar Distance. ° ′	Log. Distance from Earth.	Intensity of Light.
March 10	15 56 1	98 54·4	0·0960	0·188
,, 14	16 1 41	99 10·5	0·0808	—
,, 18	16 7 2	99 26·2	0·0657	—
,, 22	16 12 2	99 41·6	0·0506	—
,, 26	16 16 39	99 56·9	0·0355	—
,, 30	16 20 51	100 12·6	0·0206	0·278

An acceleration of four days in the time of arrival at perihelion will alter the comet's position on March 10, + 10m. 1s. in R.A., and + 1° 4′ in N.P.D.; and on March 30, + 11m. 51s. in R.A., and + 1° 11′ in N.P.D.

BIOLOGICAL NOTES

NEW FISHES FROM CENTRAL ASIA.—The last number of the *Bulletin* of the Imperial Academy of Sciences of St. Petersburg contains an interesting communication from Prof. Kessler on the fishes obtained by Prjvalsky during his recent expedition to Lob-Nor, a district previously unvisited by any naturalist. Herr Prjvalsky's collection from Lob-Nor and the basin of the Tarim contained examples of eleven species of fishes, eight of which belong to the family of *Cyprinidæ*, and three to that of *Cobitidæ*. As might have been expected, nearly the whole of these are new to science, and belong to genera (*Diptychus*, *Schizothorax*, &c.) characteristic of the high lands of Central Asia. One of the Cyprinoides is so peculiar as to necessitate the institution for it of a new genus, which Herr Kessler proposes to call *Aspiorhynchus*. *Aspiorhynchus prjvalskii*, as Prof. Kessler names this fish, in honour of its discoverer, inhabits the lower Tarim and Lob-Nor, where it attains a considerable size and furnishes an excellent article of food. Prof. Kessler suggests that two of the fishes obtained by the late Dr. Stoliczka during Forsyth's expedition to Yarkand, which were referred by Dr. Day to the genus *Ptychobarbus*, probably belong to his genus *Aspiorhynchus*.

DREDGING OPERATIONS, GULF OF MEXICO.—The last *Bulletin* (No. 9) of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass., contains an account of some wonderful new or rare forms of echini, by A. Agassiz, of corals by L. F. de Pourtalès, and of ophiurans, by T. Lyman, all the specimens having been dredged, during the survey of the United States steamer *Blake*, in the Gulf of Mexico. Preceding the technical descriptions there is a bibliographical notice of the publications relating to the deep sea investigations carried on by the United States Coast Survey from 1850 to the present time. Of the echini described and figured in the present number is a most interesting new species of Dorocidaris (*D. blakei*). While the recent Cidaridæ, so far as at present known, do not by any means show the great variety in the form of their spines, which is found so common among the fossil genera of the family; yet here we have at least one species in which the variety of the shape of the spines is extreme. Its long tapering spines would have indicated its position in Dorocidaris, but its extraordinary flattened fan-shaped spines seem nearly identical with those of the Jurassic genus Rhabdocidaris—when

alive these echini were of a brilliant vermilion colour. *Salenia pattersoni* spec. nov., is described as the most exquisitely coloured of the living Salenidæ, thus far found; the test was of a light cream colour, as well as the shafts of the primary spines. These are banded with a brilliant vermilion, the two colours nearly equally divided. The secondary spines are also cream-coloured, but separated at the base by dark violet lines which extend from the apical to the actinal system. Similar dark violet lines separate the genital and ocular plates. *Conoclypus sigsbei* is described as a magnificent species, by far the most striking sea-urchin which A. Agassiz had ever seen. The first time it was seen the dredge brought up half a dozen of the huge, brilliant lemon-coloured specimens. All these species, as well as the remarkable *Periaster limicola*, are figured from photographs. Count Pourtalés describes a number of new or rare forms of corals. As far as our present knowledge goes, he writes, no sea-bottom can rival in abundance of deep-sea corals the West Indian. It is not at all unfrequent for a single cast of the dredge to bring up a dozen different species represented by more or less numerous specimens of each. A very young specimen of Holopus was dredged from a depth of 100 fathoms. It has been sent for study to Sir Wyville Thomson, but a beautiful figure by A. Agassiz is here given. Several new species of Antedon are described by Pourtalés. A large number of new species and two new genera of ophiuroids are described by Theodore Lyman. The descriptions are accompanied by excellent figures.

United States Fish Commission.—Messrs. G. Brown Goode and Tarleton H. Bean give an account of some fifty species of fishes from the east coast of the United States, some of which are new to science and all of which are new to the fauna of that portion of the American States. Among the more interesting of the new forms may be mentioned *Phycis chesteri*, the largest specimens measured without the tail about eight inches in length; they were taken off Cape Ann. A new species of Dr. Günther's genus Haloporphyrus was taken on the outer edge of Le Have Bank at a depth of 400 to 500 fathoms. Two specimens of the rare *Remoropsis brachyptera* (Lowe) Gill, were obtained; one was found clinging to the side of a sword-fish, harpooned in the channel south-west of George's Bank, and the other on the deck of a Halibut trawler fishing to the north-east of George's Bank, at a time when sword-fish were being taken on the trawls. A specimen of *Nemichthys scolopaceus* was taken alive from the stomach of a cod caught on the same bank. *Amia calva* is reported from St. John's River, Florida, and from Spruce Creek, a tributary of Halifax River, about lat. 28°. Its range has not hitherto been recognised south of Charlestown, South Carolina, from whence Garden sent specimens to Linnæus (*American Journal of Science and Arts*, January, 1879).

American Œdogoniaceæ.—Dr. V. B. Wittrock has just published a revision of the species of Œdogoniaceæ found in America, as far as they are known (*Botaniska Notiser* utgifne af O. Nordstedt, November, 1878). The list contains twenty-three species belonging to the genus Œdogonium, and eight belonging to the genus Bulbochæte. Of these, nine are found in Greenland, five in Pennsylvania, one in California, five in Mexico, three in the West Indies, one in Venezuela, one in Bolivia, and seven in Brazil. It would seem very certain that this number constitutes but a small part of the species which will by further investigations be discovered; still it enables the author to draw, with a considerably high degree of probability, the following conclusions:—1. That the œdogoniaceous vegetation of America differs but little from that of the European. 2. That the species found towards the more northern portion of this area are perfectly identical with those to be met with in Northern Europe, while the species met with in the more southern portion of the same area are either species quite different from those met with in Europe, or, at most, extreme varieties of European forms. Only one of the South American species forms an exception to this (*Œdogonium crispum* (Hass.), Wittr.), which would seem to be nearly a cosmopolitan. 3. That the genus Bulbochæte has in America, as in Europe, most of its species indigenous to the cold temperate or arctic zone. Of the eight species known from America, five are natives of Greenland.

Chemico-Agricultural Stations in Italy.—Stations for the scientific observations of subjects connected with agriculture in its widest sense, have now been for several years established throughout Italy. These are under the general control of a Minister of Agriculture. We have lately received the reports (Atti) of the stations at Rome and Palermo, contributed by Prof. J. G. Briosi; they have, as might be expected, mostly to do with the subject of the diseases of the vine and the olive. Among the more important of these reports are the following: On the Phytoptus of the Vine (*Phytoptus vitis*), with figures; an account of the Marciume of the Vine (*Albinia wockiana*), with figures; on a Fungoid Disease attacking Lemons (*Fusisporium limoni*), with figures. At Messina a lemon-tree, in good condition, of fair size, will, it is reckoned, produce about 2,000 ripe fruit each year. These fruits are sold at from twenty to forty lire the thousand, according to size and quality, so that a lemon orchard is of great value, and a good deal of distress has been caused by the destruction of the lemon crops by this disease.

Asparagin in Plants.—The physiological *rôle* and distribution of asparagin in the plant kingdom have been lately studied by Herr Borodin (*Botanische Zeitung*, 51 and 52, 1878). He states, as the result of his researches, that whenever a vigorous part of a plant becomes poor in non-nitrogenous substances, asparagin occurs as a product of decomposition, and accumulates. This may be explained in either of two ways: either the presence of non-nitrogenous matters hinders the decomposition of albumen, while these alone are decomposed; or (conversely) in life albumen is always decomposed and asparagin constantly formed, but where carbohydrates are present albumen is regenerated, and it is only where these are deficient that asparagin accumulates. The former hypothesis supposes different processes of decomposition in life according as carbohydrates are present or not; Herr Borodin thinks it therefore the more improbable, and adopts the other, doing so the more readily that the regeneration of albumen from asparagin and carbohydrates certainly occurs, and is necessary for the transference of the albuminous matters. Not all carbohydrates are adapted for regeneration of albumen from asparagin, and therefore asparagin may accumulate even when carbohydrates are present. Such unsuitable carbohydrates are starch and the oils, whereas glucose is the suitable form.

THE PIC DU MIDI OBSERVATORY

OUR readers may remember that early in the year General de Nansouty, the hardy director of the Pic du Midi Meteorological Observatory, was cut off from communication with the world below, the severe weather having so affected the telegraph as to prevent it from acting. Fears were entertained for the General's safety, and M. Albert Tissandier resolved to organise a party for the ascent of the Pic and the succour of the veteran observer. An interesting account of this ascent appears in *La Nature*, to which we are indebted for the accompanying illustrations. The snow-storm having somewhat abated at Bagnères-de-Bigorre on January 9, M. Tissandier resolved to attempt the Pic next day, in company with three of General Nansouty's usual guides.

They set out at 9 A.M. on the 10th, and in spite of the deep snow and fallen avalanches, the ascent was at first not difficult. After equipping themselves for snow work at some huts occupied only in summer, the ascent was begun in earnest. The weather was grey and uncertain, the temperature 0° Cent., with a thick mist that prevented anything being seen beyond 300 metres. The snow became deeper and deeper as they advanced, and one of the guides went before to show the way, the others following the marks of his footsteps up the steep slope of the mountain side. Sometimes they were buried to the waist, and often they had to rest to recover breath. The ascent was slow and difficult, but they were often rewarded by the magnificent effects resulting from the play of light upon the snowy landscape beneath, or of the clouds advancing majestically into the midst of the snows. After attaining an altitude of 6,000 feet, they had got over the steepest part of the slope. But now the weather changed, the wind rose, and mists more and more obscured the sky. Squalls of snow were driven into their faces, and seriously hindered their progress. Alongside the track heaps of snow showed where avalanches had recently fallen from the rocks above. The telegraph posts, 7 metres high, were often buried; five or six of them were even broken by the violence of the recent storms, and the wires were broken. The weather got worse as they ascended, and M. Tissandier had all the symptoms of mountain-sickness, which he had not experienced before, even when ascending Mont Blanc. At last, however, the summit was reached, and, as might be expected, General de Nansouty gave the party a warm reception. A glorious fire and an excellent dinner soon set M. Tissandier all right again.

The establishment of the General is far from being luxurious, M. Tissandier tells us; although none of the usual necessaries of life are wanting, one is struck with the devotion which impels him, for the sole purpose of advancing science to accept an existence so isolated, so primitive, and that during eight months of the year.

The observatory of the Pic du Midi is most picturesque. We enter first a passage with glass doors at the sides, in order to protect as much as possible from the violence of the wind and the gusts of snow. The telegraphic office is at the bottom. A respectable provision of wood furnishes this passage; a few hens inhabit it; one of them was slaughtered in M. Tissandier's honour. A room adorned with an immense fireplace is next presented to the visitor's view; it is the vestibule. The guides sleep here on a camp bed, and have for messmates two dogs and two cats, presided over by the intendant, the faithful guardian of the observatory. All round this apartment, carefully arranged as on shipboard, may be seen a variety of provisions. The dining-room opens in this vestibule. In summer a separate part of the building is arranged for the reception of tourists, and a stable for horses is placed below the principal structure.

To the first storey there is no staircase, as there is no room for it; there is only a ladder with a knotted cord as balustrade. On ascending this, a small vaulted room is entered; a stove ruddy with fire heats the whole floor, and the cold of the outside is unknown in these hospitable chambers. The chief ornaments of this apartment consist of two sets of beds, one near the floor, used by M. Baylac, the second observer and devoted companion of General de Nansouty. Above is another bed, or rather shelf, to use the General's expression; this is for the use of visitors. It is reached by a ladder, and the mattress consists of an excellent sheepskin, on which, M. Tissandier declares, he slept so soundly, 8,000 feet above the sea, that he reluctantly left it on the morning after his arrival. On this first floor the General has a workroom in common with M. Baylac. This room is too small for the work which has to be done in it.

Everybody is up at daybreak; this is the inexorable order. The General then begins the day's observations. It is necessary to go outside to examine the thermometers and barometers, which are placed under a shelter constructed on a stone terrace. Every two hours, and oftener when the atmospheric conditions require it, the observations are renewed, precisely recorded, and preserved with care. Thus the whole day is passed, night alone putting an end to the work. M. Tissandier bears testimony to the energy and patience of the courageous observer of the Pic du Midi in carrying on his work.

FIG. 1.—Ascent of the Pic du Midi, January 10, 1879.

Happily the rather too primitive arrangement of the General will soon be changed for the better, thanks to the generous donations of those who love and desire to advance science.

During the small amount of leisure which can be found between the hours of observation, General de Nansouty directs his companion in a great variety of labours. A very interesting herbarium of the flora of the high regions of the Pic du Midi has thus been formed. M. Tissandier admired some rare plants, such as *Gentiana glacialis*, *Daphne cnæorum*, *Salix herbacæa*, &c. Mineralogical

Fig. 2.—Observatory of the Pic du Midi.

Fig 3.—The crests of Peune Longue (Pic du Midi) emerging above the clouds.

specimens are also collected and arranged. Traps are also laid by M. Baylac, and thus a fine collection of ermine skins has been formed.

The guides repaired the damage to the telegraph which had interrupted the General's communication for ten days. M. Tissandier says rightly that these accidents

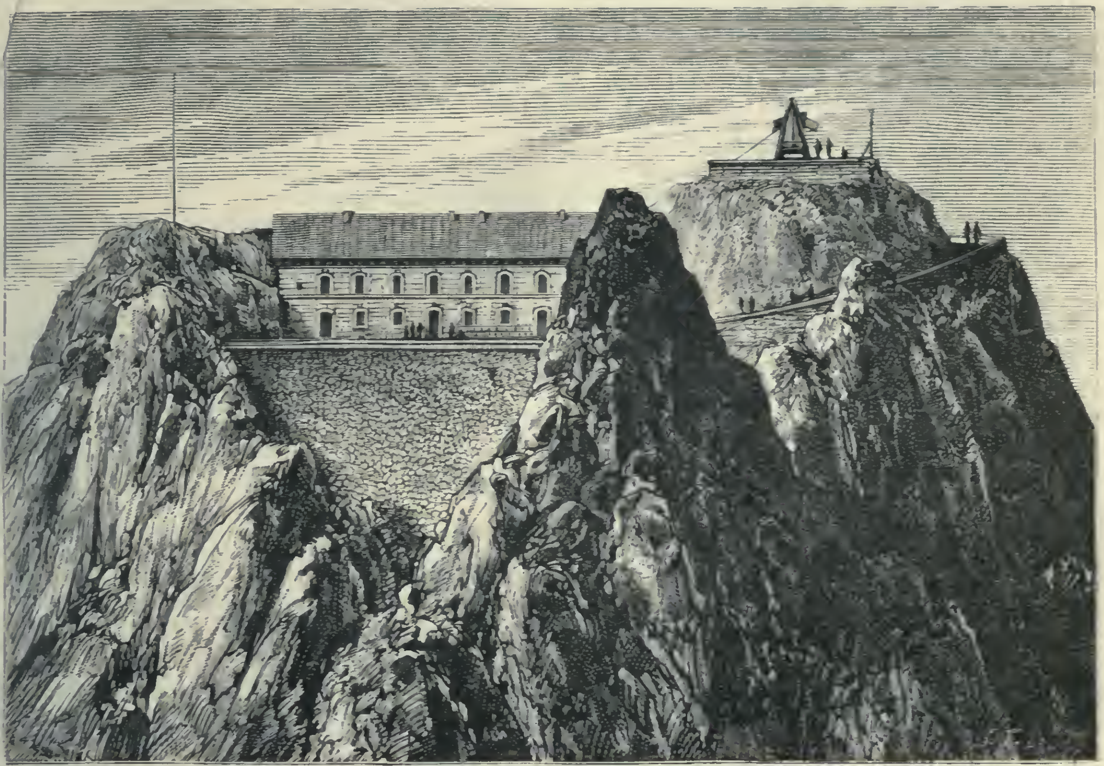

FIG. 4.—View of the New Observatory of the Pic du Midi, now building

should not be allowed to happen to a man who so generously devotes himself to the progress of a department of science that is of the greatest national utility. For at least a part of the distance between the summit and the plain the wires should be placed underground, and thus beyond the reach of injury from avalanches and

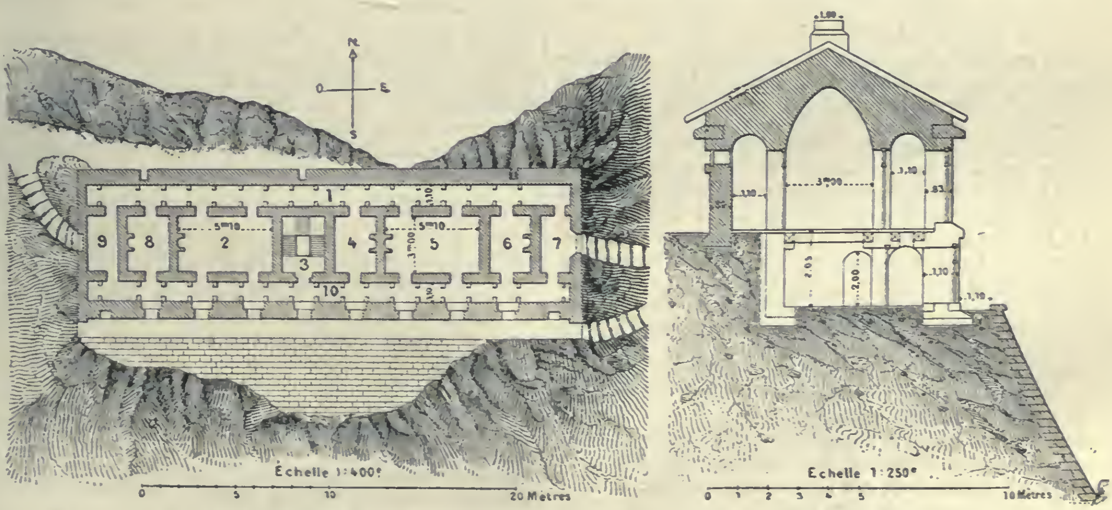

FIG 5.—Plan and section of the New Observatory. Plan of the first floor:—1, Passage, magazine; 2, *Salon*; 3, Staircase; 4, Dining-room; 5, Work room; 6, Guest-chamber; 7, Telegraph; 8 and 9, Bed-rooms. The ground-floor will be used for provisions.

storms. After M. Tissandier's visit, our readers may remember, telegraphic communication was again interrupted with the Pic. M. Tissandier bade the General good-bye on the 12th, and during his descent took several sketches. While it took him nine hours to make his ascent, he came down in four.

The improvements contemplated by General de Nansouty comprise an entirely new and much more solid and durable structure at the very summit of the mountain. A few generous friends of science have come to his aid and placed in his hands the means of carrying out the execution of his enterprise. M. Bischoffsheim gives 15,000 francs, the Minister of Public Instruction and the Minister of Public Works each 10,000 francs, the Academy of Sciences 1,200 francs; and large sums have been given by various other societies and individuals while many smaller subscriptions, down to one franc, have been placed at the General's disposal. There is every reason to believe, that though the work will be much more costly than originally expected, it will be thoroughly and promptly completed.

Our illustration (Fig. 4) shows the new observatory as it will appear when the works are completed; it is at present half built. To the right is seen, perched on a platform, the shelter for the instruments of observation. In the centre is the dwelling-house, the arrangements of which will be seen in the plan, Fig. 5. To the left is the lightning-rod, intended to protect the structure from the lightning which so frequently strikes the summit of the Pic. This lightning-rod, with its cable, which plunges 500 metres lower down in Lak Oncet, has cost 2,800 francs. The excavation of the hollow in which the structure is built has cost 2,500 francs; so much of the building as has been raised, that is one half, has cost 22,000 francs. No doubt all the necessary funds will be forthcoming; it is said that the Minister of Public Instruction will this year contribute another sum of 10,000 francs.

The example of General de Nansouty has already borne fruit in France. General Farre has installed an observatory at the foot of Infernet. In Provence a movement has been set on foot to place one on Mont Ventoux. With the fine observatory of the Puy de Dôme, France will possess an important net-work of high meteorological stations which cannot but render valuable services to a knowledge of atmospheric phenomena, and be of great practical value to national industry. Let us hope that in a very few months our own country will possess at least one of these lofty stations which the French Government, the French people, and French science think it their interest and duty to give substantial encouragement to.

GEOGRAPHICAL NOTES

AT the meeting of the Geographical Society on Monday evening Sir Rutherford Alcock announced that the Earl of Dufferin had formally resigned the office of president in consequence of his appointment to the St. Petersburg embassy. Mr. Clements R. Markham read a paper on the basin of the River Helmund and the smaller basin of the Abistada Lake, in Western Afghanistan, a region which is classic ground, and is the scene of many of the ancient Persian tales related in the pages of Ferdosi. Mr. Markham gave some interesting particulars respecting the formation of the river of Ghazni, which drains the eastern half of the remarkable isolated basin of Lake Abistada, on the east side of the western Sulimani Range. He afterwards read a paper by Lieut.-General Kaye, on the mountain passes leading to the valley of Bamian, based on that officer's recollections of his visit to the region north of Kabul, nearly forty years ago, supplemented by notes made at the time. With regard to the idols of Bamian, the limit of his journey, General Kaye mentions a curious fact, viz., that between the images and at their sides, peeping over their shoulders—and some even above their heads—were many caves in the cliff-side on which they are cut, having intricate connecting approaches and galleries cut within the rock; these formed dwellings for many Bamianchis and also for some camp-followers of the British. The two papers were illustrated by the large diagram of Afghanistan which has just been constructed for the Society.

THE London Missionary Society have received letters down to October 17 from their mission at Ujiji, on Lake Tanganyika, which announce the death from apoplexy of the Rev. Mr. Thomson, the leader of the party after the Rev. Roger Price's departure. The Arabs, though well disposed, refuse to allow the missionaries to settle away from Ujiji. Mr. Hore, the scientific member of the mission, has taken several observations with the view of settling the position of Ujiji.

IN consequence of the prevailing ignorance on the subject, Mr. E. F. im Thurn, of Demerara, has begun to draw up some notes on the Indians of Guiana. In the first instalment he remarks that the main tests by which to distinguish the various tribes are language, geographical position, physical features, and customs, as expressed in their characters, habits, and legends. In applying these tests to the tribes of British Guiana he thinks it best to look first at their geographical position. British Guiana consists of three regions—the coast region nearest the sea, within that the forest region, and within that again the savannah region, which passes without break into the great savannahs of Brazil. The coast region, in the north, towards the sources of the Orinocco, is inhabited by the Warau Indians, and further south by the Arawacks, while here and there are settlements and families of the Caribisi, a term which appears to be not strictly synonymous with Caribs. The forest region is almost entirely inhabited by the Ackawois, with a very few Carabisi settlements scattered among them. The Savannah region is peopled by a large number of tribes. Beginning from the north towards the Orinocco, the chief are the Arecunas, Macusis, Wapianas, and Atorais. Further south are the Tarumas and Woiowais, and the small remnants of the Maopityans, or Frog Indians, and the Pianoghottos. Here and there travellers report the existence of other tribes, but these Mr. im Thurn maintains to be groups of hybrids between two tribes. Of the Maopityans and the Pianoghottos nothing beyond a few details as to their peculiar personal appearance and manners is known, and of the Woiowais only the name is known. Mr. im Thurn next dwells at some length on the linguistic peculiarities of the other tribes mentioned, excepting the Tarumas, and he afterwards describes the way in which they most probably came into the positions they now occupy.

THE statue of Captain Cook, which has been erected in the Hyde Park, Sydney, was unveiled on February 25. The ceremony, which was of an imposing character, was performed by the Governor, Sir Hercules Robinson, in the presence of the Ministry, the public bodies of the city, detachments of the naval and military forces, and upwards of 20,000 spectators.

DR. EDWIN R. HEATH, of whose proposed exploration in South America we have already made mention, left New York on November 23, and reached Pará on December 19. He was to have taken a steamer up the Madeira on the 23rd, and thence to Exaltacion or else across to Reyes, on the Beni River, where he proposed to spend some time in making collections and the necessary preparations for descending the river. He expected to obtain Greenwich time at San Antonio, the latitude and longitude of which is well established, and to work his longitudes by them until arriving at the Beni, where he intended to correct it by "lunars." With a good outfit and apparatus he was to take observations of latitude and longitude at every suitable opportunity, as also to make records of the thermometer, barometer, and boiling-points. After completing all his preparations at Reyes, and having his *balsa* properly constructed and equipped, he proposed to commit himself to the current, and take his chances of what might happen.

THE Minister of Public Instruction at Paris has received an interesting communication from Dr. Jules

Crevaux (see NATURE, vol. xix. p. 298), written on October 30 from the River Kou, an affluent of the Yary, one of the lower tributaries of the Amazon. When he last wrote he had just crossed the crest of the Tumuc-Humac range. The Rouassir, an affluent of the Kou, was at length reached, after many difficulties, on September 27, but proved to be navigable for less than 250 yards; its course then led through a marshy country, in which it was divided into numerous streams, encumbered with a virgin vegetation, which had to be cut through. The party only reached the confluence of the Kou on December 2. Here Dr. Crevaux met some members of the Roucouyenne tribe whom he had seen before, and who were journeying towards the Oyapock River, in Guiana. They took his letters and some of his collections, while a few of them undertook to remain with him and guide him to the Yary, and thence to the Paru.

AT a recent meeting of the Geographical Society of Lyons, Capt. Baudot read a report on M. Duponchel's project of a railway from Algeria to the Senegal. He characterised the scheme as a dream and an illusion, and basing his remarks on his experiences gained during a long sojourn in Algeria, he enumerated the difficulties which rendered the project incapable of realisation in our time.

WE have received the first part of a new edition of Stieler's well-known Hand-Atlas, published by Perthes, of Gotha. A large number of new maps are promised; one of these, North-West Africa, is contained in the first part, and seems to us to be well up to date. It is only four years since the last edition was published, but much has happened during the interval to render a new one necessary.

A NEW Society of Geography has been established at Nancy, the head town of French Lorraine, and the first meeting took place on February 24. Another Society of Geography for Normandy has also been found at Rouen.

AT a recent meeting of the Berlin African Society, the sum of 2,000 reichsmark was awarded to the well-known traveller, Herr Ad. Krause, who is now in Northern Africa, for a special tour to the Ahaggar mountain range. A further sum of 20,000 mark will be distributed amongst several other travellers shortly. In the next part of the Society's *Mittheilungen*, interesting reports just come to hand from Drs. Gerhard Rohlfs and Buchner, will be published.

IT is stated that Major Butler, of the 9th Regiment, has returned to India from Turkestan, after completing a survey of nearly 6,000 miles of the country. In the course of his explorations he visited and held a conference with the Tekké Turcoman chiefs at Kizil Arvad, which was afterwards occupied by the Russians, but from which it is said that General Lomakin has found it necessary to beat a retreat.

IN the list of observations for fixing positions on the Amazons, taken by Commander Selfridge, U.S.N., which were recently published in the *New York Herald*, we learn that by an accidental error the longitude of Pará was given as 48° 59′ 15″, instead of 48° 29′ 15″, and that the latter will have to undergo a correction of 50″ for the difference between the meridians of the Rio de Janeiro Observatory and Fort Villegagnon, the distance having been erroneously calculated from the latter.

UNDER the title of "L'Amérique Equinoxiale" M. Ed. André has just commenced, in the *Tour du Monde*, a series of admirably illustrated papers on the United States of Colombia, Ecuador, and Peru, in which he travelled on a scientific mission from the French Government in 1875–6.

NOTES

FREDERICK SMITH, F.L.S., assistant-keeper of the zoological department of the British Museum, died on the 16th inst. at the age of seventy-three. Mr. Smith had devoted himself to entomology, and was one of the first living authorities on hymenopterous insects.

THE first *soirée* given at the Paris Observatory by Admiral Mouchez took place on February 21, and was very successful. More than a thousand persons belonging to influential circles visited the scientific exhibition of telephones, microphones, electric pens, Feil's new specimens of artificial gems, &c. A lecture was given by M. Wolf illustrating a new projection by electric light; the revolution of a radiometer could be observed for the first time on a screen. Admiral Mouchez had secured the services of the band of the Republican Guard, and a ball terminated the proceedings. Science seems to be somewhat more human and gay in Paris than in London; we do not think this does any harm to science, and is certainly a very effectual method of spreading an interest in it.

ENCOURAGING experiments were made at the British Museum on Tuesday night in lighting up the reading-room by means of the electric light. To-night further and more complete experiments are to be made, and we trust that as the result a considerable extension of working-hours will be possible for the hundreds who make the great room their daily workshop. A week or two ago the enterprising authorities of the Dundee public library made similar experiments with hopeful results. For such purposes there can be no question of the utility of the light, if suitable and safe arrangements could be made. The form of light used at the Museum was the modification of the Jablochkoff candle introduced by the Société Générale d'Électricité. By way of experiment the electric light has recently been introduced into the Vienna Art Exhibition at the "Künstlerhaus" and has enabled the directors to keep open their picture galleries until late at night. The experiment was a perfect success, and the new light will remain permanently established at the galleries.

THE subjects of the Croonian Lectures for this year are announced. Lecture I. will be on the physical basis of auscultation, Lecture II. on tension, Lecture III. on the rate of the heart's hypertrophy. All the lectures will be illustrated by means of physical experiments and oxyhydrogen projection. The lecturer is Dr. W. H. Stone.

ON Wednesday, March 5, Prof. Flower will give the first of his nine lectures at the College of Surgeons, on the comparative anatomy of man, in continuation of the course of last year, to be continued on Mondays, Wednesdays, and Fridays, at 4 o'clock, to March 24. The following are the heads of Prof. Flower's lectures:—Recapitulation of the best ascertained facts in connection with the subjects treated of in the last course, including the physical characters and geographical distribution of the Australian, Tasmanian, Melanesian, Papuan, Malay, and Polynesian races, with further illustrations from recent additions to the Museum; the inhabitants of the Andaman Islands, briefly touched upon last year, will next be treated of in detail, as typical examples of the Negrito race, and their osteological characters and relations to other races demonstrated from a series of skeletons and crania lately received; the Mongolian type and its various modifications, illustrated as far as the materials in the Museum permit; ethnology of Eastern and Southern Asia; the Ainos, a non-Mongolian people of Northern Japan; the Eskimos. The lectures are free to all who are interested in the subject.

A NEW society has been created at Paris for aëronautics. It is styled "Académie d'Ascensions météorologiques," and a

museum is being fitted up and will be opened in March for public inspection, admittance free. It contains all the apparatus devised for constructing or directing balloons or taking meteorological observations in the air.

We regret to see from the *Times* correspondent at Daka, that notwithstanding all that science has done for warfare, the Afghan war has been an unscientific one. "India," he states, "is unprepared for scientific war." "The enemy like ourselves possesses arms of precision and artillery. Their artillery at Ali Musjid and the Peiwar Kotal was probably equal if not superior to our own. Rockets, however, so easily carried over rough mountain roads, and so terrifying to barbarians from their eccentric course, exciting their astonishment more than any other appliances of art except the telegraph, have never been introduced in the campaign. Nor have any steam launches been sent to traverse the navigable Cabul river, explore its windings, and secure the left flank from gatherings of the enemy. No lime-lights or other lights, of which science boasts so many, have ever been supplied to prevent the enemy perpetually harassing our troops and disturbing their much-needed repose by creeping within range under cover of night and firing into our camp." This is rather disheartening, and we trust that in the campaign against the Zulus more attention will be paid to recent applications of science, and that for example night surprises will be made impossible by the use of one or other form of light which by a little ingenuity might be made to light up all the ground around any position.

An unusually brilliant meteor was seen in the north of England on Monday morning at about twelve minutes to three o'clock. It is described as a pear-shaped ball of fire in the northern heavens, which travelled slowly downwards towards the horizon, and emitting scintillations and a light of great brilliancy almost equal to that of day, so great indeed that it is said the smallest print could have been read. The light having disappeared, a sound, described by some as resembling the discharge of heavy cannon, and by others as that of the rumbling of distant thunder, was heard, but in all cases it seems to have been sufficiently violent to rattle windows, &c., and to have raised various speculations as to what could be the cause, some ascribing it to an earthquake, others to lightning, while others who saw the meteor set it to the account of that unearthly visitor.

A smart shock of earthquake, lasting about four seconds, was felt at North Unst Lighthouse, Shetland, on January 4, at five minutes past one o'clock in the afternoon. At 7.45 A.M. on Sunday week, a smart shock was felt at Milan and Brescia.

A simple and convenient way of demonstrating the vibratory movements of chords, is described by M. Schwedoff in a recent number of the *Journal de Physique*. The stationary waves of the chord are produced by means of an electric trembler (like that used for bells), the chord being attached at one end to the soft iron palette, and caused to vibrate transversely. The other end of the chord is attached to a stretcher for varying the tension. A movable runner allows of varying the length of the vibrating portion. A blackened board with figures on it is supported behind the chord.

By last accounts from China we learn that Mr. C. Moreno, the agent of the American projectors of a scheme for connecting China with the west coast of America by a submarine cable, is now at Tientsin for the purpose of soliciting the support of the Chinese Government. The Japanese are said to have promised assistance if the project finds favour at Peking.

An interesting archæological observation has recently been been made quite accidentally. It is well known that the urns found on Roman burial-grounds and containing the bone remains of cremated bodies are often covered with clay cups or dishes. The object of these dishes was supposed to have been to contain spices, which sent forth agreeable odours during the progress of the cremation. Herr Dahlem, a well known German archæologist, was able to verify this view in the following manner. He had obtained a dish of this kind which was broken, and after cementing it had placed it upon a stove for the purpose of drying the cement. Shortly afterwards he noticed a strong and by no means unpleasant odour proceeding from the heated dish. It seems therefore that the ingredients burnt in the dish some fifteen centuries ago had left traces behind, which announced their presence upon becoming heated. Herr Dahlem remarks that the odour was not unlike that of storax.

Prof. C. V. Riley, entomologist of the U.S. Department of Agriculture, reports that serious complaints have come from the Pacific slope during the year of a new insect that is killing many of the orchard and ornamental trees in that section of the country. Specimens received from Mr. A. W. Saxe, of Santa Clara, California, show it to be a species of *Dorthesia*, an abnormal bark-louse (family *Coccidæ*). It is an Australian insect (apparently *D. characias*, Westw.), and has of late years been introduced on Australian plants into South Africa, where, according to Mr. Roland Trimen, curator of the South African Museum, it has multiplied at a terrible rate, and become such a scourge as to attract the attention of the Government. It has evidently been introduced (probably on the blue gum or eucalyptus) to California, either direct from Australia or from South Africa, and will doubtless become a great evil, because most introduced insects are brought over without the natural enemies which keep them in check in their native country, and consequently multiply at a prodigious rate. The best remedy is a judicious use of kerosene or linseed-oil.

In the *Revue d'Anthropologie* for January, 1879, Dr. Gustave Le Bon contributes an important memoir, "Recherches anatomiques et mathématiques sur les Lois des Variations du Volume du Cerveau et sur leurs Relations avec l'Intelligence." One of the most interesting of his conclusions is that relating to the cranial differences between the sexes, both in weight of brain and dimension of skull. This question has long been studied by English anthropologists, and recently conclusions have notably been drawn by Mr. Darwin in his chapter on "Difference in the Mental Powers of the Two Sexes," in his "Descent of Man," and collaterally by Mr. Fras. Galton, in his work on "Hereditary Genius." Dr. Le Bon states as the result of his investigations that the female brain is not only less in weight than that of man, but that this inferiority exists "à âge égal, à poids égal et à taille égale," and that the cranial differences between the sexes are greater among the cultivated and more highly-developed races than among those in the most primitive condition, which he ascribes to the fact that the mental activity of civilised society is conducted in the aggregate by the male sex. These conclusions are highly corroborative of the views advanced in a paper read before the Anthropological Institute of London in 1874 (Distant, "On the Mental Differences between the Sexes," *Journal* of the Anthropological Institute, vol. iv. p. 78). Dr. Le Bon points out also the important fact that though the average capacity of the crania of the superior races is much greater than that of the less-developed ones, still what really constitutes the superiority of one race over the other is that the civilised races contain necessarily more largely-developed crania than can be found among the inferiorly developed races. As regards a strong opinion often held as to the superiority in size of the left hemisphere of the brain over that of the right, Dr. Le Bon can add nothing in confirmation. From a measurement of 287 crania he found inequalities indicated on either side in such proportion as to preclude his describing them as anything but a variable character: "sans qu'il soit possible d'assigner des raisons sérieuses à cette inégalité de développement."

ONE of the most interesting questions in American archæology has long been that of the age of the "mound-builders." Modern views seem now opposed to a prehistoric date for these people. Amongst other American workers who have inclined to the more recent date of these structures may be mentioned S. F. Haven, who considered the ancestors of the present Indians to have been the authors of these erections, and Dr. P. J. Farnsworth, who believed that the mound-builders were identical in race with the historical Indians of North America. On this subject a paper read before the Congrès International des Américanistes, 1877, by M. F. Force, has just been reprinted in pamphlet form by Clarke and Co., Cincinnati, 1879, entitled "To what Race did the Mound-builders belong?" The following are some of the author's conclusions:—That so far as indications are given by the growth of vegetation, it is not necessary to hold that any of the works were abandoned more than one thousand years ago. That the absence of all tradition concerning the mounds among the recent Indians is no proof of their great antiquity, as Indian tradition is short-lived and evanescent. Although the advent of De Soto with his armed followers, pillaging and ravaging the country, must have been calculated to make a deep impression, yet, when Europeans visited the country a century and a half later, they found not a vestige of a tradition of De Soto. Finally, Mr. Force considers that the mound-builders were tribes of Indians, more advanced than the Algonquins or the Dakotahs, but much less advanced than the Aztecs or the Peruvians, and on the same plane with the Pueblo Indians, and that they were living in full prosperity in the time of Charlemagne. Mr. Force reviews the evidence as to their antiquity derived from an examination of crania from these mounds, and endeavours to prove that either the skulls were not obtained from the mounds under consideration, or in other instances would not bear the conclusions based on their examination.

A CURIOUS thermo-magnetic motor, devoid, probably, of practical value, but having some scientific interest, has been devised by Prof. Houston and Thomson (*Journal* of the Franklin Institute). A disk or ring of thin steel is mounted on a vertical axis so as to be quite free to move with its edges opposite the poles of a horse-shoe magnet. The wheel of course becomes magnetised by induction. On heating a section of it, it begins to move. The reason is, that the heated section has its coercitive force increased, and so, being less powerfully magnetised by the induction of the adjacent pole, than the part next it, the attraction exerted by the pole on this latter part is sufficient to cause motion. A constant source of heat gives continuous rotation. The disk must be sufficiently thin to prevent its acquiring a uniform temperature. The heat may be applied at diametrically opposite parts, with similar effect. What renders the motor of little value is the amount of heat required being so enormous as compared with the force developed.

THE *Monats-Berichte der Berliner Akademie* (September and October 1878) contain some researches by M. Paalzow on the spectrum of oxygen: as, however, M. Paalzow has fallen into the old and common mistake of taking the spectrum of carbonic oxide for the spectrum of oxygen, he cannot be said to have made any decisive addition to our knowledge.

A CEYLON paper furnishes some interesting notes respecting the culture of the cinchona tree in the island. The variety known as *C. succirubra* yields a large quantity of bark, and is so hardy that, though the proper zone of elevation for its culture is from 2,000 to 4,500 feet, many planters are induced to try it at higher elevations. The zone for *C. officinalis* is from 4,000 feet upwards, and it has been grown on Dodabetta at a height of 8,000 feet, but in that case the bark of the unmossed tree becomes covered with lichens. *C. calisaga* will grow wherever the last-named variety does. *Cinchona officinalis* is highly recommended as a sheltering tree for coffee-shrubs against the effects of wind.

THE first number of the *Journal* of the Russian Physical and Chemical Society (both societies now united) contains an interesting paper by M. Beketoff on the specific heat of hydrogen when compounded with palladium; a paper by M. Ponomareff on derivatives from uric acid; additions and corrections, by Prof. Menschutkin, to his papers on etherisation of secondary alcohols; two papers on organic chemistry by MM. Kiabinine, Saytzeff, and Semlianitzin; and a paper by M. Lermantoff on the chemical and photographic action of light. Many very interesting notes give a very complete account of recent work in chemistry.

THE formation of hail and the various causes which contribute to it are still a very obscure question in meteorology. The following points on which information is desired have lately been indicated by M. Colladon:—1. Dates as exact as possible, and made comparable with the hour at Paris, Berne, or Geneva, of the commencement and end of the hail shower; extent of the surface covered. 2. Average and maximum dimensions of hailstones, their form, the average or maximum number of layers they present. Do the successive layers increase in thickness from the central nucleus? 3. Apparent form and elevation of hail clouds; have they the appearance of a vast continuous gyratory movement, or simply of movements of attraction and repulsion? Multiplicity of flashes, their average number per minute; are they, or not, accompanied by resounding noises and frequent descents of lightning on the ground? or are they mostly mute? Are there notable falls of hail without apparent and well-marked electrical phenomena? 4. Average temperature of the air before or during an electric storm, and temperature of the rain-water accompanying it, at the moment of its fall. M. Colladon has contrived an inexpensive apparatus for measuring the last item (*La Nature*, February 15). A funnel conducts the rain to a capsule holding the bulb of a minimum thermometer which has the upper part of its stem bent horizontally and a scale attached to this part.

IN a note on brewing contained in a report on Sapporo and Ishcari (Japan) we read that the beer is poor, weak stuff that will not keep. In course of time, however, it is fully expected that the art of brewing will succeed, more especially as a native director has spent several years in America and Europe devoting his attention to brewing. The hops used, it seems, are imported, and foreign hop seed has been sown, the plants raised from which appear to be doing well. The wild hops, which are found in great abundance on the road from Morarau to Sapporo, and have been found to be unsuitable for brewing in their wild state, are now being cultivated, as it is supposed that by care and attention they will prove to be as good if not better than foreign hops. Consequently great pains are now being taken with these hop plantations.

THE additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. Douglas Murray; a Bennett's Wallaby (*Halmaturus bennettii*) from Tasmania, presented by Mr. W. E. Windus; a Common Hare (*Lepus europæus*), British Isles, presented by Mr. Alfred Withers; two Indian Barred Doves (*Geopelia striata*), a Chinese Turtledove (*Turtur chinensis*) from India, presented by Capt. H. Braddick; a Tayra (*Galictis barbara*) from Panama, two Grand Galagos (*Galago crassicaudatus*) from East Africa, three Australian Wild Ducks (*Anas superciliosa*) from Australia, two Cardinal Grosbeaks (*Cardinalis virginianus*) from North America, purchased; a Spotted Eagle Owl (*Bubo maculosus*) from Africa, deposited; two Prairie Marmots (*Cynomys ludovicianus*) from North America, received in exchange.

ON A NEW CHEMICAL INDUSTRY ESTABLISHED BY M. CAMILLE VINCENT[1]

"AFTER I had made the discovery of the *marine acid air*, which the vapour of spirit of salt may properly enough be called, it occurred to me that, by a process similar to that by which this *acid* air is expelled from the spirit of salt, an *alkaline* air might be expelled from substances containing the volatile alkali. Accordingly I procured some volatile spirit of sal-ammoniac, and having put it into a thin phial and heated it with the flame of a candle, I presently found that a great quantity of vapour was discharged from it, and being received in a basin of quicksilver, it continued in the form of a transparent and permanent air, not at all condensed by cold."

These words, written by Joseph Priestley rather more than 100 years ago, describes the experiment by which ammonia was first obtained in the gaseous state. Unacquainted with the composition of this alkaline air, Priestley showed that it increased in volume when the electric sparks are passed through it, or when the alkaline air (ammonia) is heated, the residue consists of inflammable air (hydrogen). Berthollet, in 1788, proved that this increase in bulb is due to the decomposition of ammonia into nitrogen and hydrogen, whilst Henry and Davy ascertained that two volumes of ammonia are resolved into one volume of nitrogen and three volumes of hydrogen.

The early history of sal-ammoniac and of ammonia is very obscure. The salt appears to have been brought into Europe from Asia in the seventh century, derived, possibly, from volcanic sources. An artificial mode of producing the ammoniacal salts from decomposing animal matter was soon discovered, and the early alchemists were well acquainted with the carbonate under the name of *spiritus salis urinæ*. In later times sal-ammoniac was obtained from Egypt, where it was prepared by collecting the sublimate obtained by burning camel's dung.

Although we are constantly surrounded by an atmosphere of nitrogen, chemists have not yet succeeded in inducing this inert element to combine readily; so that we are still dependent for our supply of combined nitrogen, whether as nitric acid or ammonia, upon the decomposition of the nitrogenous constituents of the bodies of plants and animals. This may be effected either by natural decay giving rise to the ammonia, which is always contained in the atmosphere, or by the dry distillation of the same bodies, that is by heating them strongly out of contact with air, and it is from this source that the world derives the whole of its commercial ammonia and sal-ammoniac.

Coal—the remains of an ancient vegetable world—contains about 2 per cent. of nitrogen, the greater part of which is obtained in the form of ammonia when the coal undergoes the process of dry distillation. In round numbers 6,000,000 tons of coal are annually distilled for the manufacture of coal-gas in this country, and the ammoniacal water of the gas-works contains the salts of ammonia in solution.

According to the most reliable data 100 tons of coal when distilled so as to yield 10,000 cubic feet of gas of specific gravity 0·6, give the following products, in tons:—

Gas.	Tar.	Ammonia Water.	Coke.	
22·25	8·5	9·5	59·75	Average.

This ammonia-water contains about 1·5 per cent. of ammonia; hence the total quantity of the volatile alkali obtainable from the gas-works in England amounts to some 9,000 tons per annum.

A singular difference is observed between the dry distillation of altered woody fibre as we have it in coal—and woody fibre itself. In the products of the first operation we chiefly find in the tar the aromatic hydrocarbons such as benzene, whilst in the second we find acetic acid and methyl-alcohol are predominant.

The year 1848 is a memorable one in the annals of revolutionary chemistry, for in that year Wurtz proved that ammonia is in reality only one member of a very large family. By acting with caustic potash on the nitriles of the alcohol radicals, he obtained the first series of the large class of compound ammonias, the primary monamines. Of these, methylamine is the first on our list:—

$$\left.\begin{matrix}CH_3\\CO\end{matrix}\right\}N + 2KOH = \left.\begin{matrix}CH_3\\H_2\end{matrix}\right\}N + CO\left\{\begin{matrix}OK\\OK\end{matrix}\right.$$

The years that followed, 1849-51, were prolific in ammoniacal discovery. Hofmann pointed out that not only one atom of hydrogen in ammonia can be replaced by its equivalent of organic radical, but that either two or all of the three atoms of the hydrogen in ammonia can be likewise replaced, giving rise to the secondary and tertiary amines by the following simple reactions:—

$$1.\quad CH_3I + \left.\begin{matrix}H\\H\\H\end{matrix}\right\}N = HI + \left.\begin{matrix}CH_3\\H\\H\end{matrix}\right\}N$$

$$2.\quad CH_3I + \left.\begin{matrix}CH_3\\H\\H\end{matrix}\right\}N = HI + \left.\begin{matrix}CH_3\\CH_3\\H\end{matrix}\right\}N$$

$$3.\quad CH_3I + \left.\begin{matrix}CH_3\\CH_3\\H\end{matrix}\right\}N = HI + \left.\begin{matrix}CH_3\\CH_3\\CH_3\end{matrix}\right\}N$$

To these bodies the names of methylamine, dimethylamine, and trimethylamine were respectively given. They resemble ammonia in being volatile alkaline liquids or gases, which combine with acids to form crystallisable and well defined salts.

Hitherto, these compound ammonias have been chemical curiosities; they have, however, recently become—as has so often been the case in other instances—of great commercial importance, and are now manufactured on a large scale.

We are all well aware that the French beet-root sugar industry is one of great magnitude, and that it has been largely extended in late years. In this industry, as in the manufacture of cane-sugar, large quantities of molasses or treacle remain behind after the whole of the crystallisable sugar has been withdrawn. These molasses are invariably employed to yield alcohol by fermentation. The juice of the beet, as well as that of the sugar-cane, contains, in addition to the sugar, a large quantity of extractive and nitrogenous matters, together with considerable quantities of alkaline salts. In our sugar-producing colonies, the waste liquors or spent-wash from the still—called *vinasses* in French—are wastefully and ignorantly thrown away instead of being returned to the land as a fertiliser, and thus the soil becomes impoverished.

In France it has long been the custom of the distiller to evaporate these liquors (*vinasses*) to dryness, and calcine the mass in a reverbatory furnace, thus destroying the whole of the organic matter but recovering the alkaline salts of the beet-root. In this way 2,000 tons of carbonate of potash are annually produced in the French distilleries. More than thirty years ago the idea was entertained of collecting the ammonia-water, tar, and oils which are given off when this organic matter is calcined, but the practical realisation of this project has only quite recently been accomplished, and a most unexpected new field of chemical industry thus opened out through the persevering and sagacious labours of M. Camille Vincent, of Paris.

The following is an outline of the process as carried out at the large distillery of Messrs. Tilloy, Delaune, and Co., at Courrières. The spent-wash, having been evaporated until it has attained a specific gravity of 1·81, is allowed to run into cast-iron retorts, in which it is submitted to dry distillation. This process lasts four hours, the volatile products pass over, whilst a residue of porous charcoal and alkaline salts is left behind in the retort. The gaseous products given off during the distillation are passed through coolers, in order to condense all the portions which are liquid or solid at the ordinary temperature, and the combustible gases pass on uncondensed, and to serve as fuel for heating the retorts.

The liquid portion of the distillate is a very complex mixture of chemical compounds resembling, in this respect, the corresponding product in the manufacture of coal gas. Like this latter, the liquid distillate from the spent-wash may be divided into—1. The ammonia water; 2. The tar. The ammonia water of the vinasses resembles that of the coal-gas manufacture, in so far as it contains the carbonate, sulphydrate, and hydrocyanide of ammonia; but it differs from this (and approximates to the products of the dry distillation of wood) by containing, in addition, methyl alcohol, methyl sulphide, methyl cyanide, many of the members of the fatty acid series, and, most remarkable of all, *large quantities of the salts of trimethylamine.*

The tar, on redistillation, yields more ammonia water, a large number of oils, the alkaloids of the pyridene series, solid hydrocarbons, carbolic acid, and lastly, a pitch of fine quality.

The crude alkaline aqueous distillate is first neutralised by sulphuric acid, and the saline solution evaporated, when crystals of sulphate of ammonia are deposited, and these, after separating and draining off, leave a mother-liquor, which contains the more soluble sulphate of trimethylamine. During the process of

[1] Lecture given at the Royal Institution by Prof. Roscoe, LL.D., F.R.S., February 21, 1879. Revised by the Author.

concentration, vapours of methyl alcohol, methyl cyanide and other nitriles, are given off, these are condensed and the cyanide used for the preparation of ammonia and acetic acid by decomposing it with an alkali.

Trimethylamine itself is at present of no commercial value, though we are not without hopes that a useful employment for this substance may soon be found. The question arises as to how this material can be made to yield substances capable of ready application in the arts. This problem has been solved by M. Vincent in a most ingenious way. He finds that the hydrochlorate of trimethylamine, when heated to a temperature of 260°, decomposes into (1) ammonia, (2) free trimethylamine, and (3) chloride of methyl:—

$$3NMe_3HCl = 2NMe_3 + NH_3 + 3MeCl.$$

By bubbling the vapours through hydrochloric acid, the alkaline gases are retained, and the gaseous chloride of methyl passes on to be purified by washing with dilute caustic soda and drying with strong sulphuric acid. This is then collected in a gasholder, whence it is pumped into strong receivers and liquefied. The construction of one of these is shown in Fig. 1. They consist of strong wrought-iron cylinders, tested to resist a pressure of 20 kilos per square centimeter, and containing 50, 110, or 220 kilos of chloride of methyl. The liquid is drawn from these receivers by opening the screw tap (D), which is covered by a cap (C) to prevent injury during transit.

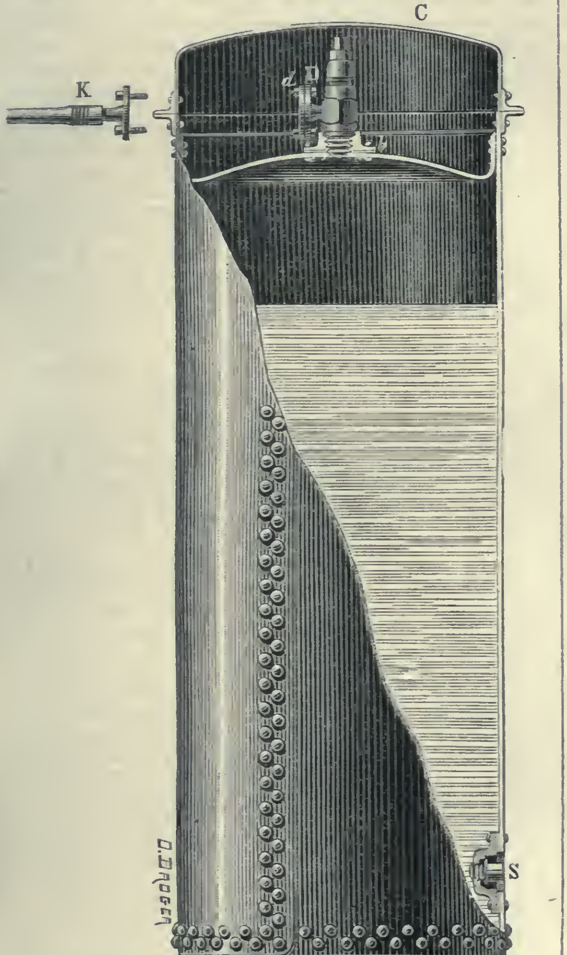

FIG. 1.

Both ammonia and chloride of methyl are, however, substances possessing a considerable commercial value. The latter compound has up to this time, indeed, not been obtained in large quantities, but it can be employed for two distinct purposes —1. It serves as a means of producing artificial cold. 2. It is most valuable for preparing certain methylated dyes, which are at present costly, inasmuch as they have hitherto been obtained by the use of methyl iodide, an expensive substance.

Methyl chloride was discovered in 1840 by MM. Dumas and Peligot, who obtained it by heating a mixture of common salt, methyl alcohol, and sulphuric acid. It is a gas at the ordinary temperature, possesses an ethereal smell and a sweet taste, and its specific gravity is 1·738. It is somewhat soluble in water (about 3 volumes), but much more in acetic acid (40 volumes), and in alcohol (35 volumes). It burns with a luminous flame tinged at the edges with green, yielding carbonic and hydrochloric acids. Under pressure, methyl chloride can readily be condensed to a colourless, very mobile liquid, boiling at −23 C. under a pressure of 760 mm. As the tension of the vapour is not high, and as it does not increase very rapidly with the temperature, the liquefaction can be readily effected, and the collection and transport of the liquefied chloride can be carried on without danger. The following table shows the tension of chloride of methyl at varying temperatures:—

At 0°	the tension of	CH_3Cl is	2·48	atmospheres.
,, 15°	,,	,,	4·11	,,
,, 20°	,,	,,	4·81	,,
,, 25°	,,	,,	5·62	,,
,, 30°	,,	,,	6·50	,,
,, 35°	,,	,,	7·50	,,

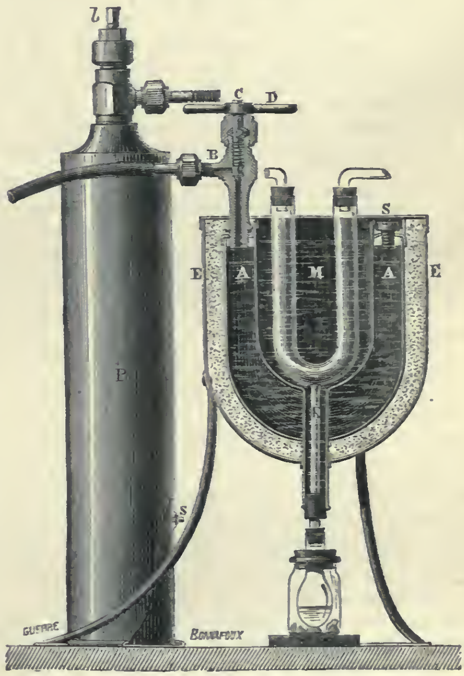

FIG. 2.

From these numbers we must of course subtract 1 to obtain the pressure which the vapour exerts upon the containing vessel.

As a means of producing low temperatures chloride of methyl will prove of great service both in the laboratory and on a larger industrial scale. When the liquid is allowed to escape from the receiver into an open vessel, it begins to boil, and in a few moments the temperature of the liquid is lowered by the ebullition below −23°, the boiling-point of the chloride. The liquid then remains for a length of time in a quiescent state, and may be used as a freezing agent. By increasing the rapidity of the evaporation by means of a current of air blown through the liquid, or better, by placing the liquid in connection with a good air-pump, the temperature of the liquid can in a few minutes be reduced to −55°, and large masses of mercury easily solidified.

The construction of a small copper receiver and of the freezing-machine employed by M. Camille Vincent is shown in Fig. 2. It consists of a double-cased copper vessel, between the two sides of which the methyl-chloride is introduced. The central space is filled with some liquid such as alcohol, incapable of solidification. The chloride of methyl is allowed to enter by the screw-tap (B), and

the screw (S) left open to permit of the escape of the gas. As soon as the whole mass of liquid has been reduced to a temperature of $-23°$ the ebullition ceases, the screw (S) may be replaced, and if a temperature lower than $-23°$ be required the tube (B) placed in connection with a good air-pump. By this simple means a litre of alcohol can be kept for several hours at temperatures either of $-23°$ or $-55°$, and thus a large number of experiments can be performed for which hitherto the expensive liquid nitrous oxide or solid carbonic acid was required.

M. Camille Vincent has recently constructed a much larger and more perfect and continuous form of freezing-machine, in which, by means of an air-pump and a forcing-pump, the chloride of methyl is evaporated in the freezing-machine, and again condensed in the cylinders. This enlarged form of apparatus will probably compete favourably with the ether- and sulphurous acid freezing-machines now in use, as they can be simply constructed, and as the vapour and liquid employed does not attack metal, is non-poisonous, and as the frigorific effects which it is capable of producing are most energetic.

The second and perhaps more important application of methyl-chloride is to the manufacture of methylated colours.

It is well known that rosaniline or aniline red, $C_{20}H_{19}N_3$, yields compounds possessing a fine blue violet or green colour, when a portion of the hydrogen has been replaced by the radicals, methyl or ethyl, and the larger the proportion of hydrogen replaced, the deeper is the shade of violet produced. Then we have triethyl rosaniline, or Hofmann's[1] violet, $C_{20}H_{16}(C_2H_5)_3N_3$.

By the replacing one or two atoms of the hydrogen of aniline by methyl, and by oxidising the methyl-aniline, Charles Lauth obtained fine violet colours, whilst about the same time Hofmann observed the production of a bright green colouring matter now known as iodine green, formed during the manufacture of the violet, and produced from this latter colour by the action of methyl-iodide.

In order to prepare aniline green from the pure chloride of methyl a solution of methyl-aniline violet in methyl-alcohol is placed in an iron digester, and the liquid rendered alkaline by caustic soda. Having closed the digester, a given quantity of liquid chloride of methyl is added by opening a tap, and the digester thus charged is placed in a water bath, heated by a jet of steam until the temperature reaches 95°, and the indicated pressure amounts to from 4 to 5 atmospheres. As soon as the reaction is complete the hot water is replaced by cold, and the internal pressure reduced by opening the screw-tap of the digester. The product of this reaction, heated and filtered, yields the soluble and colourless base, whose salts are green. To the acidified solution a zinc-salt is added to form a double salt, and the green compound is then precipitated by the addition of common salt. By adding ammonia to a solution of the methyl green salt, a colourless liquid is obtained in which cloth mordanted with tannic acid and tartar-emetic becomes dyed green (R. S. Dale).

If rosaniline be substited for methyl-aniline in the preceding reaction, Hofmann's violet is obtained. The application of methyl-chloride to the preparation of violets and greens is, however, it must be remembered, not due to M. Vincent; it has been practised for some years by various aniline colour makers. M. Vincent's merit is in establishing a cheap method by which perfectly pure chloride of methyl can be obtained, and thus rendering the processes of the manufacture of colours much more certain than it has hitherto been. By the use of th's material the aniline can be methylated in simple cast-iron boilers heated by steam, and under a pressure much more moderate than is otherwise required.

In reviewing the new chemical industry of the beet-root vinasses one cannot help being struck by the knowledge and ability which have been so successfully expended by M. Camille Vincent, on the working out of the processes. Here again we have another instance of the utilisation of waste chemical products and of the preparation on a gigantic scale of compounds hitherto known only as chemical rarities. All those interested in the progress of scientific research must congratulate M. Camille Vincent on this most successful issue of his labours.

ILLUMINATION IN SPECTROSCOPY[2]

AFTER having shown how intrinsic brilliancy of the light operated on was the chief visual step to excellence in spectroscopic observations, the author proved that the temperature of the light must be kept constant, or we might be landed in a totally different class of physical phenomena of a most confounding character.

Coming, then, practically to *flame*-spectroscopy, he described the results hitherto obtained by all the leading spectroscopists respecting the peculiar lines and bands, all of them very faint, of the blue-grey blowpipe flame of coal-gas and common air; and then showed how, by merely looking at one and the same flame *end-on*, in place of transversely, according to the usual custom, all the features hitherto chronicled may be seen some five times brighter; while many other details not dreamt of before come into view, and the temperature remains undisturbed.

Next applying the same principle to the electric-spark illumination of gas-vacuum tubes, a still greater proportional improvement was obtained. But not until the author had invented or arranged a new description of such tubes, which rendered the application of the end-on principle possible. Examples of these new tubes, as prepared lately for the author by M. Salleron, 24, rue Pavée au Marais, Paris, were exhibited; and several proofs of their superior brightness of illumination were given. The last being that in a narrow and critical region of a rather faint and difficult carbonaceous spectrum, where the Royal Society, London, has published eight lines only, and those dark ones—the new tubes showed thirty-one lines, and all of them bright ones. As yet the author had only been able to get twelve different gases thus prepared; but with such decided improvement of spectroscopic vision in every case, that he hopes so increased a demand may soon flow in upon M. Salleron, as will make it worth his while to prepare similar end-on tubes of all known volatile products; and the result can hardly but prove most favourable to the progress of spectroscopic science.[1]

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

DR. J. H. BALFOUR, Professor of Botany in the Edinburgh University, has resigned his chair, which he has filled since 1845, on account of failing health. The patronage of the Chair of Botany is vested in the curators of the University. Among the candidates for the chair, we learn, are Mr. Carruthers, Prof. Dickson, of Glasgow, Mr. J. Bailey Balfour, and Prof. McNab, of Dublin.

THERE was much fine talk last Wednesday at the Mansion House on the subject of University extension in London, and it was pleasing to see a prince take an apparently genuine interest in the intellectual advancement of the people. We sincerely hope that the movement may lead to a substantial and durable result, though we very much doubt it. What we want most in London is a true university after the German model, not a "Cambridge extension." We are glad that Prince Leopold, in his really able address at the Birkbeck Institution on Tuesday, insisted so strongly on the weak point of the British workman, and that he can only hope to hold his own by the side of the foreign workman by starting with an equally good education.

AT the annual meeting of the trustees of the Birmingham Science College, under the presidency of the founder, Sir Josiah Mason, who celebrated on Monday his eighty-fourth birthday, it was announced that the college building, a handsome Gothic structure in the rear of the Birmingham Town Hall, is rapidly approaching completion, and the formal opening will probably be made on the founder's next birthday. Nearly 150,000*l*. has been expended on the college building and endowment.

SCIENTIFIC SERIALS

American Journal of Science and Arts, February.—We have here two interesting papers on acoustics. Mr. Jacques has inquired into the velocity of loud sounds, measuring the velocity at different short distances from a cannon by means of a series of membranes electrically connected with a chronograph. He finds that the velocity of sound is a function of its intensity, and that experiments in which a cannon is used contain an error, probably due to the bodily motion of the air near the cannon. Immediately in the rear of the cannon the velocity was less than at a distance, but, going from the cannon, the velocity rose to a maximum considerably above the ordinary velocity, and then fell gradually to the rate usually received. When the cannon was pointed

[1] Hofmann, *Proc.* Roy. Soc., xiii. 13 (1863).

[2] Abstract of paper upon "End on, in Place of Transverse, Illumination in Private Spectroscopy," by Mr. Piazzi Smyth, Past President of R. Sc. Soc. Arts. Edinb., February 10, 1879.

[1] Tubes similar to those referred to are already well-known in England; Dr. Monckhoven has in fact pointed out the value of such tubes, and sent specimens to several observers in England.—ED.

at right angles to its first position, the maximum velocity came nearer it, and had the cannon been pointed towards the membranes, the retardation would probably have become an acceleration.—Dr. Ihlseng has measured the velocity of sound in wood, both by Kundt's method and the graphic method, in which latter a blackened glass plate was drawn rapidly in a horizontal direction (across the direction of the rod's length), by a falling weight, and a pen attached to the rod wrote its vibration on the plate, which also registered those of a tuning-fork. This method was found one of considerable accuracy. Its results were slightly below those by Kundt's method, and (when the plate was moved in a different direction) it demonstrated the existence of transverse along with longitudinal, and gave their ratios.—Mr. Pumpelly writes on the relation of secular rock disintegration to lœss, glacial drift, and rock basins; Mr. Fontaine continues his notes on the mesozoic strata of Virginia; and Mr. Hawes describes a group of dissimilar eruptive rocks in Compton, New Hampshire.—In a method of determining dip, devised by Mr. Hodges, a compound bar is used consisting of two joined at right angles at a point near their ends; when placed so that the two branches make equal angles with the line of dip, the two joined poles neutralise each other, and any needle suspended near that point is unaffected. A slight turning of the compound bar renders the field round the needle a north or a south, and the needle moves correspondingly.—Some notices of recent American earthquakes are furnished by Prof. Rockwood, Jun.

Journal de Physique, January.—This number opens with M. Joubert's researches on the rotatory power of quartz and its variations with the temperature; they prove that quartz constitutes a thermometer of extreme sensibility.—A new and simple regulator of velocity, for electric motors especially, described by M. Deprez, rests on the principle of centrifugal force acting on one end (loaded) of a flexible metallic strip, and (with a certain intensity) breaking contact of an adjusting screw at that end with a metallic piece, and so stopping a current which actuates the motor.—M. Witz studies the thermal effects of the walls of an inclosure on the gases it contains.—M. Pellat describes an apparatus for effecting the synthesis of compound colours, the very colours of the spectrum being taken in the desired proportions and mixed completely: and M. Schwedoff one for studying the vibratory movements of cords, the cord being set in motion by means of an electric trembler at one end.

THE *Sitzungsberichte der naturwissenschaftlichen Gesellschaft Isis in Dresden* (1878, part 1, January to July) contain the following papers of interest:—On Heligoland and Norderney from a geological point of view, by C. D. Carstens.—On the tertiary flora of the Klein Purberg near Czernowitz, by Herr Engelhardt.—On amber, by Dr. O. Schneider.—On the saltpetre and the guano from the Atakama Desert, by Herr Weis.—On fossil man, by Herr Engelhardt.—On some prehistoric remains discovered on the Hradischt in Bohemia, by W. Osborne.—On palm leaves, by Herr von Biedermann.—On the largest elm tree of Germany, by C. F. Seidel. The tree in question stands in the village of Schimsheim near the railway station of Armsheim in Rhenish Hessen, and measures 30 metres in height, its trunk 15·07 metres in circumference, while its age is estimated at close upon 600 years.—On the colour which a gas flame assumes in the vapour generated by a mixture of permanganate of potash and sulphuric acid, by Carl Bley.—On the formation of crude coralline and of resorcine-oxalëine, by Dr. Schmitt.—On the action of ethyl mercaptan on diazo-compounds, by the same.—On a curious occurrence of tape-worms, by Herr Ebert.—On a Chinese work on insects, by C. F. Seidel.—On the origin of organic life upon the earth, by Dr. Steil.—On the red gneiss near Freiberg in Saxony, by Dr. Geinitz.—On the natural history of the Caucasus countries, by Dr. Oscar Schneider.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 30.—"On the determination of the Rate of Vibration of Tuning Forks," by Herbert McLeod, F.C.S., and George Sydenham Clarke, Lieut. R.E. Communicated by Lord Rayleigh, F.R.S.

The paper contains a description of some experiments made with a view to determine the absolute pitch of tuning forks by means of a method proposed by the writers in a previous paper (*Proc.* Roy. Soc., 1877, xxvi. 162).

It commences with a description of the time-measurer adopted, consisting of a compensated pendulum, worked by electricity, the impulse being given by a driver depending for its action on gravity alone. The pendulum is arranged to give second contacts, driving a clock-wheel with sixty teeth. This wheel has a platinum pin giving minute contacts, but it is used merely as a switch, the circuit being closed by the pendulum itself. The current works a relay, and closes the circuit required.

The tuning-fork apparatus consists of a brass drum resting on friction-wheels, and driven by a weight and train. Uniformity of motion being of great importance, an air-regulator, consisting of a fan inclosed in the lower compartment of a cylindrical box, is employed. By means of a diaphragm and vanes the fan can be made to do more or less work by pumping air from the lower into the upper compartment. The fan spindle carries a pulley driven by a thread passing round the drum.

Round one end of the drum are wrapped strips of paper on which white equidistant lines have been so ruled that they are parallel to the axis of the drum when the strips are in position. The strip most frequently used has 486 lines round the complete circumference of the drum. Opposite this graduated strip is placed a microscope with its axis horizontal. In the substage is placed a 2″ objective, producing an image of the graduations at the focus of the object-glass of the instrument. At the common focus of the two lenses is placed the tuning-fork, the stem of which is held vertical in a vice. The fork is partially inclosed in a glass case, and is so adjusted that the image of one of its limbs seems to cut the image of the graduations at right angles. The fork is set in motion by a suspended double-bass bow. If, when the fork is in vibration the drum is made to rotate with such a velocity that one of the graduations passes over the interval between two adjacent graduations in the time of one vibration of the fork, a stationary wave is seen of length equal to the length of that interval. To determine the number of vibrations of the fork in a given time, it is only necessary, therefore, to be able to count the number of graduations which pass in that period. As a perfectly uniform rotation has not been obtained, a regulator under the control of the operator is employed. This consists merely of a piece of string which passes round the axis of the drum, and also round a pulley which can be turned by the operator's left hand. An upward or downward motion of the wave denotes that the drum is going too fast or too slow, and by means of the pulley a gentle check or acceleration sufficient to keep the wave steady is given to the drum.

An electric counter gives the number of complete revolutions accomplished by the drum in any given period, and a fine-pointed tube, containing magenta, is carried by a saddle above the drum, and being actuated by an electro-magnet, makes a dot on a piece of white paper wrapped round the drum at the beginning and end of the experiment. The distance apart of these dots gives the additional fraction of a revolution accomplished by the drum during the period of the experiment. Electric circuits are so arranged that a reverser turned a few seconds before the minute at which it is intended to begin the experiment, causes a current to be sent exactly at that minute by the clock relay, which starts the electric counter, and also makes a dot on the drum. Just before the expiration of the last minute of the experiment the reverser is turned in the opposite direction, and at the expiration of that minute the counter is stopped, and a second mark made on the drum.

Some of the results obtained with different forks are given. The results of further experiments made to determine the effect of temperature, of continuous and intermittent bowing, and of the mode of fixing the fork are appended.

An optical method by which two slightly dissonant forks may be compared without altering the period of either, is described.

Figures and diagrams fully explaining the apparatus employed accompany the paper.

February 13.—"Note on the Development of the Olfactory Nerve and Olfactory Organ of Vertebrates." By A. Milnes Marshall, M.A., D.Sc., Fellow of St. John's College, Cambridge. Communicated by W. S. Savory, F.R.S., Surgeon to, and Lecturer on Surgery at, St. Bartholomew's Hospital.

In the course of an investigation into the development of the cranial nerves of the chick, certain facts came to light indicating that the olfactory nerve, instead of being, as usually described, a structure differing totally in its mode of origin from all the other nerves in the body, in reality "exactly corresponds in mode of development with the other cranial nerves, and with the posterior roots of the spinal nerves." [1]

[1] *Proc.* Roy. Soc, March 8, 1877, p. 50, and *Quarterly Journal of Microscopical Science*, January, 1878, pp. 17-23.

The present paper contains the results of further investigations on this point; it deals also with some features in the development of the vertebrate olfactory organ, and with certain questions of a more general nature affected by the conclusions arrived at.

The Development of the Olfactory Nerve

The olfactory nerve of an adult vertebrate is usually described as consisting of three parts—a proximal *tractus olfactorius*, an intermediate *bulbus olfactorius*, and a distal *nervus olfactorius*, connecting the bulb with the olfactory organ. Of these parts the two former are commonly said to arise as a hollow diverticulum of the cerebral hemisphere—the so-called olfactory vesicle or olfactory lobe. The third part, the *nervus olfactorius*, is described as arising at a later stage either from the olfactory lobe, from the olfactory organ, or from the intervening mesoblast. In consequence of these peculiarities in its mode of development, the olfactory nerve is said not to bear the slightest resemblance to the other cranial nerve, and to be in no way comparable with them. Dr. Marshall, however, finds, from an examination of a large number of vertebrate embryos—chick, dogfish, salmon, trout, axolotl, frog, and lizard—that the *nervus olfactorius* is the first part to be developed; that it arises at the same time as the other cranial nerves and in the same manner; that it appears before the cerebral hemispheres, and consequently arises from the original fore-brain. He finds further that there is no trace whatever of an olfactory vesicle in the chick till the end of the seventh day, or in the dogfish till stage O of Balfour's nomenclature; in the salmon and trout there is no trace of an olfactory vesicle up to the time of hatching, nor indeed, for some time afterwards. Mr. Marshall maintains that the olfactory vesicle must therefore be regarded as a structure of merely secondary importance; and that the olfactory nerves, since in their early stage they do not differ embryologically in any respect from the segmental cranial nerves, must be regarded as the first or most anterior pair of true segmental nerves.

The Development of the Olfactory Organ

This will, in the absence of figure, be treated very briefly; those points only being noticed which are of special interest in connection with the conclusions arrived at in the preceding part of the paper.

The olfactory pits appear at almost the same time as the visceral clefts; or, to speak more accurately, they first become conspicuous objects at, or very shortly after, the time when the anterior visceral clefts become open to the exterior. This occurs about stage K in the dogfish, and about the fiftieth hour in the chick.

In their early stages the olfactory pits present a striking resemblance to the visceral clefts in position, shape, size, and general relations; their external apertures elongate and become slit-like, and the direction of the slit, like that of the visceral clefts, is at right angles to the longitudinal axis of the head. These facts are best illustrated by the study of whole embryos, and of longitudinal vertical sections.[1] They come out with great clearness in all the types of vertebrates examined, but with especial distinctness in the axolotl and salmon.

The development of the Schneiderian folds presents several points of great interest, which can be most favourably studied in the elasmobranchs. Attention has already been directed by Balfour[2] to the very early appearance of these folds. The important point, so far as the present question is concerned, is that these Schneiderian folds appear at the same time as, or very shortly after, the first rudiments of the gills. In addition to this identity in time, there is also identity in structure; in both cases development consists in the formation of a series of equal, closely apposed folds, mainly epithelial, but involving the underlying mesoblast to a certain extent. These folds are in the two cases—gills and Schneiderian folds—of the same width, the same distance apart, have epithelium of the same thickness and same histological character, involve the mesoblast to exactly the same extent, and in exactly the same manner; in a word, are structurally identical.

In the later stages the Schneiderian folds, like the gills, receive a very abundant supply of blood-vessels; and the relations of these vessels to the folds, which are very peculiar and characteristic, are identical in the two cases. Even in the adult elasmobranch there is a remarkable histological resemblance between the gills and the nose.

[1] For figures of whole embryos illustrating the points referred, *vide* Parker, "On the Structure and Development of the Skull in Sharks and Skates," *Trans.* Zool. Soc., vol. x. part iv., 1878; Pl. 25, Fig. 1; Pl. 39, Figs. 1 and 2; Pl. 40, Fig. 1; and Balfour, *op. cit.*, Pl. 7, Stage L.

[2] *Op. cit.*, p. 184, and Pl. 44, Fig. 14.

The facts above recorded concerning the development of the olfactory nerve and olfactory organ point towards the same conclusions as to the morphology of these structures, viz., that the olfactory organ is a visceral cleft; that the olfactory nerve is the segmental nerve supplying that cleft in a manner precisely similar to that in which the hinder clefts are supplied by their respective nerves; and that the Schneiderian folds are gills.[1]

These conclusions, if accepted, will considerably simplify our conception of the segmentation of the vertebrate head. As there are no nerves or clefts in front of the olfactory segment, the olfactory nerve must be taken as the most anterior nerve, and the nose as the most anterior cleft. The next cleft is that in front of the maxillo-palatine arch, of which a part probably persists in the adult as the lachrymal duct: the segmental nerve corresponding to this cleft is the *third*, or oculomotor nerve. Next comes the mouth cleft, supplied by the *fifth*, or trigeminal nerve; and then in succession the clefts supplied by the facial, glossopharyngeal, and pneumogastric nerves. This view of the constitution of the vertebrate head is found to accord well with the later researches of Prof. Parker on the morphology of the skeletal elements of the head.

Some at least of the labial cartilages will probably prove, on this view, to be homologues of the extrabranchials, a comparison that has already been made by Prof. Parker.[2]

If the olfactory organs are visceral clefts, they must originally have communicated with the mouth cavity. Indications of a former connection of this kind are by no means wanting; thus in salmon embryos the alimentary canal extends forwards, so as to underlie the nasal sacs; as development proceeds, this anterior prolongation of the mouth cavity gradually shrinks; it persists for a short time as a pair of cæcal diverticula, which ultimately disappear altogether.

In conclusion, it may be noted that the Schneiderian folds afford an instance, on the theory here maintained, of structures originally hypoblastic in nature becoming, from changed circumstances, epiblastic.

"On an Extension of the Phenomena discovered by Dr. Kerr and described by him under the title of 'A New Relation between Electricity and Light.'" By J. E. H. Gordon, B.A., Assistant Secretary of the British Association. Communicated by Prof. Tyndall, F.R.S.

In November, 1875, Dr. Kerr announced in the *Philosophical Magazine*, that he had discovered a new relation between electricity and light. He showed that when glass is subjected to an intense electrostatic stress that a strain is produced which causes the glass to act like a crystal upon polarised light.

On Wednesday, February 5, 1879, I was working at this experiment in the Royal Institution, and endeavouring, by means of the electric light, to project the effect on a screen, in preparation for a lecture on the next day.

In the experiment as described by Dr. Kerr, and which was shown plainly on the screen, on February 6, the light is extinguished by the Nicols, and reappears when the coil is set going.

In the projection experiment a patch of moderately bright white light, about 3 inches diameter, appeared on the screen when the coil was worked. The images of the points inside the glass were about 1½ inches apart. On Wednesday, however, the electrostatic stress was accidentally allowed to become strong enough to perforate the glass. Immediately before perforation there occurred the effects which are the subject of the present communication.

First appeared a patch of orange-brown light about six or seven inches diameter. This at once resolved itself into a series of four or five irregular concentric rings dark and orange-brown, the outer one being perhaps fourteen inches diameter. In about two seconds more these vanished and were succeeded by a huge black cross about three feet across, seen on a faintly luminous ground. The arms of the cross were along the planes of polarisation, and therefore (the experiment being arranged according to Dr. Kerr's directions) were at 45° to the line of stress.

The glass then gave way, and all the phenomena disappeared except the extreme ends of the cross, and the discharge through the hole, where the glass had been perforated, was alone seen.

The phenomena were seen by Mr. Cottrell, by Mr. Valter (the second assistant), and by myself. A fresh glass plate was

[1] Cf. Dohrn, "Ursprung der Wirbelthiere," p. 23.

[2] *Proc.* Zool. Soc., vol. x. part iv., 1878, p. 212.

at once drilled in hopes of repeating the phenomena in the lecture next day, but owing to sparks springing round we did not succeed in perforating the glass, and therefore saw only the faint return of light described by Dr. Kerr.

Some more glasses have been prepared and their terminals insulated, and I now propose to make another attempt to repeat the new effects before the Royal Society.

Zoological Society, February 18.—Prof. W. H. Flower, F.R.S., president, in the chair.—The Secretary exhibited, on behalf of the Rev. T. O. Morris, an example of *Bombyx quercûs* with malformed antennæ.—Mr. Sclater exhibited a new humming bird from Northern Peru, which he had received for identification from M. L. Taczanowski, C.M.Z.S., and which he proposed to name *Thaumatius taczanowskii*.—Mr. Sclater exhibited a living amphisbænian (*Bronia brasiliana*) lately received by the Society from Monte Video.—A communication was read from Mr. E. L. Layard, C.M.G., F.Z.S., containing a note on *Pachycephala icteroides*, Peale, with the description of a supposed new species of the genus from Ovalau, Fiji group, proposed to be called *P. neglecta*.—A communication was read from Dr. A. Günther, F.R.S., containing a description of four new species of chameleons from Madagascar, proposed to be called *Ch. malphe, Ch. brevicornis, Ch. gularis*, and *Ch. globifer*.—A communication was read from Mr. Edgar A. Smith, F.Z.S., containing a description of a large collection of mollusca from Japan, formed by Capt. H. C. St. John, R.N., of H.M.S. *Sylvia*.—Messrs. Godman and Salvin read descriptions of a number of new species of butterflies from Central and South America.—A second communication from the same authors gave an account of a collection of butterflies made by the Rev. G. Brown in New Ireland and New Britain.—Mr. A. G. Butler gave an account of the Heterocera contained in a collection from the same locality.—A communication was read from Mr. W. A. Forbes on the systematic position of the genus *Lathamus*, in which, from a study of its pterylosis, osteology, and other points in its external and internal structure, he showed that this parrot must be referred to the neighbourhood of the *Platycercidæ*.—Mr. R. Bowdler Sharpe read a note on *Heliodilus soumagnei*, Grandidier, of which a specimen had recently been acquired by the British Museum.—Mr. Sharpe likewise pointed out the characters of a second species of the genus *Dromæocercus*, from Madagascar, proposed to be called *D. seebohmi*.—A communication was read from Mr. A. Boucard, C.M.Z.S., containing descriptions of two supposed new species of South American birds.—Dr. F. Day read some remarks on the occurrence at Southend of the little gurnard, *Trigla pœciloptera*.

Meteorological Society, February 19.—Mr. C. Greaves, president, in the chair.—Eleven new Fellows were elected and thirteen candidates proposed.—The following papers were read:—Diurnal variations of barometric pressure in the British Isles, by Frederick Chambers. The object of this paper is to show that differences of types of the diurnal variations of pressure at inland or sea-coast stations are due to the superposition, on a common type of diurnal variation at all the stations, of a distinct diurnal variation of barometric pressure, such as is required to satisfy the convection-current theory which explains the well-known diurnal land and sea breezes. To show this, all that is necessary is to take the differences of the corresponding hourly inequalities of the barometric pressure at pairs of inland and coast stations, and to exhibit these differences in the form of curves, which are then found to closely resemble the curves of diurnal variation of air temperature.—On a standard cistern siphon barometer, by Frederick Bogen.—On the relation existing between the duration of sunshine, the amount of solar radiation, and the temperature indicated by the black bulb thermometer in vacuo, by G. M. Whipple, B.Sc., F.R.A.S. The author has instituted a comparison between the duration of sunshine, as determined by Campbell's sunshine recorder, and the amount of solar radiation, as ascertained from the readings of the black bulb thermometer in vacuo, for the year 1877, at the Kew Observatory. It is evident that there is a close relation between these phenomena, but owing to the great range of the black bulb thermometer, the exact nature of the connection is not immediately evident. The author says that it may be safely concluded that the measure of solar radiation as given by the black bulb thermometer is only to be considered at any place as an indication of the relative presence or absence of cloud from the sky at the locality, and so its use as a meteorological instrument may with advantage be set aside in favour of the sunshine record, which has not the elements of uncertainty attached to it, inseparable from the former instrument.—Results of meteorological observations made at Buenos Ayres, by William B. Tripp, Assoc. Inst. C.E.

Anthropological Institute, February 11.—Prof. W. H. Flower, LL.D., F.R.S., vice-president, in the chair.—The election of Sir Henry Sumner Maine, K.C.S.I., LL.D., F.R.S., as a Member, was announced.—Prof. W. H. Flower, LL.D., F.R.S., exhibited and described a scaphocephalic cranium from Fiji, and Mr. A. L. Lewis exhibited a series of implements and photographs from Australia.—Mr. John E. Price read a paper on the Australian Aborigines, by Mr. D. Macallister. After describing their social and domestic observances, traditions, and religious notions, the author concluded that he had no doubt that had the continent of Australia remained undiscovered by Europeans for a few thousand years longer, the climatic and general physical changes which would doubtless have occurred, together with the contact at intervals with their more civilised Polynesian neighbours, would have constituted an environment more favourable to progress than any which has ever existed, and would have tended to an improved condition of the people. As it was, the total absence from the continent of ferocious or powerful animals, the comparative ease with which the poor and limited quantity of their food was obtained, and their national isolation, may have been a patent cause for the non-progressive character of the people.—The director also read a paper by Capt. W. E. Armit, F.L.S., on the customs of Australian Aborigines.

Entomological Society, February 5.—Sir John Lubbock, Bart., V.P.R.S., president, in the chair.—Messrs. H. W. Bates, J. W. Dunning, and F. Smith, were nominated vice-presidents for the ensuing year.—Mr. H. J. Elwes exhibited a collection of lepidoptera from a small island at the mouth of the River Amur.—Mr. Waterhouse exhibited a remarkable spider from West Africa, *Gasteracantha Cambridgei*, Butt.—A specimen of *Harpalus oblonguisculus*, taken at Weymouth, was exhibited by Mr. Champion.—The Rev. A. E. Eaton remarked on the peculiarities in the neuration of the wings of most of the *Ephemeridæ*, and exhibited drawings of wings of *Trichoptera* and *Tineina*, to show the homologies in the neuration of the same.—The Secretary read a note from Dr. Fritz Müller, recording a remarkable case of mimicry in the Brazilian butterfly, *Eueides pavana*, which mimics *Acræa thalia*. It is, however, in the male sex of *E. pavana* that the greatest resemblance to the *Acræa* is found.—The following papers were also communicated: On the lepidoptera of the Amazons, &c., Part iii., Noctuites, by A. G. Butler.—Description of a new genus of rhyncophorous coleoptera, &c., by C. O. Waterhouse; and descriptions of the species of the lepidopterous genus Kallima, by F. Moore.

Institution of Civil Engineers, February 18.—Mr. Brunlees, vice-president, in the chair.—The paper read was on the construction of heavy ordnance, by Mr. J. A. Longridge, M. Inst. C.E.

BOSTON

Society of Natural History, May 15, 1878.—The Devonian brachiopoda of the Province of Para, Brazil, by R. Rathbun, late assistant geologist to the Geological Commission of Brazil.

PHILADELPHIA

Academy of Natural Sicences, November 5, 1878.—Descriptions of Ichneumonidæ, chiefly from the Pacific slope of the United States and British North America, by E. T. Cresson.

November 12.—Descriptions of a new species of Delabella from the Gulf of California, with remarks on other sp cies, by R. E. C. Stearns.

November 26.—On the structure of the gorilla, by Dr. Chapman, dealing with the muscles of the extremities as found in a male of two years old.

BERLIN

Chemical Society, February 10.—At the close of Prof. Kopp's remarks (see p. 387), Herr Frank exhibited a mass of infusorial earth saturated with bromine. As this form of silica will take up eight or ten times its own weight of bromine, he claimed that this would be a convenient form of handling, weighing, and using this corrosive liquid. The speaker also stated that he had found petroleum to be a specific for the burns and stains of bromine.—Herr Baumann then exhibited a specimen of hydroquinon found in the form of hydroquinon-sulpho acid in

the urine of a dog poisoned with carbolic acid. This remarkable conversion, in the system, of a mono-phenol into a di-phenol, was less remarkable than the statement of the speaker that he had also found a compound of paracressol in the urine of a horse, where it had been produced from the ordinary food.

Vienna

Imperial Academy of Sciences, December 19, 1878.—The following, among other papers, were read:—Explanations of some orographic and topographic details of European Turkey, not rightly understood by geographers hitherto, by Dr. Boué.—Preliminary remarks on the formation of rational plane curves on one another, by Dr. Weyr.—Researches on the relations of nutritive matters to the transpiration of plants (second series), by Dr. Burgerstein.—On some chemical constants, by Prof. Peschka.—The theory of electrotonus, by Dr. Fleischl.—On the orbit of the planet (153) Hilda, by Herr Kühnert.

January 9.—Critical researches on the species of the natural family of *Cervi* (concluded), by Dr. Fitzinger.—On direct muscle-excitation with the muscle-current, by Prof. Hering.—On the magnetic behaviour of pulverised iron, by Prof. Waltenhofen.—Spectroscopic researches, by Herr Ciamician.—Determination of coefficients of elasticity from bending of a bar, by Prof. Pscheidl.—On a new water-wheel, by Herr Kersovini.—On a new problem of ballistics, by Dr. Simony.—The daily period of the velocity and direction of the wind, by Dr. Hann.—On the action of nitrous acid anhydride on protocatechuic acid, by Dr. Gruber.

January 16.—Natural history of the Flagellata (the third part of a work on Infusoria), by Prof. Ritter v. Stein.—On Dr. Rosicky's experiments with Geissler tubes, by Prof. Mach.—On the summation of stimuli by the heart, by Prof. Ritter v. Basch.—On condensation-products of gallus acid, by Prof. Oser and Herr Böcker.—Report on the results of investigations and excavations by the prehistoric commission during the past year.

Paris

Academy of Sciences, February 17.—M. Daubrée in the chair.—The following papers were read:—Meridian observations of small planets at the Greenwich and Paris Observatories during the fourth quarter of 1878, communicated by M. Mouchez.—Determination of the coefficient of elasticity of different substances and of their limit of elasticity, by M. Phillips. The method suggested is based, like a previous one, on the theory of a regulator spring, but the influence due to inertia of the spring is suppressed. The coefficient of M. Deville's new alloy of iridium is given.—New researches on electric fish; characters of the discharge of the gymnotus; effects of a torpedo's discharge sent through a telephone, by M. Marey. He finds the discharge of the gymnotus pretty similar to that of the torpedo, and it is similarly affected by temperature. Using the telephone, a gentle excitation of a torpedo produces a short croaking sound, each of the small discharges consisting of only a dozen fluxes, and lasting hardly $\frac{1}{15}$th of a second. But the sound from a prolonged discharge, caused by pricking the electric lobe of the brain, lasts three or four seconds, and is a kind of moan, the tonality being near *mi*, (165 vibrations).—On the project of the interior sea in Algeria, by M. Favé. The topographic levellings at Suez, on land, comparable to that of Sahara, were proved (M. Favé urges) to have sufficient exactness.—Does the didymium of samarskite differ from that of cerite? by M. Lecoq de Boisbaudran. Both, he finds, give the same three blue lines.—New spectral lines observed in substances extracted from samarskite, by the same. He finds new lines or bands (not described by MM. Delafontaine, &c.), both of emission and absorption, which correspond together (at least the principal), and belong apparently to some new body. He expresses high admiration of Prof. L. Smith's generosity in distributing to chemists in France and America, rare and arduously elaborated products which he had not completed the examination of.—On the measures taken by the Sanitary Intendance of Marseilles, in the fear of invasion by the plague, by M. de Lesseps. He argues that it is foolish and useless to hamper the commerce of Marseilles with quarantine, &c., as the disease is not contagious but infectious, spreading by emanations carried by the air; and it would not be likely to attack such a town. He points out that the plague in Lower Egypt in 1834-5 did not spread to Upper Egypt, though the communications were not interrupted. M. Bouley contended that where the plague had appeared in Western Europe, it had come by diseased persons or objects in contact with them. M. D'Abbadie thought M. Bouley too absolute in asserting that the Oriental plague always spread by contagion.—On the Foucault's top transformed into a gyroscopic pendulum, by M. Gruey.—On the determination of the number of double points of a space defined by algebraic conditions, by M. Saltel.—Application of the direct potentials of Lamé to calculation of the equilibrium of elasticity of an isotropic and indefinite homogeneous solid, solicited in a finite extent by any exterior forces, by M. Boussinesq.—On unequal propagation of light polarised circularly in bodies submitted to the action of magnetism, according to the nature of the magnetisation and the direction of the luminous vibrations, by M. Becquerel. The fact here stated he demonstrated experimentally. The displacement of interference fringes under the magnetic influence, was the criterion employed.—Researches on the compressibility of gases at high pressures, by M. Amagat. The method (in which a deep pit is resorted to has been already described. Under a pressure of 430 atmospheres (the greatest reached), the volume of nitrogen is nearly a fourth greater than that deduced from Mariotte's law; this corresponds to a difference of nearly 100 ctm. in the pressure necessary to get the reduction of volume deduced from this law.—Note on the phenomenon observed by M. Duter, by M. Korteweg.—Improvements in Harrison's electric lamp, by M. Ducretet. Apparatus inclosed in the supporting case regulates automatically the consumption of the carbons and keeps the luminous arc constant.—On the relations which unite tetric and oxytetric acids and their homologues to succinyle, malyle, and other radicals of bibasic acids, by M. Demarçay.—Bromocitraconic acid, by M. Bourgoin.—On the respiratory innervation in the poulpe, by M. Fredericq. The integrity of visceral nerves, the subœsophagean masses, and the palleate nerves, seem alone indispensable to normal production of respiratory movements.—On the functions of the ganglionic chain in decapod crustaceans, by M. Yung.—On the existence of Saigas in France in the age of the reindeer, by M. Gaudry. M. Lartet, several years ago, announced the discovery of horns of the animal, but thought these had been brought as arms by a strange people. Jaw-bones and bones of the limbs have now been found.—Geological study of strata traversed by a tunnel of 14,400 metres, for directly connecting the Fuveau basin of lignite with the sea, by M. Dieulafait.

CONTENTS

THURSDAY, MARCH 6, 1879

COAL AND COAL-MINES

A Treatise on Coal, Mine-Gases, and Ventilation. By J. W. Thomas. (London: Longmans, 1878.)

MR. THOMAS is known to chemists by reason of the numerous and excellent analyses of the gases inclosed in various coals which he published some time ago in the *Journal* of the Chemical Society. Marsilly in France, and Meyer in Germany, first broke ground on this subject, but it is to Mr. Thomas, who very greatly improved the method of research, that we owe our most exact knowledge of the character of these occluded gases. The matter is of considerable importance from a twofold aspect. It not only serves to throw light upon the scientific question of the formation of coal, but also affords us information on the very practical question of the nature of the influence of the atmosphere upon the deterioration of coal. These observations attracted sufficient attention from mining engineers and persons connected with mining to induce the author to reprint them together with other matter relating to the general subject, and the result is the volume before us—a work which it is not too much to say ought to be in the hands of the manager and sub-officers of every colliery in the kingdom. Although the subjects of gases in coal and of the deterioration of coal by atmospheric influences are treated at considerable length, the relative or connected matter occupies by far the greater portion of the book, and we have chapters on the Diffusion and Transpiration of Gases, Explosions in Mines and Ships, Combustion, Ventilation, &c.

The opening chapter is occupied by a discussion of that most vexed of questions, What is Coal?—a question which has already cost some people much patience and more money to get answered, but hitherto without success. We are bound to say our author does not help us towards a solution; we are apparently as far off as ever from a scientific definition of this everyday commodity. His remarks on the classification of coals are extremely just. It is not surprising, from the author's connection with the South Wales Basin, that the particular subject of anthracite should receive special attention at his hands. The origin of this form of mineral fuel has given rise to much discussion in the past, but it is only within recent time that the opinion that it is not necessarily the oldest transition-product in the decay or alteration of vegetable matter has begun to gain ground. Our author throws considerable light on the process which has been at work in the formation of anthracite, from a study of the composition of the gases which are found inclosed in this class of coal. He regards the process of the formation of coal (after the first or primary decomposition which ensued during the time the vegetable matter was in the act of being buried) to have taken place in four ways, viz.:—

1. The dry process aided by heat, leading to the production of anthracite.
2. The dry process without much abnormal heat, to which the production of steam coal and Wigan cannel is due.
3. The wet process with heat, to which may belong the production of Scotch cannel and the more dense varieties of bituminous or house coals.
4. The wet process without long-continued heat, to which we may assign the formation of ordinary bituminous (house coals) and lignite.

With respect to the nature of the material from which coal has been originally derived, Mr. Thomas has little or nothing to add to what is commonly stated in popular works on the subject. To dismiss the spore-theory in some six or eight lines, with the assertion that "there is little evidence of a weighty character to confirm this hypothesis," is, in the light of existing facts, scarcely just to the researches of Carruthers, Morris, and Huxley. To say that all coal has been derived from lepidodendroid spores would unquestionably be incorrect, but to assert that many coals have been largely derived from such material is undoubtedly true. This much, we suppose by this time, is conceded by the majority of authorities on the subject: the difference between them is rather as to the proportionate part played by the spores.

As regards the character of the gas occluded in coal, this depends not merely upon the structure of the coal, but upon the conditions under which its formation has occurred and the depth below the surface at which it is situated. On the whole Mr. Thomas's analytical results agree fairly well with those of Dr. Meyer, in spite of the somewhat faulty method adopted by the latter; but the inferences which the two experimenters draw from their observations are not unfrequently opposed. There seems to be a decided difference between the gas inclosed in lignites and that found in coals of the older formations. Zitowitsch first detected carbonic oxide in some specimens of Bohemian lignites, and the author has detected the same gas in appreciable quantity in the well-known lignites of Bovey Heathfield, in Devonshire. On the other hand, no coal of the carboniferous period has been found to contain this gas. Another characteristic difference between the coals of the carboniferous and tertiary periods is seen in the absence of all hydrocarbons among the gases occluded in the latter varieties.

The presence of carbonic oxide in "after-damp" has been frequently surmised, but Mr. Thomas brings direct evidence to prove the fact. Until quite recently it was the general belief among chemists that when marsh-gas—the "fire-damp" of the miners—is mixed with less air than is required for complete combustion, only as much of the hydrocarbon is burnt as the oxygen present can convert into carbon dioxide and water. It is now found that when marsh-gas is exploded with a quantity of oxygen or air insufficient for complete combustion, the *whole* of the marsh-gas disappears into carbonic oxide, carbon dioxide, water, and free hydrogen. The importance of this observation is obvious. It has not unfrequently happened that after an explosion the expressions of the countenances of the victims, who have been found sitting or leaning in the most natural positions, have worn no trace of fear or intense anxiety, such as we should suppose would come upon them amidst the roar and wreck of the disaster. These men have not been asphyxiated by carbonic acid or drowned in excess of nitrogen: they have been struck down by the infinitely more deadly carbonic oxide. Instances too have occurred in which the explorers after an explosion have been almost as suddenly

overwhelmed, even when their lamps were burning freely. These facts point to an entirely different mode of treating those who have been rescued and who are still suffering from the effects of after-damp, from that which has been hitherto adopted. It is not too much to say that many a life could have been saved if, acting on Hoppe-Seyler's observations, artificial respiration could have been maintained for some time after apparent death.

There is much in this book that we should have liked to have dwelt upon had space permitted, for almost on every page we discern evidences of originality and freshness such as might be expected from one who, as we have seen, has brought the researching spirit to bear upon his subject. Whilst chemists are wearying themselves and others with vain speculations as to bonds and atomic-groupings, far too many of the common matters of every-day life are thrown aside as unfruitful or worked out. No one, however, could take up this book and not see that in the matter upon which it treats there are fifty problems waiting for solution—some of them most pressing in the interests of humanity, and any one of them capable of yielding a rich harvest of facts. T. E. THORPE

THE MORPHOLOGY OF THE ECHINODERMS

Morphologische Studien an Echinodermen. Von Dr. Phil. Hubert Ludwig, Director der naturwissenschaftlichen Sammlungen in Bremen. 1 Band mit 23 Tafeln und 5 Holzschnitten. (Leipzig: Verlag von Wilhelm Engelmann, 1877-79.)

WITHIN the last three years very numerous researches have been made on that most interesting group, the Echinoderms, to which, we are glad to see, the rank of a distinct sub-kingdom is now generally assigned. Greeff, Götte, Lange, Ludwig, Simroth, and Teuscher, in Germany; Perrier in France, Théel and Lovèn in Sweden, Agassiz, Lyman, and Pourtalès in America; and in our own country Sir Wyville Thomson, Duncan, Sladen, and the two Carpenters, father and son, have each contributed more or less to our knowledge of the morphology and physiology of the group.

Of the many observations made and recorded by the above-mentioned naturalists, those of Dr. Ludwig ("Ei-bildung Ludwig," as we have heard him called by embryologists) seem to us to be among the most important, alike from their variety, and, as we are strongly inclined to believe, from their general accuracy. We are not so sure, however, that all Dr. Ludwig's conclusions are as correct and reliable as his observations are trustworthy, for there are certain points on which we have very strong grounds for dissenting from his views.

The volume before us, representing the result of three years' work, mostly microscopic, is the first of a promised series of studies in Echinoderm morphology, and consists principally of memoirs on the anatomy of Crinoids and Starfishes.

It contains much that is new, or rather that was so when the individual memoirs were first published in the *Zeitschrift für wissenschaftliche Zoologie*, and much that is to be found, stated more or less correctly in the writings of other workers, both before Ludwig and contemporaneous with him.

The first paper in the series, forming about one-third of the whole volume, is devoted to the anatomy of Comatula. While generally confirming Dr. Carpenter's results, respecting the canals of the arms and the chambered organ, Ludwig (whose observations on this type were contemporaneous with those of four other observers, two in Germany and two in this country) publishes several new and interesting anatomical details.

Among the most important of these is the presence of blood-spaces around the genital organs, and also of a system of blood-canals ventral to the water-vascular system. Both of these systems probably communicate with the vascular "axial prolongation" of Dr. Carpenter, which runs up into the disc from the chambered organ situated in the calyx, and represents the "heart" of the Starfishes.

Ventral to the radial blood-canal is the fibrillar sub-epithelial band, to which Dr. Ludwig assigns a nervous character from its resemblance to a similar and similarly placed structure, that is generally, though not universally, supposed to be the nerve of the arm of a Starfish.

Ludwig's views have been completely adopted by Gegenbaur, in spite of the fact that this band is absent from half, or sometimes from more than half, the arms of many Comatulæ. We scarcely think that Ludwig has taken this fact sufficiently into consideration in his discussion of Dr. Carpenter's suggestion that the axial cords of the skeleton constitute the chief nervous system of Comatula; and we are not altogether satisfied with the purely diagrammatic manner in which he figures this axial cord, and with the meagre description which he gives of it. He makes no mention whatever of the regular manner in which it gives off branching bundles of fibres to the muscles and other structures in the middle of every arm-joint, except in quoting their discovery by others, though he cannot well have helped seeing them, and he does not deny their existence. At the same time he seems inclined to admit the probative force of Dr. Carpenter's experiments at Naples, which tend to show that these axial cords are the *motor* nerves, at any rate of the complex Crinoid organisation, permeated though they may be by a coagulable fluid. Should this view be the true one, it is another argument in favour of Leuckart's separation of the Crinoids and their allies from the other Echinoderms, to form a distinct class, the Pelmatozoa.

Until lately the Crinoids have not been credited with an ambulacral system homologous with that of the other Echinoderms. Götte, however, has shown that, as far as development is concerned, this is not the case, and the true water-vascular ring of the adult Comatula was first described by Ludwig, though its radial branches in the arms have long been known. Depending from it into the cœlom are numerous small tubules which Ludwig describes as open at the ends, and compares to the sand-canals of the other Echinoderms, more especially of the Holothurians.

After finishing his researches on the anatomy of Comatula, Ludwig turned his attention to Rhizocrinus, and found that it corresponds with Comatula in all essential points of structure. This was the first stalked Crinoid in which the presence of a chambered organ was determined. It has since been found in Pentacrinus and Bathycrinus, and Ludwig's discovery that its chambers are continued down the axis of the stem as five blood

canals disposed around a central axis, has received abundant confirmation.

The curious genus Rhopaladina has been supposed by some writers to be the type of a new group of Echinoderms, with mouth, anus, and genital opening at the centre of one of the poles of the body. Ludwig shows, however, that it is merely an aberrant Holothurian, much bent on itself, owing to the almost complete disappearance of the medio-dorsal inter-radius, an exaggeration, in fact, of a condition very commonly met with in Cucumaria. Ludwig's memoirs on the Asterids contain several novelties, two of the most important of which are as follows:—

1. The pores in the madreporic plate have no communication with the cœlom, but lead solely into the sand canal. The same fact has been noted by Perrier for the Echini, and it is the more singular because the body cavity of the Crinoids is in free communication with the exterior.

2. The genital glands do not discharge their products into the body cavity, but are provided with longer or shorter ducts that open directly on the exterior of the body. Around the glands are blood spaces, just as in the Crinoids, and these are connected with a very complicated blood-vascular system, which Dr. Ludwig describes exceedingly well, clearing up many points which had hitherto been very obscure and scarcely understood. This is especially the case with the remarkable genus Brisinga, which Sars supposed to be without a blood-vascular system. Ludwig contributes many valuable observations to the anatomy of this type, and shows that in all essential features it is a true Asterid, though he does not share Sars' views of its relationship to Protaster.

The volume closes with a striking paper on the Ophiurids, in which it is shown that the whole of the oral skeleton of the disc is the result of modifications of the first two arm-vertebræ and of the adambulacral and superambulacral plates corresponding to them. But the chief novelty in this paper relates to the genital clefts. These have been hitherto supposed not only to let the genital products pass out of the cœlom, but also to admit water into it. Ludwig shows, however, just as in the case of the Asterids, that both these hypotheses are incorrect. The sexual products are not discharged into the cœlom, nor does water enter it by the clefts, but the latter open into pouches or bursæ which are merely involutions of the general integument of the body, and receive the short ducts of the genital glands, probably serving also as a respiratory apparatus. Their inner surface, that turned towards the cœlom, is curiously folded, and their whole structure is so very similar to that of the hydrospires of the Blastoidea that Ludwig is led to suggest a homology between these two sets of similarly placed organs. Billings considered the hydrospires to be respiratory in function, and found them to be connected with the "spiracles" or genital openings, which would thus be homologous with the genital, or, as Ludwig prefers to call them, "bursal" clefts of the Ophiurids.

Should further investigation confirm this interesting discovery of Ludwig's, and the conclusions he has drawn from it, we quite agree with him in regarding it as one of great importance respecting the relations of the various echinoderms *inter se*.

We shall look with great interest for the publication of Dr. Ludwig's promised researches on the Echini and Holothurians at, we trust, no distant time; and also for his concluding work on the comparative morphology of the sub-kingdom as a whole, which will not, we imagine, be altogether a pillar of strength to Haeckel's celebrated "Worm Theory of the Echinoderms."

OUR BOOK SHELF

Jornal de Sciencios Mathematicas Physicas e Naturaes. Publicado sob os auspicios da Academia Real das Sciencias de Lisboa. No. xxiii. agosto de 1878. (Lisboa, 1878.)

Giornale di Matematiche: ad uso degli Studenti delle Università Italiane. Pubblicato per cura del Professore G. Battaglini. Vol. xvi. (Napoli, 1878.)

WE have not seen any previous numbers of the first of these publications, but from the specimen before us we should certainly conclude that this Society is doing good work. In fifty-two octavo pages we have specimens of work in all the lines indicated in the above title. The opening paper, by C. A. Moraes de Almeida, is an "estudo geral dos espelhos curvos" (continuation, 11 pages); Chapter IV. treats of spherical, elliptical, and parabolical mirrors of very small aperture; Chapter V. discusses some cases of practical difficulty in the formation of images. The second paper is a mathematical one by L. F. Marrecos Ferreira; 1st part, on the geometrical properties of the intersections of right cones, derived from the principle of the homological transformation; 2nd part, on the properties of conics tangential to the sides of an angle and their application to the study of surfaces (18 pages). Both papers are neat, and contain interesting properties.

Zoology follows, with two contributions by J. V. Barboza du Bocage, first with a list (the sixteenth) of the birds in the Portuguese possessions in West Africa (15 pages), next under the title "Mélanges Ornithologiques," remarks on new species of Angola (*Nectarinia anchietæ*[1]) and on individual birds of the families Certhiidæ (*Hylypsornis Salvadori*), Paridæ (*Parus rufiventris*), Laniidæ (*Lanius Souzæ*, *Nilaus affinis*).

The last two pages contain a slight account of 111 plants, collected in Caconda by Signor Anchieta. Remarks are made on the points of contact between some of the plants in this collection and that got together by the late Dr. Welwitsch

The second journal maintains its reputation for its contributions to the study of geometry. Where there is so much to praise we must limit ourselves to giving here the bare titles of some of the longer papers:—Ricerche geometriche sopra alcune proprietà dei sistemi di rette nel piano e dei sistemi di circoli che passano per un punto sul piano e sulla sfera, per T. Fuortes (56 pp.); Sulla riforma dell'insegnamento geometrico, nota di G. Fiedler seguita da tre lettere inedite dell' autore (13 pp.); Sull' infinità circolare non Euclidea, per G. Battaglini (7 pp.); Relazione fra l'area e il perimetro, fra il volume e la superficie, fra i momenti, fra le coordinate dei centri di gravità per gli spazi limitati da linee e superficie che hanno l'equidistante della stessa natura per U. Dainelli (20 pp.); Sulla teoria delle quadriche omofocali del punto di vista sintetico per F. Maglioli (36 pp.); Nozioni preliminari per la geometria projettiva dello spazio rigato, Nota i di F. Aschieri (19 pp.); Sopra le curve piane del 3° ordine con un punto doppio, per P. Anelli (14 pp.).

The History of Coal. By the Rev. T. Wiltshire, M.A. F.G.S., &c. (E. and F. N. Spon, Charing Cross and New York. Pp. 36, 1878.)

THIS pamphlet is the introductory lecture which the

[1] So named from the finder, who met with a single specimen, October, 1877, in the interior of Benguella.

author, as Dean of the Evening-class Lectures at King's College, London, delivered before the Principal, the staff, and students, at the commencement of this Winter Session.

It deals with the antiquities of coal, and as might have been anticipated, shows much patient scholarship and research amongst the works of those classical and mediæval writers who are not usually troubled by geologists. Prof. Wiltshire considers that there is evidence to show that ignition of coal, which had been selected as hut-making material with the help of palæolithic and neolithic implements, occurred and probably accidentally; the mineral not having been selected for its now well-known qualities. Nevertheless, he admits that "the general non-employment of the coals and lignites, in the stone and bronze ages, is well evidenced by the absence of allusion to their use, both in myths and traditions of that date and in the manuscripts which followed not long after." Noticing the silence of Homer on the use of coal, the author very properly places the Levitical coals and those of the Gospels amongst charcoals derived from wood, and he shows that the "carbo" of Pliny and the "anthrax" of Theophrastes were identical. Searching over the dreary pages of this last-named author, Prof. Wiltshire shows that this Greek first noticed and recorded that certain stones were obtained from the ground, and that, broken in pieces, they burn like anthrax (charcoal), and that they come from Liguria and Elis.

The tertiary strata in those localities are lignitic, and true palæozoic coal does not exist there. As years rolled on many coal-like minerals were used, and fancies environed them. Certain it is that during the Roman occupation of Britain, coal was stored and used, for it has been found beneath the ruins of villas in Warwick, Shropshire, Yorkshire, Lancashire, and Durham. The gradual extinction of the Romano-British was followed by hundreds of years of forgetfulness of coal, and then we find the Abbot of Peterborough leasing ground, a part of the rent to be paid in coal, about the time of Alfred. About 1190 the Edinboro' coal-field began to be used, and in 1239 a monopoly of the sale of coal was granted to the Newcastle people.

This interesting lecture is most readable, and well worthy of the accomplished editor of the Palæontographical Society's volumes. P. M. D.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Quarantine in Italy

ALLOW me to say a few words in defence of the prohibitive measures taken by the Italian Government against the introduction into our country of that pest, the phylloxera, and which, though subjected to much unreasonable criticism and selfish opposition here and abroad, have at least been hitherto perfectly successful in protecting our vineyards. Certainly at first sight it would seem that the only necessary measure to be taken ought to be a restriction on the importation of foreign vines, and of the plants (such as fruit-trees) usually cultivated along with them in nursery-grounds, and on whose roots some stray insects might probably occur; and to such a restriction did the Government at first limit itself. But it was soon found out to be quite ineffectual, as it only gave occasion to smuggling on a grand scale, encouraged, I am sorry to say, by certain horticultural firms that did not fear to compromise their respectability by so doing. We all know what ingenious persons smugglers can be; they began introducing the prohibited plants in their leafless state labelled as choicest exotics, to the utter confusion of the Custom House officials, who, being neither naturalists nor scientific men to any degree, were at a loss how to act, until the Government was obliged at last to send the order that no live plant should pass the frontier; a simple rule that anybody can understand, and not to be regretted when one has seen in France and elsewhere thousands of acres of what were once flourishing vineyards blasted by the pest, and has heard of the millions of money lost that way. I shall not mention such petty annoyances as being obliged to leave a bouquet at the Custom House; but surely it is better that our gardens should be deprived of those novelties and rarities that are not obtainable through seeds, rather than run the slightest risk of diminishing one of the principal resources of our impoverished country. Though myself a director of a botanic garden, I own I cannot push my love for flowers to that extent to sacrifice to it much greater interests.

Pisa, February 26 T. CARUEL

Captain Cook's Accuracy

Apropos of your article on the centenary of Capt. Cook's death in NATURE, vol. xix. p. 334, it may be interesting to call attention to his remarkable accuracy in determining the positions of places laid down in his charts. There is a great contrast between his accuracy and the evident carelessness of some more recent navigators. Some years ago when I was sailing in the Pacific we were one day approaching the recorded position of an island which no one on board our vessel had seen. I was conversing with the captain, and asked him whether he expected to find it in its recorded place. To this he replied: "It is sure to be there, for Capt. Cook determined its position; and although I have been now a good many years in the Pacific, I have never yet found him wrong. Had it been the United States Exploring Expedition which determined its place, I should have thought the chances just about equal as to whether it is right or wrong."

There is, unfortunately, too much ground for the remark about the observations of Admiral Wilkes's Expedition. Those who have had opportunities to test the work done by it know that it is often most inaccurate. Quantity of work rather than the quality of it appears to have been the rule with the navigators who conducted that expedition. I believe all Cook's work was worthy of his reputation.

As this is a serious charge to make against such an expedition, it may be well to give some examples to substantiate it. The particulars of the first I take from Findlay's "South Pacific Directory," pp. 633-34. Respecting Vatoa, or Turtle Island, south-east part of the Fiji Archipelago, he says: "A singular mistake crept into the survey by the United States' Expedition. On May 5, 1840, the *Vincennes* 'had a sight of Turtle Island, and determined it to be in lat. 19° 48′ S., long. 178° 33′ W. It has the appearance of a small rounded knoll.' This would seem to be circumstantial, and is further confirmed by a foot-note on the same page. In a subsequent passage the *Porpoise* is said to have determined it to be in lat. 10° 50′ S., long. 178° 37′ 45″ W. 'It was found to be three miles long, by one and a quarter mile wide. The reef extends all around the island, and is from one and a half to two miles wide.'"

If we suppose that in the above 10° is a misprint for 19° there will be only 2′ in lat. and 4′ 45″ in long. difference between the two determinations. But it appears that both of these are about 30′ in error in their longitude. On this Findlay remarks: "This singular variation in longitude from that assigned to it by the great discoverer Cook (178° 0′ W.), or 37′ in error, is startling, because the accuracy of Cook in this instance had been confirmed by other navigators." He then gives an account of an examination of it made by Capt. Worth in H.M.S. *Calypso* in 1848, when he made the island to be "apparently about six miles in length," its centre twenty-nine miles eastward of Wilkes's position, and, instead of the reef from one and a half to two miles wide all around, on the south-west a reef five or six miles wide, with "a large oval coral patch detached from it lying north and south, eight or nine miles in length."

My second example of inaccuracy shall be one in cartography. Some years ago I was about to make a trip into the mountains of Savaii, the largest of the Samoa Islands. Before starting, with Wilkes's chart in my hand, I took a few bearings of points which might serve for comparison when I reached the mountains. I was standing at Tuasivi, a place on the eastern end of the island which may be seen marked in Grundemann's map. To my surprise I found I could see from that place to Tafua point, the south-east extremity of the island, whereas the chart made

the land to project very considerably between the two points. On further examination, I found that, instead of making the land trend inward to a very deep bay at Sapapali'i and Iva, as it does in reality, it had been made to extend seaward in a series of headlands. This error is perpetuated in all the maps I have seen, including Grundemann's, and that published in the *Journal des Museum Godeffroy*, both of which are based on Wilkes's chart.

My third and last example shall be one of a different kind. In 1870 I visited an island north of Samoa known as Quiros or Gente Hermosa. Wilkes' expedition described it as being *without a lagoon*. I found it to be barely four miles in diameter, but with a *deep fresh-water lagoon in its centre about three miles in diameter*. Now as the ring of land around this lagoon is only about one-third of a mile across, I cannot imagine how any members of the expedition could have landed without seeing the water. Such an inaccuracy as this would have been bad enough in the description of an ordinary traveller. It is inexcusable in an expedition specially fitted out for scientific observation.

S. J. WHITMEE

Cook's Collections

IN NATURE, vol. xix. p. 373, a remark of Dr. Hamy, of Paris, is reproduced, concerning "the fate of Cook's collections in being buried in an Austrian museum." It will be of general interest to make known that, what there is in Vienna of ethnographical objects in relation to Cook, consists of 260 numbers, chiefly originating from Cook's third voyage. These objects were bought by order of the Emperor Franz in the year 1806, at the auction of the Parkinson Museum in London (previously Lever Museum), and now form part of the large ethnographical collections, which will be accessible to the public in a few years in the new, nearly completed, Imperial Natural History Museum of Vienna. This museum will become one of the greatest and most complete on the continent, uniting all mineralogical, geological, palæontological, prehistoric, anthropological, ethnographical, zoological, and botanical imperial collections of Vienna under the charge of Prof. Hochstetter. A. B. MEYER

Royal Zoological Museum, Dresden, February 28

Magnetic Storm, May 14, 1878

THERE appears to have been a slight error in my note (vol. xix. p. 148); in the sixth line, it should read 14th instead of 15th.

With this exception the observations are correctly reported, and the period during which the greatest trouble was experienced in working on the Persian Gulf cable covers the time at which the magnetic storm was observed at Stonyhurst to be at its height (vol. xviii. p. 617).

I cannot quite agree with Mr. Preece when he suggests (vol. xix. p. 173) the advisability of recording earth-currents in Webers. Comparatively few of the readers of NATURE would appreciate the magnitude of an earth-current if expressed in those terms, while every one, I think, will understand me when I say that the earth-current passing through the line equalled that which would be produced by a certain number of cells connected to the same circuit.

The systematic observation of earth-currents in different parts of the world is no doubt very desirable, but to be of value it must be regulated and collated by some central authority. I feel convinced that if the Society of Telegraph Engineers invited assistance in this matter, and pointed out what was actually required, the appeal would be very readily responded to.

Kurrachee, February 6 HENRY C. MANCE

Intellect in Brutes

IF Mr. Henslow will read my letter again he will find it distinctly stated that the "several occasions" on which the leakage took place were referred to in connection with the agency of rats only. The plumber informs me that in none of the cases (four or five) was there any sign of injury to the pipe by frost. In the specimen which I have, the rats have made two *ineffectual* attempts to perforate the lead, and have succeeded in two distinct places. Had a frost crack existed, with consequent escape of water, there would have been no necessity to make two fruitless attacks on the pipe elsewhere. The specimen may be seen by any one interested at the office of *The Country*, 170, Strand. Metaphysicians will probably think that Mr. Henslow has stumbled into a quagmire in his discussion of "practical" and "abstract" reasoning. Does he believe that brutes and boys in common have nothing but the faculty of "practical" reason? When a boy finds the value of x in a simple equation, is he not dealing with "abstract" ideas? ARTHUR NICOLS

I AM not opposed to Dr. Darwin's teachings, nor do I care much whether science proves that man is descended from Adam or from some extra clever race of monkeys, so long as the *truth* is established. In regard to the explanation given at p. 365 of the rats eating the pipe to get at the water because they "heard the water trickling," I am inclined to look at the matter in a simple matter-of-fact way, and so feel inclined to think they cut the pipe because it was somehow in their way. Lead and block-tin gas-pipes are found cut in a similar way. Now, are they cut to get at the *gas?* Lead waste-pipes are also often found so cut, both from the outside and inside. I happen to be a practical plumber myself, and have had to deal with rats in many ways, but I scarcely think that "the reasoning power of the rat" in this case has been properly reasoned out.

21, Renfrew Street, Glasgow, March 1 W. P. BUCHAN

I BELIEVE "that the reasoning faculty in man and animals differs in degree only." But I do not think Mr. Nicols' plumber's lead-pipe case (NATURE, vol. xix. p. 365) a well-tested instance of rat sagacity. We have not sufficient proof that the rats gnawed the pipe for the purpose of getting at the water; though, of course, they used the water after having come upon it. It seems more likely that they gnawed the pipe because it obstructed their tunnelling operations; else why did they cut it in two separate places? Mr. Nicols says "a rat will not drink foul water." Neither will I when I can get better, but I am afraid I should need to put up with the foul if I lived in a sewer.

Cambuslang HENRY MUIRHEAD

It is somewhat difficult to understand Mr. Henslow's remarks on the above subject in NATURE, vol. xix. p. 385. He tells us that if the dog that rang the bell to fetch the servant to let him out of a room in which he was shut up, *had not been taught to ring the bell*, "it would have been *abstract reasoning*, but it was only practical." Further on he says that brutes never acquire "abstract reasoning."

The Arctic fox, by Mr. Henslow's own showing, appears to have used "abstract reasoning," because it had never been taught to cut the line attaching the bait to the trigger of the gun before taking the bait (of which I have seen several cases), or to dig a trench in the snow to avoid the shot. Can Mr. Henslow be a sportsman? If so, he ought to know that in the case referred to, to pull the bait downwards *out of the line of fire* was the only safe way for the fox to have acted, so as to get his head out of danger. Had he used what Mr. Henslow calls "abstract reasoning"—which, I presume, means pulling the bait, not the line, to *one side* out of the line of fire, the fox would certainly have been shot, as the bait could not have been moved more than four or five inches from the wooden stake through which the bait-line passes.

If Mr. Henslow really means that the fox should have shown his powers of "abstract reasoning" by going up to the line of fire between the gun and the bait, and then pulled the string until the gun went off, I think the chances of reynard's ever eating the bait would be very small indeed. I have known him do what showed equal or greater intelligence, namely, cut the bait-string, as already mentioned. JOHN RAE

Royal Institution, February 28

MR. HENSLOW, in his letter on this subject, complains that "brute reasoning is always *practical*, but never *abstract*." As an instance of what appears to me abstract reasoning in a dog, I beg to offer the following: A few years ago we had in our possession a terrier gifted with a propensity (probably instructive) for worrying the sheep that were put to graze in a field separated from our house by another field. Coming out of the house one day I observed this dog crossing the latter field, evidently intent upon a little amusement. I called him back, he obeyed; but when he came to a patch of brushwood which hid him from view, he cut straight across the field, under cover of the brushwood, to behind a hedge, and then pursued his course

towards the sheep field. What taught him that he could thus reach his game unobserved? A. J. A.

HERE is an instance of "instinct" which shows, I think, that there is no difference whatever between the reason of animals and that of men.

A mare here had her first foal when she was ten or twelve years old. She was blind of one eye. The result was that she frequently trod upon the foal, or knocked it over when it happened to be on the blind side of her, in consequence of which the foal died when it was three or four weeks old. The next year she had another foal; and we fully expected that the result would be the same. But no; from the day it was born she never moved in the stall without looking round to see where the foal was, and she never trod upon it or injured it in any way. You see that reason did not teach her that she was killing her first foal; her care for the second was the result of memory, imagination, and thought, after the foal was dead and before the next one was born. The only difference that I can see between the reasoning powers of men and of animals, is that the latter is applied only to the very limited sphere of providing for their bodily wants, whereas that of men embraces a vast amount of other objects besides this.

The above limitation does not, I think, apply strictly to domestic animals, dogs especially, which seem to acquire some perceptions beyond mere animal ones.

Hull, February 28 C. W. STRICKLAND

Parhelia

ALLOW me to record the occurrence of parhelia here this morning. The phenomenon lasted about twenty minutes, and was fairly brilliant. No halo was apparent, merely a mock sun on either side of the true one, and the line passing through the three, dipped towards the south at an inclination of 2° to 3° to the horizon. E. W. PRINGLE

Uxbridge, March 4

Unscientific Art

As a specimen of Unscientific Art, let me bring to the notice of your readers a two-page engraving in the last number of the *Illustrated London News*, entitled "Capture of Sirayo's Stronghold."

If there is any truth in the laws of perspective the Zulus flying before the cavalry are indeed "sons of Anak."

Scientific Club, March 2 E. W. PRINGLE

Bees' Stings

IN NATURE, vol. xix. p. 385, a correspondent asks whether the identity of bee-poison with formic acid has yet been determined. Some sixteen years ago I made a few experiments with the poison from wasp-stings, and found, to my astonishment, that it was invariably alkaline instead of acid. A living wasp, duly held in the cavity of a perforated cork, was easily induced to sting a piece of turmeric paper; a brown-red spot immediately appeared. A. H. CHURCH

Cirencester, March 1

A NEW PROCESS IN METALLURGY[1]

LONG before human art acquired the knowledge of metal-making, prehistoric man had learned to make fire of the dry stems and branches of trees; in the charred fragments of half-burnt wood we recognise a form of carbon, the first simple elementary body produced by man from the complex natural bodies with which he was surrounded. In the knowledge of the use of fire, then, was the first dawn of art, particularly of that art which deals with the reduction of simple bodies from compound minerals. To convert metallic compounds into metallic elements is the domain of the metallurgist, and the means by which this is effected constitute the basis of metallurgic art. Carbon was thus a necessity to metallurgy—with the knowledge of fire the world emerged from the stone age. From those early times down to the present day, no fusion has been effected without using carbon, which in the form of wood, coal, or charcoal, has been the substance invariably used by the metallurgist for the production of heat, and to enable him to decompose and to smelt metal-bearing materials.

The new process, however, we are about to describe, has for its object the smelting of metalliferous substances without the employment of carbonaceous fuel. The sulphides of iron, lead, and zinc are known to be combustible substances of almost universal occurrence, and when burnt under favourable conditions give rise to a great evolution of heat. We have calculated the relative temperatures thus produced, from which it appears that the temperature at which iron pyrites (bisulphide of iron) burns in air under the conditions most favourable to the development of a high temperature is over 2,000° C., protosulphide of iron burning at about 2,225° C. Zinc sulphide, or blende, gives a temperature of 1,992° C., and galena 1,863° C.; while calculations made in a similar manner with coal, assuming it to be completely burnt, show the temperature attainable to be 2,787° C. These mineral sulphides, which are therefore natural and almost inexhaustible sources of heat and energy, can under certain circumstances be burnt more economically than their heat-giving equivalent of coal.

The best means, however, of utilising this heat-producing property of metallic sulphides is not so apparent as would appear at first sight. Only iron pyrites is sufficiently combustible at a low temperature to burn in the open air, the mass being raised to the temperature at which the oxidation takes place solely by the union of the sulphur and iron with aerial oxygen. In Spain this is carried on in vast heaps of hundreds of thousands of tons, and the operation extends over many months. The oxide of iron that remains is typical of those mineral substances which, once burned in the primeval operations of nature, gave up their stores of heat and force, and became, as it were, inert bodies.

Going back now to the combustion of carbon, it is well known that it burns at widely varying temperatures, as, for example, in our bodies, in a common coal fire, or in a powerful furnace. A great deal of attention and thought has been spent upon the subject of the economy of carbonaceous fuel, and great advances have been made in this direction, yet the expenditure of coal or coke necessary, say, to melt a given quantity of metal, still far exceeds the theoretical limit. The main causes of this discrepancy may be accounted for as follows:—(1) That only a fractional part of the oxygen of the air passed into the furnace acts upon the material to be burnt. (2) That the oxygen is not brought in contact with the combustible matter with sufficient rapidity to attain the necessary temperature for the operation. (3) That gases pass off hot and unburnt; these are now, however, frequently utilised.

There is one metallurgical operation in which the first two sources of loss are perfectly avoided—namely, by blowing air through molten crude iron, as in the Bessemer operation, where, by the burning of small quantities of carbon and silicon contained in the crude iron a very high temperature is attained, which is not the case in the process of puddling, where the oxidation is spread over a considerable period of time, although the same constituents are frequently burnt in similar proportions. But even in the Bessemer process the carbon is only half burned, and a large amount of heat escapes with the carbonic oxide and nitrogen. When, however, we blow thin streams of air through molten sulphide of iron lying upon a tuyère hearth, a high temperature is produced by the perfect combustion which ensues in the midst of the sulphides, and no unburnt gases excepting sulphur vapour escape from the surface of the molten mass. Hot nitrogen and sulphurous acid being the only gaseous products

[1] A paper with full details of the process was read by John Hollway at the Society of Arts on February 12, 1879; Prof. H. E. Roscoe, F.R.S., in the chair.

of the operation (excepting the small quantities of hydrogen from the aqueous vapour of the air), these may be caused to act upon iron pyrites and other mineral matter. When pyrites is thus heated, an atom of sulphur held in feeble combination is in great part expelled, and thus is obtained protosulphide of iron, with which the operation commences, and which can exist in the molten state. Sulphide of zinc thrown into this bath of molten sulphide is converted into oxide: the sulphides of copper, nickel, and silver do not burn at all so long as sulphide of iron is present, and, accordingly, if oxides, silicates, or carbonates of these latter metals are introduced into the molten sulphide of iron, the iron present will take away the oxygen with which the metals are combined and concentrate them into a regulus of sulphides. But the question then arises, How, after fractional decomposition by oxidation, we can separate the sulphides from the oxides? This is accomplished by the addition of siliceous matter introduced into the furnace with the charge of sulphides, so that in the manner explained are obtained from crude materials five principal classes of products, viz.:—(1) sulphur; (2) sublimates of volatile sulphides and oxides; (3) a slag of silicates of certain more oxidizable metals, principally iron; (4) regulus containing the nickel, copper, and silver; (5) sulphurous acid and nitrogen. Under certain circumstances a sixth class of products may be obtained consisting of the metals copper and lead. Thus, when the sulphides of iron and copper present in the bath are treated continuously with the blast of air without the addition of combustible sulphides, a point at length arrives when the whole of the iron present is oxidised, and the regulus in the bath consists of subsulphide of copper. If now a limited supply of air is introduced, the copper is reduced to the metallic state, with the evolution of sulphurous acid. Further experience in the matter may lead to the adoption of this continuation of the process. Again, sulphide of lead present in the bath may be caused to yield metallic lead by partial oxidation. The sulphurous acid can be made into sulphuric acid in chambers or condensed to the liquid state. Thus we have in this new process a metallurgical operation, the necessary heat for the decomposition and fusion being entirely obtained by the combustion of the iron and sulphur contained in the materials operated on.

Some large experiments have been made in order to prove the more important points here enunciated. They are all to be found described in the paper upon the subject in the *Journal* of the Society of Arts, dated February 14 and 21, 1879. A brief record of some of the phenomena witnessed at the February experiments at Penistone may not be uninteresting. At seven in the morning on February 12 last a small party of gentlemen arrived at Messrs. Cammell & Co.'s Penistone Steel Works, in order to see the operation from its very commencement. Two Bessemer converters were ready for the experiments; one of these was charged at 10 A.M. with some molten protosulphide of iron (made by fusing some pyrites in a cupola), and a blast of air was driven through the tuyères. Lumps of sandstone were continuously thrown in together with cupreous pyrites. A flame of the burning vapour of sulphur expelled from the pyrites passed from the converter mouth to the chimney shaft; it was from 6 to 10 feet long, blue at the edges and greenish in the body of the flame. About noon this experiment broke down through an accident, after which the product was taken out. An experiment was then commenced by setting fire to some sulphide of iron by means of about 2 cwt. of coal thrown into the vessel to start the combustion; pyrites and sandstone were then thrown in, in lumps, which rapidly melted, this being continued until midnight (over eight hours). The molten mass in the vessel remaining perfectly liquid, was from time to time partially poured out to make room for the addition of further similar materials. During the whole of the eight hours not an ounce of coal was used, the converter being "fed with stones," and "vomiting forth fire and brimstone," as a gentleman present graphically expressed it. In this latter experiment about eighteen tons of raw pyrites was thus treated, and over four tons of sulphur was distilled and afterwards burnt. More than half a million cubic feet of sulphurous acid and nitrogen left the mouth of the converter at a high temperature, taking away with them a considerable fraction of the heat produced by the oxidation. This was very unfavourable to the success of the experiment, as will be readily understood when this great loss of heat is taken into account. With a suitable plant the heated gases would be utilized to drive off sulphur from pyrites, so as to produce the molten protosulphide required to continue the operation. Heat is not only obtained by the oxidation of the metallic sulphides, but also by the oxidation of iron protoxide to peroxide when the contents of the vessel are over-blown. In an experiment made in July last the oxidation was thus purposely continued. "As soon as the subsulphide of copper began to burn a splendid emerald green flame suddenly appeared, lasting about a minute, and all the lines except those of copper and sodium left the spectrum. During the last few minutes of the blow the mouth of the converter was dull and without flame."

Some of the products of these experiments were shown at the Society of Arts; they consisted of crystalline masses of ferrous silicate and blocks of 50 per cent. copper regulus. No sulphur was collected, it being impossible to do so with Bessemer plant, which, in actual operations, will not be used for the process. These experiments, however, enabled those present to witness, in the course of a few hours, the principal effects produced. "A remarkable spectrum was obtained from the burning sulphur vapour; viewed through a small direct vision spectroscope, many absorption bands were seen occurring at apparently regular intervals from the red to the violet. The lines of sodium, lithium, and thallium were recognizable, but the majority of the lines are of (as yet) unknown origin, though they are the most important, since the changes furnish indications of the progress of the chemical changes taking place in the vessel. The lithium was, probably, derived from the sand introduced with the pyrites."

The process is peculiarly suitable—(1) For the treatment of metalliferous substances which cannot be advantageously utilised by other processes. For the extraction of sulphur by distillation, and simultaneously for the concentration and separation of copper, silver, and nickel from such materials in the form of a metallic regulus; while lead, zinc, arsenic, &c., accrue in the sublimates. (2) For the treatment of cupreous pyrites, large quantities of which exist in many parts of the world where fuel is scarce, and where the present mode of treatment by the cementation (wet) process involves not only the loss of vast quantities of sulphur, which is burnt to sulphurous acid, but causes the destruction of all vegetation within its influence. For example—About one million tons of pyrites, too poor in copper to pay for shipment to the United Kingdom, are annually treated in Spain by the cementation process. Such ores thus treated, containing 1½ per cent. of copper, leave only a small profit, whereas it is calculated that similar ores by this new process will yield a profit more than five times as great. (3) For the treatment of copper and nickel ores, so as to produce a concentrated regulus without employing carbonaceous fuel.

It is therefore obvious that this process will effect a great revolution in the treatment of metallic sulphides, such as iron, cupreous and nickeliferous pyrites, also copper and nickel ores and the refuse gangue of mining operations, which can thus be smelted without the employment of carbonaceous fuel, the necessary heat being obtained by the oxidation of the metallic sulphides.

ON THE FREEZING OF LAKES[1]

In pursuance of investigations, now extending over some years, into the natural conditions of our seas and lakes, I took advantage of the recent frost to make some thermometric observations in fresh-water lakes covered with ice.

Most of the observations were made with one of Negretti and Zambra's "half-turn" deep sea thermometers, which proved to be a useful instrument for this species of inquiry. It was necessary however to fit it with a suitable inverting contrivance, as the apparatus supplied for this purpose by the makers is quite useless. When this thermometer was accidentally disabled, thermometers on the Millar-Casella type with certain improvements introduced by myself were used.

Observations were made in Linlithgow Loch at different dates, and in Loch Lomond on January 28 and 29. The results are given in the accompanying tables.

TABLE I.—*Loch Lomond*

Depth. Feet.	Temperature, Fahrenheit, at Station.			
	No. 1.	No. 2.	No. 3.	No. 4.
3	33·00	33·50	33·60	33·70
6	33·50	33·70	33·70	33·80
Bottom 10	—	—	33·80	—
12	33·70	33·80	—	—
18	33·95	33·95	—	33·80
24	34·05	33·95	—	—
Bottom 27	—	33·95	—	—
30	34·15	—	—	33·95
36	34·35	—	—	34·35
42	34·60	—	—	34·55
48	35·20	—	—	35·05
Bottom 51	—	—	—	35·05
54	35·45	—	—	—
60	36·20	—	—	—
Bottom 65	36·30	—	—	—
Mean ...	34·46	33·74	—	34·05

TABLE II.—*Linlithgow Loch*

Depth. Feet.	Temperature, Fahrenheit, at Station.					
	No. 1.	No. 2.	No. 3.	No. 4.	No. 6.	No. 7.
3	34·90	—	—	35·90	36·00	36·00
6	35·25	36·10	36·00	36·30	36·60	36·80
12	37·15	36·80	36·85	36·80	37·35	37·50
Bottom 16	—	—	37·40	—	—	—
Mud 16	—	—	37·80	—	—	—
Bottom 16½	38·50	—	—	—	—	—
18	—	36·95	—	36·90	37·35	37·80
21	—	—	—	—	—	37·80
24	—	37·30	—	37·30	37·50	38·15
30	—	37·40	—	37·40	37·90	38·30
36	—	37·60	—	37·70	38·45	39·00
42	—	—	—	38·40	39·80	40·70
44	—	38·60	—	—	—	—
45	—	—	—	—	—	42·00
Mud 46	—	—	—	39·85	—	—
Mud 47	—	39·75	—	—	—	—
Mud 48	—	—	—	—	41·70	{42·05 42·00
Mean...	—	—	—	37·22	37·83	38·28

[1] Substance of two papers read before the Royal Society of Edinburgh on January 20 and February 17, 1879.

Had the freezing of the loch taken place according to the commonly received idea, that is, had its waters been first reduced uniformly throughout its depth to the temperature at which the density of water is a maximum, and the surface layer then cooled further until a covering of ice was formed, we should have expected to have found the *remains* of this uniform temperature on examining the water after a firm coating of ice had formed. Distilled water reaches its maximum density at 39·2° Fahr., and I naturally expected to meet with a considerable stratum of water at or near this temperature. Both in Linlithgow Loch and in Loch Lomond there was a tendency to uniformity in the temperature of the water, but in Linlithgow this temperature was approximately 37° F. and in Loch Lomond 34° F. A single glance at the curves of the observations in these two lakes shows that they could never have been developed, if at the time of the formation of the first coating of ice the bulk of the water had been at a uniform temperature of 39·2° F.

In order to explain the existence of this unexpectedly low temperature, I at first imagined that there might be sufficient saline matter dissolved in the water to lower its temperature of maximum density. The presence of five parts of common salt in one thousand parts of water would have sufficed to lower this temperature to about 37°, and as the water of Linlithgow Loch was otherwise excessively foul, it appeared at first sight to be a likely explanation. It was not however verified by experiment. Although possessing a most offensive odour, the water was remarkably free from saline ingredients, and when its change of volume at low temperatures was compared with that of distilled water in the same dilatometer no difference could be detected. Seeing then that the temperature of maximum density was the same as that of distilled water, it was evident that, before being covered with ice, the whole of the water had been cooled down much below that temperature, and that this effect had been produced in a still more marked manner in Loch Lomond.

Let us consider what would be likely to take place during the cooling and freezing of a lake such as the frozen part of Loch Lomond. The water would be cooled down gradually by radiation from the surface, and we may admit that at some date, probably early in December, the whole water from surface to bottom would have a sensibly uniform temperature of 39·2° F. I believe that even in a very small lake there would be no date when the whole of the water would be uniformly at the temperature of maximum density, but it is a condition to which it would approximate in a greater or less degree, according to local circumstances. As the cold continued, circulation would be completely stopped, and cooling would be confined to the surface-layer, supposing the climate of the surface to be absolutely identical all over the surface of the lake. It is impossible that this condition can be fulfilled for more than an instant of time, for it would be disturbed by the slightest movement of the atmosphere. As a matter of fact there is great diversity of climates even at points close to each other, causing among other effects great variations in the temperature of the surface water. Did lakes cool absolutely uniformly throughout their whole extent, there would be no reason why ice should begin to form in one part more than another, and the ice would begin to form at one and the same moment all over the lake, which is contrary to experience.

Let A B C (Fig. 1) represent the bed of a lake in section, D E the surface of the water. At the date when the water has approximately the uniform temperature 39·2° F. D E will represent the isothermal of 39·2. Now let the cooling go on and let the first ice appear, as it naturally would, at the edge. Let E F represent the first piece of ice, which for simplicity's sake we may suppose to have been formed suddenly, and let us consider the effect of its

presence. This effect would be expressed graphically by the dipping of the isothermal of 39·2, as at G, and the generation of those of temperatures intermediate between it and 32° F. This alteration in the temperature means also an alteration in the density; and if we consider a vertical section through the ice at H K and through the middle of the lake at L B we shall find the mean density at L B greater than at H K, and the result would be the production of convection currents. What takes place at E would take place on the other side at A also, and we should have a system of circulation which in broad outline would consist of surface currents from the sides towards the middle, and under currents from the middle towards the sides, somewhat after the manner indicated in Fig. 2. The length of time that these currents would continue to flow before the lake was covered over with ice would depend on a number of local circumstances. The shape, size, and position of the lake would have much influence, and also the severity of the frost. However low the temperature of the air, the strength of the currents would always diminish as the water got cooled down, for the lowest available temperature is 32° and the highest 39·2°. Hence the convection currents would be the strongest at the first formation of the ice, and would gradually, and at an increasing rate, get weaker as the general temperature of the water got lower. When the whole of the water was cooled to 32° they would stop altogether.

It is therefore certain that in the water of a frozen lake we must find a tolerably uniform temperature, and this temperature must lie between 39·2° and 32°. In order that either of these extreme temperatures should

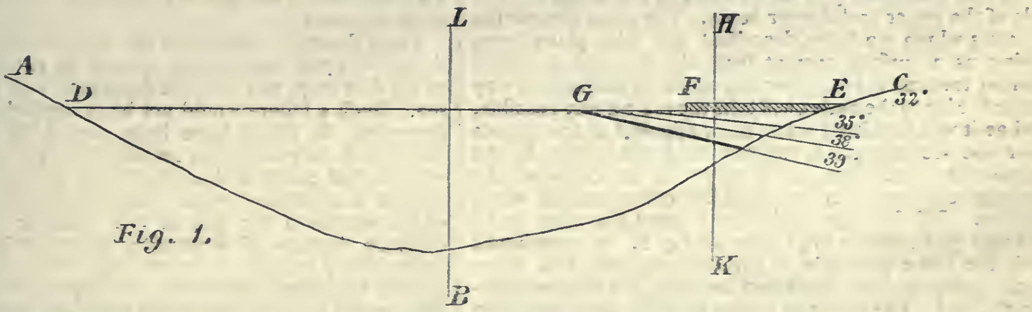

Fig. 1.

prevail, the weather would require to be of a very exceptional character. Admitting that the water had been cooled to a uniform temperature of 39·2°, this temperature could be *fixed* only by a sudden frost of extraordinary and probably unknown intensity. As the convection currents become weaker the more nearly the temperature of the water approaches 32° it would require the least possible frost during an infinite winter to reduce the temperature of the water to 32° before it was covered with ice. The uniform temperature of 32° however could be produced in another way, namely by the cooling of the water after it was covered with ice. This condition is probably not uncommon in shallow lakes in very cold climates.

In general, in climates such as our own, an intermediate temperature would prevail. In the Balloch basin of Loch Lomond this temperature was found to be about 33·9° F. In Linlithgow Loch it was much higher, about 37° F. The lake will remain open with an ice-fringe along the side as long as the water leaves the ice-edge with sufficient velocity to mix with the warmer water off shore before freezing under the influence of surface radiation. As soon however as this velocity is reduced so far as to enable the frost to overtake the water as it leaves the ice-edge before mixing with the warmer water the propagation of ice from the fringe out into the middle of the lake will take place with great rapidity, and a single night will often be sufficient to cover a large lake.

From the moment of the formation of the complete ice covering, the water is subjected to a uniform climate, its surface being everywhere in contact with ice; and it is only under these conditions that the whole of the lake can be said to be exposed to an identical climate.

Hence we see that, even admitting that a uniform tempe-

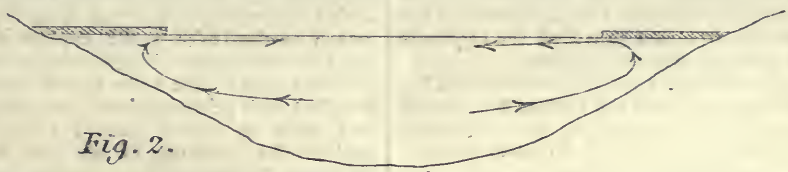

Fig. 2.

rature of 39·2° had previously existed, the whole of the water would be very considerably cooled down before it could be covered with ice, and the extent of this cooling would depend on local circumstances of position and climate, so that the final temperature of the water would be different for different lakes in the same winter, and for the same lake in different winters. The body of the water of a lake would be cooled more when it has been frozen by a moderate and comparatively long continued frost, than when the ice has been frozen quickly by very severe frost. For the more severe the frost the sooner will it be able to overtake the water leaving the ice fringe; in other words, the stronger will be the current which it will be able to arrest, and the greater the *head* which it will be able to stem. But the head which causes the current, is caused by the higher temperature of the open water as compared with that under the ice. Hence the more severe the frost, the higher will be the temperature which it will be able to fix. Let us now consider the causes which would tend to alter the temperature fixed by the ice covering.

In the first place there is alteration in the thickness of the ice and conduction from it downwards. The effect so produced would be concentrated in the surface layer, and its nature can be easily imagined.

In a large lake like Loch Lomond there is, even in severe weather, a considerable amount of change of water going on. The river Leven, which leaves it at Balloch, is navigable for small vessels, and a number of not inconsiderable streams fall in at different parts of the loch. The ice extended from the lower end at Balloch up to Luss, where it terminated in a miniature cliff which swept in a curve from the Dumbartonshire shore to the island of Inchlonaig and thence in another curve to the Stirlingshire shore. It is easy to see that the streams which fall

into the lake in its lower part when it is covered with ice, must supply water at 32° F; for even if it were at a higher temperature while forming part of the stream, it would, by impinging on the edge of the lake ice, be cooled down before being able to mix with its waters. It would thus enter the lake at 32° and would spread itself out immediately under the ice, and its effect on the temperature of the water would be similar to that produced by the thickening of the ice and conduction from it.

The excess of water which falls into the upper and open part of the lake must also find its way under the ice to the outlet. In order to estimate its effect, we must consider the conditions obtaining at the terminal edge of the ice stretching across the lake. We have already discussed what takes place when the first ice fringe forms at the sides of a lake, and we may look on the ice covering the lower and shallower part of the lake, as the fringe on that side of the upper and deeper basin. The same convection currents which we have described will take place here, giving us a surface current from the ice, and an under current towards the ice, now the water in finding its way under the ice to the outlet, will take the easiest way possible, and as it will find itself retarded or opposed at the surface near the ice edge, and assisted near the bottom by the convection circulation, its supply will be delivered by preference as an under and therefore comparatively warm current. Off the edge of the ice I found the water at 8½ fathoms 37·2°, and in 17½ fathoms 37·4° F. In accordance with the same principle of greatest easiness, this water would penetrate into the frozen basin by the deepest existing channel. The lower or frozen basin is separated from the upper and open one by a remarkable chain of islands separated from each other by channels everywhere less than there, and indeed hardly anywere more than two fathoms deep. The two channels however next the main land are each five fathoms deep, and it is through them that the bulk of the water finds its way. This was shown in a very singular way by the existence of a space of open water stretching from the main land to the nearest island over the shallowest part of the channel on the Dumbartonshire shore, there being thick strong ice over the deep water on both sides. In its passage over the ridge which rises very steeply, the warm water is thrown up near the surface and its supply is sufficient not only to preserve this space of open water, but also to raise the temperature of all the water of a depth greater than five fathoms in the lower basin.

In the case then of a lake only partially frozen, the temperature of the water under the ice is lowered by the drainage entering where it is frozen and is raised by the water supplied from the open part provided the channels connecting the two parts are not too shallow.

In Linlithgow Loch which was entirely frozen over, the very great rise of temperature near the bottom was caused by the immense quantity of filth contained in its water and in the mud at the bottom. J. Y. BUCHANAN

ON THE BURSTING OF THE GUN ON BOARD THE THUNDERER [1]

IN the interval which elapsed between the bursting of the gun and the report of the Committee much thought and some trouble has been expended in divining the possible causes which might, under one set of circumstances or another, have led to such a result. It now appears, however, that, different as have been the various suggestions, they all resembled each other in one particular, namely, that they were all wrong.

It is to be hoped, however, that all the ingenuity that has been expended will not have been thrown away, and that some improvement may result from the pointing out of such numerous defects. That in some respects, such as the increasing twist and the sudden steps or shoulders on the outside of the gun, the present system is defective, is shown quite apart from the recent accident; and although it now appears that the moving forward of the shot as the rammer was withdrawn had probably nothing to do with this accident, it cannot be considered satisfactory that this moving forward should be so much the rule as it is shown to have been in the experiments recently undertaken.

Although at first sight it may appear that the fact of the gun having been loaded with two charges of powder and two shot is amply sufficient to explain the bursting, it may not be useless to examine somewhat closely into what would result under such circumstances. The bursting of a 38-ton wrought-iron gun is an experiment of which we should make the most as we cannot expect to have it often repeated.

From the first accounts of the accident it appeared as though the gun had simply broken in two, like a carrot, at the first step, and that the front half had gone into the sea. Such a failure would not have implied an excess of pressure. It might have been caused by a great end strain such as would have resulted had the shot jammed when in full career and carried away the fore part of the gun, or it might have resulted from the gradual weakening of the section of the gun at the shoulder owing to the different degrees of expansion immediately before and immediately behind. One or other of these causes appeared to afford the most probable explanation of the phenomena as described in the early accounts. In various subsequent reports, however, it was stated that fragments of the fore part of the gun were blown about in all directions. So that the gun, instead of having simply broken in two, must have burst like a shell in front of the first shoulder. This fact placed the phenomena in an altogether different light. The explosive bursting of the zone of the gun into fragments implied an enormous excess of pressure at this point of the gun.

In order to cause the tube of the gun to burst longitudinally at all would require several times the normal pressure, and the breaking up of the wrought-iron tube into fragments would show that the force was largely in excess of what was necessary to burst it.

After seeing these reports it appeared certain that the gun had been subjected, at the point of rupture, to a pressure enormously excessive, and the question became whence could such a pressure have arisen? To me it appeared that nothing short of such an action as might, with a detonating fuse, result from the explosion of gun-cotton or dynamite would explain the breaking of the gun into fragments. Had the shot become jammed the pressure might have been raised sufficiently to burst the gun, but with pebble powder even this seemed doubtful, and such an action seemed altogether inadequate to explain the breaking of the gun into fragments. It appeared, therefore, that there was but one conclusion to be drawn—there had been something abnormal in the loading. Had the gun been loaded with small grained powder, gun-cotton, or dynamite, instead of pebble powder, such a result might have been produced; but then, the gun would, if it had burst, have burst at the breach unless the shot had slipped forward, and that there should have been two accidents appeared highly improbable. Besides, it was necessary to consider what sort of a mistake was most likely to have occurred; and the only possible mistake that could have been made on the spot appeared to be that of double loading.

The fact that if two complete charges were put into the gun, the powder of the second charge would be directly beneath the point of rupture appeared in favour of this the easiest mistake. But would, supposing the powder to have been pebble powder, the pressure from the two charges have been sufficient to cause the result? At first

[1] Read before the Lit. and Phil. Society of Manchester on February 18, by Prof. Osborne Reynolds, F.R.S., Professor of Engineering, Owens College, Manchester.

it seemed to me that even supposing that the second charge had been ignited by the first, which was doubtful, this would not explain the suddenness or magnitude of the pressure. But on further consideration it appeared certain that the second charge would not be ignited by the fire from the first; and it then became clear that in this very fact we should have an amply sufficient explanation of the excessive pressure.

My object in writing this paper is to point out the probability of this explanation, and so, if possible, to induce the authorities to test it. It occurred to me several days before the report of the Committee appeared, and in spite of the improbability of such a mistake as double loading, I could not shake off the conviction that it afforded the true explanation. As I have pointed out, the blowing into fragments of a wrought-iron tube implied an explosive action such as might result from guncotton or dynamite, but which could not be produced by the slow burning of pebble powder. The point to be explained, then, is how the second charge could be brought into such a condition that it would explode like gun-cotton. To understand this, it must be remembered that in the usual way the grains of gunpowder burn from their outside only, so that the thicker the grains the longer will be the time occupied in burning, and for the same weight of powder the slower will the gas be given off. The reason why gun-cotton is so much more destructive than gunpowder is not that it gives off more gas weight for weight, but that when ignited by a flash it burns so much quicker. If, therefore, by any means the whole mass of gunpowder could be heated up to the firing point at the same instant, so that the grains fired simultaneously inside as well as out, the action of the powder would be as quick or quicker than the gun-cotton. And, still further, if besides being heated the powder was compressed into a fraction of the space it usually occupies, the gases so confined would be capable of a still greater pressure.

Now if the after cartridge were fired and the forward cartridge were not ignited by the flash, and considering the length and fit of the shot, it could hardly have been so ignited, then the after shot would be driven forward closing on to the forward shot and compressing the powder between until the pressure on the forward shot was at least half as great as the pressure of the gases behind the after shot, which would be between ten and twenty tons on the square inch. Thus the powder would be subjected to a squeeze between the two shot such as would result from a blow. It would be compressed to a fraction of its former volume. The cubes would be crushed into a cake and the work of compression would be sufficient to heat the powder far beyond its point of ignition. Thus the entire mass of powder would be simultaneously ignited in a highly compressed and heated state. The force of such an explosion would be practically unlimited and would be located at the very point at which the gun burst. Hence in such an action we have ample cause for the effect produced.

But it will be asked why does not the same thing happen when a rifle is doubly loaded? It is said that in that case the second cartridge is generally blown out before it ignites, and this may be so, for in the rifle the intensity of the pressure of the gas on the shot can never exceed above a twentieth part of what it is in the 12-inch gun, and hence in the case of the rifle the pressure may well be insufficient to ignite the powder between the shot.

This view of the action resulting from the firing of powder by percussion appears to me to be one which it would be well worth while to test, for if proved it would completely re-establish confidence in the strength of the guns, which has been somewhat rudely shaken.

Let a 12-inch gun be loaded with a double charge of powder and a double charge of shot, or a shot of double weight, and fired. If, as is probable, the gun does not burst, confidence in the gun will be re-established. Then let it be loaded twice over with the powder between the shot so as to ascertain whether the action of the powder when fired by percussion would not produce an effect similar to that which we are here considering. The destruction of one gun for the purpose of establishing confidence in all the rest would not seem to be an unworthy sacrifice.

MOSELEY'S NATURALIST ON THE "CHALLENGER"[1]

THIS is certainly the most interesting and suggestive book, descriptive of a naturalist's travels, which has been published since Mr. Darwin's "Journal of Researches" appeared, more than forty years ago. That it is worthy to be placed alongside that delightful record of the impressions, speculations, and reflections of a master-mind, is, we do not doubt, the highest praise which Mr. Moseley would desire for his book, and we do not hesitate to say that such praise is its desert. The same argus-like power of observation, the same readiness to appreciate the true interest and significance of every seemingly little fact, the same energy and indomitable perseverance in gathering information and material from every source in the short space of time at the circumnavigator's disposal which distinguished Mr. Darwin, characterise also his disciple and follower in many a distant ocean land and tropical forest.

Before the *Challenger* expedition set sail, Mr. Moseley was known as an accomplished biologist, trained in the laboratories of Stricker, of Vienna, and of Ludwig, of Leipzig. He had taken part in the eclipse expedition to Ceylon, and besides making valuable spectroscopic observations on the sun, had found time when there to study and collect specimens of the land Planarian Worms, the structure of which was the subject of a memoir by him in the *Philosophical Transactions* (published after the *Challenger* had left in 1874) which threw altogether new light on such important matters as the nature of metameric segmentation and the origin of the blood-lymph space, cœlom, or body-cavity of higher organisms generally.

During the *Challenger* expedition, and since its return in 1876 (when he was by special statute elected to a "research" fellowship by the members of his old college of Exeter, in Oxford), Mr. Moseley has, apart from this volume and its varied contents, produced a series of original memoirs published chiefly by the learned societies of London, which have been the means of making known the most important of the results to which the *Challenger* expedition has led in the field of biological science. It is to his industry and skill, combined with the opportunity afforded by the *Challenger's* cruise, that we owe the thorough description of the anatomy of the worm-like land-living Peripatus, and its development, studied by him at the Cape of Good Hope (*Philosophical Transactions*, 1874), whereby a totally new light is thrown upon the relationships of the great group of Hexapodous and Myriapodous insects, and the origin of tracheæ; to him we are indebted for the discovery and description of the most remarkable among the many pelagic or surface animals taken by the *Challenger* on the high seas, the transparent Pelagonemertes (*Annals* and *Mag. Nat. Hist.*, 1875), as well as for the detection of the only really markedly aberrant form of life dredged by the *Challenger* in deep waters (*Linnean Transactions*, 1878), the Ascidian, Octacnemus bythius. The colouring matters, also, of various marine animals have been studied by him with the spectroscope and the spectra, carefully recorded in the *Quart. Journ Microsc. Sci.* 1877. But of still greater importance and merit was Mr. Moseley's study of corals allied to Millepora and

[1] Notes by a Naturalist on the *Challenger*. By H. N. Moseley, M.A., F.R.S., Fellow of Exeter College, Oxford.

Stylaster, previously unknown (or nearly so) in the living state, although familiar as dry and bleached museum specimens. These, when freshly dredged by the *Challenger*, were treated by Mr. Moseley with those subtle devices known only to trained histologists, and as a result, he has been able to give the full anatomy of the soft parts of these corals, to show that they are compound organisms with variously differentiated "tentacular

FIG. 1.—The bird and the rat living together in the same hole.

polyps" (dactylozooids) and "mouth polyps" (gastrozooids), and that they constitute a new group of hydroids, and do not belong to the Anthozoa or ordinary coral-producing class of polyps. The results of this elaborate investigation, forming the Croonian lecture for 1878, have been recently published, illustrated with twelve quarto plates by the Royal Society.

Whilst thus actually producing the chief zoological

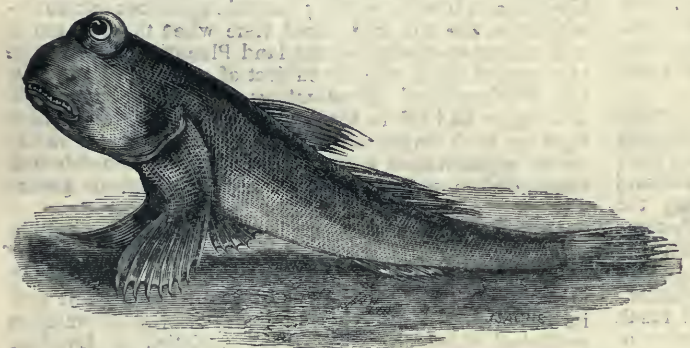

FIG. 2.—*Periophthalmus Kolreuteri* (on land; in the act of leaping).

results of the expedition, Mr. Moseley had specially undertaken the collection of plants, since no professed botanist was attached to the *Challenger*. The *Journal* of the Linnean Society, vols. xiv., xv., xvi., xviii., contain a large series of papers by Professors Oliver and Dickie, the Rev. M. J. Berkeley, and others, on the plants thus collected by Mr. Moseley's own hands on the islands visited by the *Challenger*. Not content with zoology

FIG. 3.—*Peripatus capensis* (natural size).

and botany alone, or rather, one should say, bringing his powers to bear on selected samples of the whole range of biology, Mr. Moseley has published the only anthropological memoir which has come to us from the *Challenger* staff—namely, an elaborate and careful account of an undescribed people—the inhabitants of the Admiralty Islands.

The results of all these researches are lightly sketched and often illustrated by woodcuts in the pages of the present volume, of which, however, they form but a limited portion. A still further development of biological science, namely, sociology—the history of civilisation, of manners, customs, and beliefs, is what the reader will find largely occupying Mr. Moseley's note-book, now published. And indeed, most entertaining and striking notes they are; the sayings and doings, the clothes and the amusements, the religions and the physical surroundings of Polynesians, Malays, Brazilians, Japanese, Chinese, seal-fishers, and English colonists, being set down as they impressed the observant mind of the author, accompanied by most trenchant comparisons and ingenious reflections which are characterised by a singular humour peculiar to him. Mixed with these, according to locality, we have, literally innumerable observations and suggestions with regard to such matters as basaltic columns, antarctic glaciers, flying-fish, fur-seals, phosphorescence, penguins, cockroaches, Kerguelen cabbages, land-crabs, and whales.

A few extracts will suffice to show that whilst Mr. Moseley's note-book will have special value for the professed naturalist, it is also eminently readable, and is likely to obtain great popularity amongst all those who have imaginations sufficiently vivid to allow their possessors to experience that intense form of pleasure which a good book of travels can generate. An enumeration of the titles of the chapters, to begin with, will show something of the distribution of matter in the book.

We have—I. Teneriffe, St. Thomas, Bermuda; II. Azores, Madeira, Cape Verdes; III. St. Paul's Rocks and Fernando do Norhona; IV. Bahia; V. Tristan da Cunha, Inaccessible Island, Nightingale Island; VI. Cape of Good Hope; VII. Prince Edward Island, the Crozet Islands; VIII. Kerguelen's Land; IX. Heard Island; X. Amongst the Southern Ice; XI. Victoria, New South Wales; XII. New Zealand, the Friendly Islands, Matuku Island; XIII. Fiji Islands; XIV. New Hebrides, Cape York, Torres Straits; XV. Aru, Ke, Banda, Amboina; XVI. The Philippine Islands; XVII. China, New Guinea; XVIII. The Admiralty Islands; XIX. Japan, the Sandwich Islands; XX. Tahiti, Juan Fernandez; XXI. Chile, Magellan's Straits, Falkland Islands, Ascension; XXII. Life on the Ocean Surface and in the Deep Sea, Zoology and Botany of the Ship, Conclusion.

Take the following description of a Penguin rookery at Tristan da Cunha (p. 120) as an example of Mr. Moseley's style. "It is impossible to conceive the discomfort of making one's way through a big rookery, haphazard, or 'across country,' as one may say. I crossed the large one here twice afterwards with seamen carrying my basket and vasculum, and afterwards went through a larger rookery still, at Nightingale Island. You plunge into one of the lanes in the tall grass, which at once shuts out the surroundings from your view. You tread on a slimy black damp soil composed of the bird's dung. The stench is overpowering, the yelling of the birds perfectly terrifying; I can call it nothing else. You lose the path, or perhaps are bent from the first on making direct for some spot on the other side of the rookery. In the path only a few droves of penguins, on their way to and from the water are encountered, and these stampede out of your way into the side-alleys. Now you are, the instant you leave the road, on the actual breeding-ground. The nests are placed so thickly that you cannot help treading on eggs and young birds at almost every step. A parent bird sits on each nest with its sharp beak erect and open ready to bite, yelling savagely 'caa, caa, urr, urr,' its red eye gleaming, and its plumes at half-cock, quivering with rage. No sooner are your legs within reach than they

are furiously bitten, often by two or three birds at once, that is, if you have not got on strong leathern gaiters, as on the first occasion of visiting a rookery you probably have not.

"At first you try to avoid the nests, but soon find that impossible: then, maddened almost by the pain, stench, and noise, you have recourse to brutality. Thump, thump goes your stick, and at each blow down goes a bird. Thud, thud, you hear from the men behind as they kick the birds right and left off the nests, and so you go on for a bit, thump and smash, whack, thud, 'caa, caa, urr, urr,' and the path behind you is strewed with the dead, and dying, and bleeding.

"But you make miserably slow progress, and, worried to death, at last resort to the expedient of stampeding as far as your breath will carry you. You put down your head and make a rush through the grass, treading on old and young haphazard, and rushing on before they have time to bite.

"The air is close in the rookery, and the sun hot above, and, out of breath, steaming with perspiration, you come across a mass of rock fallen from the cliff above, and sticking up in the rookery; this you hail as 'a city of refuge.' You hammer off it hurriedly half a dozen penguins who are sunning themselves there, and are on the look-out, and, mounting on the top, take out your handkerchief to wipe away the perspiration and rest awhile, and see in what direction you have been going, how far you have got, and in which direction you are to make the next plunge. Then, when you are refreshed, you make another rush, and so on.

"If you stand quite still, so long as your foot is not actually on the top of a nest of eggs or young, the penguins soon cease biting at you and yelling. I always adopted the stampede method in rookeries, but the men usually preferred to have their revenge, and fought their way every foot. Of course it is horribly cruel thus to kill whole families of innocent birds, but it is absolutely necessary. One must cross the rookeries in order to explore the island at all, and collect the plants or survey the coast from the heights."

Here is an example (p. 213) of the many observations which the book contains on the habits of birds and other animals:—

"An idea of the relations of the various birds to one another in the struggle for existence will be gained from the following incident:—I saw a cormorant rise to the surface of the water, and, lifting its head, make desperate efforts to gorge a small fish which it had caught, evidently knowing its danger, and in a fearful hurry to get it down. Before it could swallow its prey, down came a gull, snatched the fish after a slight struggle, and carried it off to the rocks on the shore. Here a lot of other gulls immediately began to assert their right to a share, when down swooped a skua from aloft, right on to the heap of gulls, seized the fish and swallowed it at once. The shag ought to learn to swallow under water, and the gull to devour its prey at once in the air. The skua is merely a gull which has developed itself by fighting for morsels."

Mr. Moseley has a great deal to say about the structure and natural history of icebergs in the chapter on the southern ice, and has illustrated this part of his book with two coloured plates and numerous woodcuts. The *Challenger* was in some danger here. "As the weather became worse, we were in a rather critical position. We were surrounded by bergs, with the weather so thick with snow, that we could not see much more than a ship's length, and a heavy gale was blowing. The full power of steam available was employed. Once we had a narrow escape of running into a large berg, passing only just about 100 yards to leeward of it by making a stern board, with all the sails aback, and screwing full speed astern at the same time. The deck was covered with frozen powdery snow, and forward was coated with ice from the shipping of seas. On February 28 again there were forty icebergs in sight at noon. It came on to snow thickly at about 4 P.M., and another gale came on. The plan adopted by Capt. Sir G. Nares was to lay down the bearings of the adjacent bergs before the weather became too thick for them to be seen, and then steaming with all the power of the ship against the gale, to hang on as long as possible under the lee of a large iceberg, and, when driven away from that, to steam rapidly across to the lee of another, the position of which was known by the bearings taken. So we went on steaming backwards and forwards through the whole of a thick, dark night."

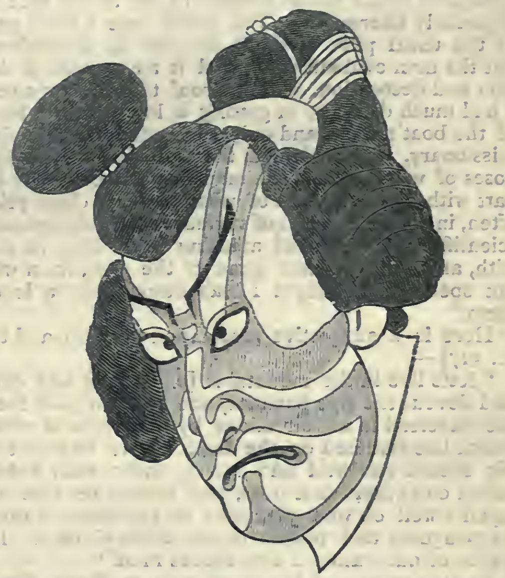

FIG. 4.—Face of Japanese Actor. (To show the mode of painting the face. From a Japanese Theatrical Picture-book.)

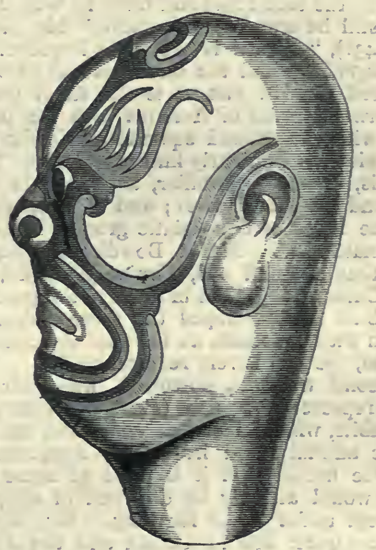

FIG. 5.—Head of figure burnt at Chinese funerals, made of paste-board. (To show the mode of painting the face.)

In warmer climes anthropology occupied, as we have said, much of the traveller's attention. The Tongans interested him by their expressive faces and gestures. A boat full of them was commanded by a noble, degraded from his rank by the missionaries, as a punishment for habitual drunkenness (p. 285).

"The coxswain of the pilot's boat, the ex-member of the nobility, wore, as I have said, a pea-jacket; a photograph was taken of the boat's crew. I could not persuade the coxswain to take off the pea-jacket, in order to make the group uniform [the others were quite naked, except a cloth round the waist]; he would only promise that if he were photographed with the jacket on in the group, he would allow himself to be taken with it off, separately afterwards. The jacket was a thick garment of the usual pilot cloth, fit only for an English winter, but the man evidently regarded it as a mark of distinction and decoration, and a proof that he was coxswain. I had much difficulty in getting a lock of hair from one of the boat's crew, and only succeeded by the help of a missionary, who explained that I did not want it for purposes of witchcraft. The man was also evidently loth to part with a single lock of what was his chief pride. I often, in collecting hairs of various races, subsequently, for scientific purposes, had amusing difficulties to contend with, and I suspect that some of the girls, from whom I got specimens, thought I was desperately in love with them."

Here is a suggestive association of man and the pig (p. 517)—

"Rats live in the mountains [of Tahiti], and climb up and devour the ripe bananas, and the groves of the trees are traversed in all directions by the tracks of wild pigs, which likewise feed on the fruit. It is strange that the pig should run wild and thrive, under such widely different conditions as it does, and should be able to exist equally well on wild plantains in the warm Tahiti, and on penguins and petrels in the chilly Crozets. In this power of adaptation it approaches man."

In his account of his short visit to Hong Kong and Canton, Mr. Moseley has much to say about the habits of the Chinese and their literature, medicines, and amusements. He reproduces several curious woodcuts from a Chinese work on natural history, the "Shan Hoi King." One of these (Fig. 1) represents, according to the description in the margin, "The Bird and the Rat which live together in the same hole. They come from the mountain of the tailed rats and birds in Wai Une, where they may still be seen." "No doubt," Mr. Moseley remarks (p. 431), "the rat is the ground squirrel (*Spermophilus mongolicus*), and the bird must be an owl, which is associated with it, just as is the small ground owl (*Speotyto cunicularia*) of America with the prairie dog and also with the ground squirrel of California, in the holes of which, as familiarly known, it lives. The genus Speotyto is, however, peculiar, as far as is known, to America and the West Indies; and the fact that an owl lives in the holes of the Asiatic ground squirrel is not known to naturalists. Mr. R. Bowdler Sharpe, however, tells me that a small owl, *Carine plumipes*, exists in northern China, which lives in holes in the ground. Possibly this bird has developed the same curious habit of association with a rodent, as has the American ground owl. If so, the fact is very remarkable."

Our second woodcut, borrowed from Mr. Moseley (Fig. 2), represents a fish (*Periophthalmus*) of very strange habits, which, like the land-crabs, though allied to aquatic animals, and irresistibly suggesting to the observer the notion that it is most at home under the water, yet would actually be drowned in all probability were it kept under water for long. Mr. Moseley has chased these queer fish in Ceylon and the Fiji Islands (p. 296). "They are very nimble on land, and difficult to catch. They use their very muscular pectoral fins to spring with, and, when resting on shore, the fore part of their body is raised and supported on these. There seems to be no figure of this very remarkable fish, which shows it at all in the attitude which it assumes when alive. The accompanying woodcut has been drawn from a specimen kindly lent to me by Dr. Günther, and I have put the fish as nearly in the natural position which it assumes when on land, as I can from memory." Space does not allow us to reproduce the excellent account of the Pearly Nautilus which follows here in the chapter on Fiji.

A good figure of Mr. Moseley's *protégé*, Peripatus, is given on p. 159, and is transferred to these pages (Fig. 3). A clear and intelligible account of the points of interest in the anatomy and habits of this caterpillar-like creature is given in the chapter on the Cape, and we read how both the author and von Suhm (one of the three other naturalists of the expedition, the other two being Sir Wyville Thomson and Mr. Murray) hunted high and low for specimens near Cape Town. Von Suhm "was unsuccessful; but I was lucky enough to find a fine specimen first under an old cart-wheel at Wynberg. Immediately that I opened this one I saw its tracheæ, and the fully-formed young within it. Had my colleague lighted on the specimen he would no doubt have made the discovery instead." It was, however, we take leave to assert, in spite of Mr. Moseley's modesty, no chance which brought the Peripatus to his hands, but, simply enough, the unwearying energy and ingenuity which characterised his proceedings throughout the voyage. At the Island of St. Thomas (p. 15) the party "heard of" Peripatus, but did not procure any. In New Zealand (p. 279) we again find mention of Peripatus; this time brought to Mr. Moseley's hands by a local naturalist, Mr. Locke Travers, F.L.S. Another result of Mr. Moseley's exertions at the Cape was his discovery of two specimens of the skull of the excessively rare and curious Ziphioid whale, *Mesoplodon Layardii*.

Tattooing and the use of paint as an ornament in China and Japan are amongst the subjects which Mr. Moseley discusses at length (p. 489), bringing a variety of facts together from his observations of Polynesians, as well as other races. The painting of the face by Chinese and Japanese is not similar to that practised by European ladies. "An even layer of white is put on over the whole face and neck, with the exception in Japan of two or three angular points of natural brown skin, which are left bare at the back of the neck as a contrast. After the face is whitened, a dab of red is rubbed in on the cheeks, below each eye. The lips are then coloured pink with magenta, and in Japan this colour is put on so thickly that it ceases to appear red, but takes on the iridescent metallic green tint of the crystallised aniline colour. In modern Japanese picture-books the lips of girls will sometimes be seen thus represented green. I suppose the idea is that such thick application of paint shows a meritorious disregard of expense. It is curious that the use of aniline colour should have so rapidly spread in China and Japan. In China, at least, such was not to be expected, but it seems to have supplanted the old rouge, and it is sold spread on folding cards, with Chinese characters on them, at Canton and in Japan."

"This form of painting the face seems to be exactly of the same nature as savage-painting, and possibly is a direct continuation of it. It is like the painting of our clowns in pantomimes. In China the faces of men (as opposed to women) seem not to be painted at the present time either on the stage or elsewhere; but in Japan, actors in certain plays are painted on the face with bright streaks of red paint, put on usually on each side of the eyes. The kind of painting is exactly that of savages (Fig. 4). It is a curious fact that this form of painting, surviving in adults on the stage, is still used elsewhere for the decoration of young children. It is quite common to see children on festive occasions, when elaborately dressed by their parents, further adorned with one or two transverse narrow streaks of bright red paint, leading outwards from the outer corners of their eyes, or placed near that position."

"Such a form of painting possibly existed in ancient

times in China. When a man of distinction was buried in China in former times, a certain number of servants were buried with him. Now, figures made of pasteboard and paper, about three feet or so high, are burnt at the funeral service, in small furnaces provided for the purpose in the temples, together with cartloads of similar pasteboard gifts, which are thus sent by the survivors for the use of the dead in the next world. Earthenware figures were similarly buried with great men in old times in Japan.

"The pasteboard heads of these funeral servants and retainers are painted with streaks, some of which are put on in almost exactly the same style at the angles of the eyes as those of modern Japanese actors. It seems a fair conjecture that the streaks on these heads (Fig. 5) are a direct survival of an actual former savage form of painting which was once in vogue in China, probably used to make fighting-men hideous. It is well known that primitive customs survive in connection with funerals all over the world with extreme tenacity. The numerous interesting survivals existing in the case of English funerals are familiar."

In connection with colour and decoration, we must draw the attention of breeders of poultry to the important experiment on sexual selection suggested on p. 373. Mr. Moseley in fact proposes to test the existence of a preference for colour on the part of hen-birds, by variously dyeing and manipulating the colours of two of more cock-birds kept with her.

The last chapter is one of the most interesting in the book, since here Mr. Moseley does not compress his wonderful richness of material into the short space which is necessary where he adopts the method of telling the reader all that he saw and thought about in one locality after another of the long list visited by the *Challenger*. Here he launches out more fully into discussion and gives a summary, intended for the general reader, of the most striking features presented by the life of the ocean surface, of the deep sea, and of the colony of cockroaches, rats, and other animals and plants which established themselves or were introduced on board-ship.

The significance of colour in marine animals is very suggestively treated, and the origin and use of phosphorescence likewise considered in an original manner. He says (p. 590):—

"The light emitted by phosphorescent animals is quite possibly in some instances to be regarded only as an accidental product, and of no use to the animal producing it; although of course, in some cases, it has been turned to account for sexual purposes and may have other uses occasionally. There is no reason why a constant emission of light should be more beneficial than a constant emission of heat, such as takes place in the case of our own bodies, and it is quite conceivable that animals might exist to which obscure heat rays might be visible, and to which men and mammals generally would appear constantly luminous."

The concluding paragraph contains a suggestion which could be carried into effect without expense by the Government, and there can be no question as to the naturalist best fitted to direct such an undertaking. Mr. Moseley says:—

"The urgent necessity of the present day is a scientific circumnavigating expedition which shall visit the least known inhabited islands of the Pacific, and at the same time explore the series of islands and island groups which yet remain almost or entirely unknown as regards their botany and zoology. These promise to yield results of the highest interest, if only the matter be taken in hand in time, before introduced weeds and goats have destroyed their natural vegetation; dogs, cats, and pigs, their animals and their human inhabitants have been swept away, or have had their individuality merged in the onward press of European enterprise. There is still, to the disgrace of British science, even in the Atlantic Ocean, an island, the fauna and the flora of which are as yet absolutely unknown. The past history of the deep sea, of the changes of depression and elevation of its bottom, is to be sought to a large extent in the study of the animals and the plants inhabiting the islands, which rear their summits above its surface. These insular floras and faunas will soon pass away, but the deep-sea animals will very possibly remain unchanged from their present condition long after man has died out."

Besides numerous woodcuts, Mr. Moseley's book is illustrated by two coloured plates of antarctic icebergs, and a track-chart of the world, with contour-colours of the sea-bottom. A very copious and carefully prepared index is appended. Throughout the book the references to literature of all kinds bearing upon the myriad topics touched upon are very abundant, and form one of the most intrinsically valuable features of the work.

E. Ray Lankester

METEOROLOGICAL NOTES

From the third annual Report of the Forest Meteorological Stations of Germany, being the Report for 1877, we learn that this system of inquiry into the influence of forests on weather and climate now includes fourteen stations scattered over a region extending over 7° of latitude and 5° of longitude, the stations being at heights ranging from 10 to 3,051 feet above the sea. The instruments and observations have been planned on satisfactory and comprehensive principles, and in a few years results eminently *ad rem* may be looked for. In the meantime the thermometric observations point to highly important results. Each station has three sets of thermometers for air temperature, similarly protected—one set in the wood, the second set high up in the crown of a tree, and the third set in an open space outside the wood, while earth thermometers are placed both in the open and in the wood, on the surface of the ground, and at depths of 6, 12, 24, 36, and 48 inches. The results show in every case a lower air temperature inside the wood as compared with the open country outside, the mean difference amounting to 1°·3. As regards the temperature of the surface of the ground, the mean deficiency in the wood shaded by the trees is 2°·5, an amount which gradually diminishes with the depth to 2°·0 at 48 inches, the lowest depth observed. It would be a problem of great interest to ascertain how deep this cooling of the earth's surface extends when it is screened by trees from solar and terrestrial radiation. What are called the "true means" of atmospheric pressure are calculated from the observations at 8 A.M. and 2 P.M., the formula being

$$\frac{\text{VIII.} \times 2 + \text{II.} \times 5}{7}.$$

Since the stations range in height from 10 to 3,051 feet, and otherwise differ in their physical conditions, it is scarcely necessary to point out that the method of reduction adopted is very faulty.

Some years ago a good deal of writing appeared in the periodical press depreciatory of the climate of Rome on account, as alleged, of the exposure of that city to the pestilential malaria of the Campagna. Many of the opinions then expressed will not bear scrutiny when confronted with the facts of the mortality and health of Rome. It was to counteract these opinions, which obtained wide currency, that a book entitled "The *Times* Newspaper and the Climate of Rome," by S. A. Smith, was recently published. The author has brought to his task the experience of a twenty years' residence, by which he has been enabled to sketch familiarly and with general truthfulness the broad features of its climate in its hygienic rela-

tions; and in addition, strong feelings, we may almost add of animosity, towards the writers whose opinions he sets himself to combat. The result is a readable book, containing much valuable information to those who intend visiting Rome, though occasionally marred by the introduction of hasty writing and hasty feeling. In comparing (p. 151) the mortality from typhoid in London in 1870 with that of Rome in 1876, the deaths in London are stated to have been 2,008, or 10 per cent. higher than the deaths in Rome; whereas the deaths from typhoid in London in 1870 were only 976, giving a mortality from this disease of little more than half that of Rome. Technical knowledge also on occasions fails the author; thus it is strongly asserted (p. 120) that the diurnal fall of temperature takes place almost exclusively between 3 P.M. and sunset, and between 9 or 10 P.M. and sunrise, but the two hours after sunset are those when the temperature is most nearly stationary. The mortality statistics, now published weekly by Cocchi, will soon supply information for a satisfactory handling of various questions which have been raised touching the health of Rome.

THE Missouri (U.S.) Weather Service Report for January last is just received (February 24), from which we learn that at St. Louis the temperature did not rise to freezing from December 11 to January 15; the mean temperature of the first ten days of January was only 9°·0, and that of the whole month 26°·9, or 4°·7 below the average; and at Oregon, in the north-west of the State, the sleighing season ended on January 25, after a continuance of fifty days. The lowest observed temperature was − 24°·0 at Phelps City on the 3rd, and at Columbia on the 4th, and temperatures nearly as low were noted on these days at many other places. The rain- and snow-fall was moderate in amount, being about two inches in the extreme south-east and south-west, and along the low country round the mouth of the Missouri, whilst in the north the fall was considerably under an inch of rain and melted snow. The cold of January, 1857, was much more intense than during last month, the mean temperature of that month being only 19°·3, or 12°·3 below the average. It is delightful to note the frank, effective manner in which Director Nipher is bringing about uniformity in his observers' reports; thus, after pointing out that "rainy" or "snowy" days are only those on which the rain or melted snow amounts to at least 0·01 inch, he adds that "this international rule is almost universally disregarded by our observers."

THE meteorological observations made at the Hydrographic Office at Pola during 1878 have been issued, with a full abstract for the year, showing the hourly means of pressure, temperature, and wind-velocity. The most prevalent winds by far are from the quarter of the compass from east-north-east to south-east, these comprising nearly half the winds of the whole year, to which there is to be added a small secondary maximum of west-north-west winds. The wind falls to its daily minimum velocity at 5 to 6 A.M., and rises to the maximum so early as noon, hours all but coincident with the daily maximum and minimum temperature. From the three years' observations now available from Pola, it is seen that in common with sea-side stations of the middle and higher latitudes, the A.M. maximum of pressure occurs later in winter than in summer, in contradiction to inland places where it occurs much earlier. Pola being in latitude 44° 52′ north, and thus within the belt to which Rikatscheff drew attention some time ago, as characterised by the occurrence, or tendency toward the occurrence, of a third maximum of pressure a little after midnight during the cold months of the year, it is interesting to note that four out of the nine individual Decembers, Januarys, and Februarys, show the occurrence of this third maximum, which also appears in the general means of December and January for the three years. The amount of this third maximum is very small, and the evidence yet adduced is not sufficient to determine whether it is a real increase of atmospheric pressure, or merely an apparent increase due to undetected instrumental errors.

OUR ASTRONOMICAL COLUMN

BRORSEN'S COMET.—Notwithstanding the track of this comet at the present appearance is a very favourable one for observation in these latitudes during April and May, the theoretical intensity of light at maximum is much less than that attaching to the first appearance in 1846; indeed, in the middle of April, when it is greatest, it is only half that of the middle of March 1846. The comet in that year was never a conspicuous object in ordinary telescopes; it approached pretty near to the earth, and on March 25 its apparent diameter was about 9′, corresponding to a true diameter of 126,000 miles.

The following positions for part of the present month are reduced to 7h. Greenwich time from Dr. Schulze's ephemeris, which has been calculated for Berlin noon:—

	R.A. h. m. s.	N.P.D. ° ′		R.A. h. m. s.	N.P.D. ° ′
March 10	1 29 4	90 15·4	March 18	1 55 56	82 59·8
,, 12	1 35 39	88 33·0	,, 20	2 2 51	81 0·0
,, 14	1 42 20	86 46·3	,, 22	2 9 50	78 55·8
,, 16	1 49 6	84 55·2	,, 24	2 16 52	76 47·3

The comet will be nearest to the earth (distance = 0·683) on the night of May 3, its position at the time being between 49 and 55 Camelopardi. Between April 14 and June 11 it will not descend below the horizon of Greenwich, attaining its greatest north declination (65° 30′) on May 11, in the head of Ursa Major. The comet was found by Dr. Tempel, as already stated, on January 14, more than a month before the ephemeris by Dr. Schulze commences, and as we remarked in a former note, was thus observed with a much less intensity of light than at any previous opposition. The error of the ephemeris has not yet been published, but it appears not to be large. We shall continue the ephemeris in due course when better advised on this point.

In his report upon the work of the Observatory of Leipsic in 1877, Prof. Bruhns mentions that Herr Harzer a student in that university had, at his request, redetermined the effect of the attraction of Jupiter upon the elements of the comet at the near approach of the two bodies in May, 1842, and with satisfactory results. In 1857 the late Prof. D'Arrest made a first approximation, by the method of the *Mécanique Céleste*, to the orbit which the comet described prior to the great perturbation or on entering the sphere of activity of Jupiter about April 19·5 Berlin time in 1842; the elements at that epoch were found to be as follows (we annex the elements in 1846 at the time of the comet's first discovery for the sake of comparison):

	1842, April 19·5.	1846, Feb. 25·4.
Long. of perihelion	133° 26′·7	116° 28′·2
,, ascending node ...	107° 44′·0	102° 41′·0
Inclination	40° 51′·0	30° 55′·9
Excentricity	0·59275	0·79338
Perihelion distance	1·50130	0·65013
Log. semi-axis major	0·56661	0·49783
Period	7·078 years.	5·581 years.

It will be seen that the perihelion distance before the encounter with Jupiter was much greater than it now is, a sufficient reason, as was pointed out by D'Arrest, for this comet to have been missed, if it had moved in the orbit which was so much changed in 1842.

MINOR PLANETS.—M. Palisa notifies his discovery of No. 192 at Pola on February 17. At 13h. 47m. m.t. its R.A. was 11h. 10m. 20s., and N.P.D., 84° 6′, eleventh magnitude.

Hilda, the most distant of the minor planets, which is

probably in the same region of the sky, has not yet been recovered. The planet which was named *Scylla*, and of which only four observations could be obtained at Pola and Berlin in November, 1875, will probably be difficult to detect again, since the observations, though inadequate to furnish elements with any pretensions to accuracy, sufficiently prove that the inclination of the orbit to the ecliptic must be pretty large. In case any one of our readers should be disposed to examine this point further, we subjoin the four observed positions reduced to longitude and latitude :—

	Greenwich mean time.		Longitude App. Eq.		Latitude North.
			° ′ ″		° ′ ″
1875, November	8·61578	...	48 5 2	...	1 58 41
,, ,,	9·42473	...	47 54 13	...	2 6 28
,, ,,	22·56605	...	44 54 44	...	3 53 18
,, ,,	23·44691	...	44 43 41	...	4 0 7

VARIABLE STARS.—Dr. Weiss, Director of the Imperial Observatory at Vienna, announces several new variable stars. One is Lalande 28607, which varies from 7·0 to 8·8 in a period not differing much from four months; this star is further to be noted for its large proper motion, $\Delta\alpha = -0^s{\cdot}080$s., $\Delta\delta = -0''{\cdot}35$. He also confirms variation in the neighbouring double star Lalande 28590, which had been suspected by Struve. Further, the stars in the *Durchmusterung*, $+17°$, Nos. 2510 and 2511, are found to be variable, the former from 8·8 to 10·0 in rather over eleven months, and the latter, also to the extent of about one magnitude, in a somewhat shorter period, about $9\frac{1}{2}$ months.

M. Ceraski, of the Moscow Observatory, also finds variation in the star, which appears thus in the *Durchmusterung*: mag. 9·2, R.A. 21h. 9m. 25s., Decl. $+67°$ 49′·5.

GEOGRAPHICAL NOTES

THE Council of the Royal Geographical Society have presented a remarkable memorial "to H.M. Commissioners of the University of Oxford, to those of Cambridge, and to the Governing Bodies of either University." The burden of this memorial is that steps ought to be taken for the establishment of professorships of geography in the two universities. The memorial points out forcibly and justly the ignorance of geography in its highest sense, in this country, where it is commonly confounded with mere topography. The Council of the Society, we are pleased to see, show that they possess an adequate conception of the position which geography ought to occupy, and which, indeed, it does occupy in the Universities of Germany, Switzerland, and France. We have often repeated that geography is really the meeting-place of all the sciences, and this is the idea which the Council endeavour to enforce upon the Commissioners and governing bodies of the universities. They show, how, to have an adequate knowledge of geography it is necessary to know something of both the biological and physical sciences, and be able to trace the mutual influence of man and his surroundings. The duties of such a professor as the Council desire to see appointed, the memorial states, would be first, to promote the study of scientific geography, and secondly, to apply geographical knowledge in illustrating and completing such of the recognised university studies as require aid. It is suggested, also, that he might deliver at least one annual discourse on some subject of geographical research. The memorial rightly states that there is no country that can less afford to dispense with geographical knowledge, but we doubt if the number of members of the Geographical Society is any evidence that we have a greater natural interest in the subject than other people. Certainly we ought to have, for our interests are as wide as the world; and as the memorial states, it would not be difficult to cite instances in which these interests have been seriously compromised by a want of geographical knowledge. Thus, that as a nation, we are far behind, both in our conception and in our knowledge of geography in its highest sense there can be no doubt, but whether this state of things is to be remedied by the founding of professorships of geography at Oxford and Cambridge is another question, which at present we cannot discuss. It appears to us at first sight as if it were beginning at the wrong end. Moreover, is not geography in its highest sense really only a branch of physiography, and would not the want in our university education be most effectually met by a professorship, or perhaps a lectureship, on that subject? At all events we are grateful to the Geographical Society for drawing attention to the importance and comprehensiveness which geography has assumed on the Continent, and to the lamentable want of interest in the subject which exists in this country.

ON the suggestion of the Bishop of Salford a committee has been formed in Manchester for establishing a Society of Commercial Geography. Mr. Armitage, the Bishop of Manchester, Mr. Arthur Arnold, Mr. Hugh Mason, Mr. Slagg, Mr. J. E. Taylor, and others, have joined the committee. We suggested some time ago the utility of forming such societies in our chief commercial centres, and we hope the example of Manchester will soon be followed by our principal seaports. That Manchester stands in need of some education in geographical matters was evidenced by the ignorance of African geography shown at the recent meetings to promote the formation of a railway from Zanzibar to the Lake Region. Similar societies have been found of great service in France. Might it not be well, however, if other towns form similar societies, that some common organisation be formed, and perhaps a common journal be published?

FROM the new Yellow Book of the Chinese Maritime Customs we gather some notes respecting the island of Hainan, the port of which, Kiungchow, has been recently opened to foreign trade. So far it has certainly not proved a commercial Eldorado, but what the real capabilities of the island are it is difficult to judge so long as the greater part remains a *terra incognita* to foreigners. This much, however, may be said in its favour, that it possesses an advantage over many islands of its size, viz., a large navigable river by which access may be gained to the interior, and which partially obviates the necessity for good roads. From the *Kiungchow Record* it appears that gold, silver, copper, tin, and loadstone are found in different parts of the island, but no mention is made of coal. The author of the report we allude to knows that peat exists, and samples of carbonate of copper have been shown him by natives. The number of different kinds of grain and other produce enumerated in the Chinese work referred to as growing in Hainan is surprising, and includes many varieties of rice, millet, Barbadoes millet, wheat, barley, beans, peas, sugar-cane, sesamum, ground-nut, taro, and yam. Of medicines (according to the Chinese pharmacopœia) exported the following are the chief items:—*Ai-fên*, a kind of camphor, obtained from the aborigines and said to be distilled from the leaves of the *Artemisia moxa*, *ho-hsiang*, stalks and leaves of *Betonica officinalis*, bitter cardamoms, cardamoms, the berries of *Abrus precatorius*, the stalks of *Dendrobium ceraia*, and tortoise-shell rind.

THE March number of the organ of the Geographical Society opens with the paper "On the Road to Merv," read at a recent meeting by Sir H. Rawlinson, which now appears, enriched with valuable notes, and illustrated by a map of the Turkoman Steppe and Northern Khorassan. Mr. C. R. Markham's paper "On the Basin of the Helmund, Western Afghanistan," is also published, accompanied by a well-executed map of the region. The Zulu-

land bibliography and cartography, which have been compiled with much care, will be found very useful at the present time, but it is to be regretted, perhaps, that a map of the country was not added. The geographical notes include accounts of M. Oshanin's further explorations in the Pamir, and of the Loochoo Islands. There is also an obituary notice of M. Nicholas de Khanikoff.

THE Archbishop of Algiers has received a detailed journal of the experiences of the French missionary expedition on its way to Albert and Victoria Nyanza, and Lake Tanganyika, which he has promised to communicate shortly to *Les Missions Catholiques.* When published in that periodical, it will be accompanied by a map of Equatorial Africa, prepared from original sources of information by Père Charmetant, under whose auspices the expedition started from the east coast.

AT the last meeting of the Society of Commercial Geography at Paris, M. Reclus communicated his report on the exploration of the Isthmus of Darien, conducted under the orders of Lieut. Wyse.

IN the *Bulletin* of the Lyons Geographical Society, which has just been issued, M. Luciano Cordeiro, the learned Secretary of the Lisbon Geographical Society, contributes a second instalment of his papers on the first explorations of Central Africa, and the Portuguese doctrine of African hydrography in the sixteenth century.

NEWS has lately been received by the German African Society from Dr. Buchner, a traveller recently sent out to West Africa. He proposed to leave Loanda towards the end of December for Dondo, on the Quanza, where Major Mechow is delayed by illness.

THE just received *Boletin* of the Madrid Geographical Society for October last contains a lecture by D. Francisco de Paula Arrelaga, on the physical geography of the sea. Also papers on Afghanistan, on Bulgaria, and other eastern countries by Sr. D. Saturnino Giménez, and an account of a journey to Morocco in 1800 by a Spanish Commission.

THE enterprise of Mr. James Gordon Bennett in preparing two vessels for arctic exploration, the one to proceed by way of Spitzbergen and the other by Behring Straits, is already well known to our readers. The steamer *Jeannette* has been assigned to the Behring Straits' service, and is, we understand, already in San Francisco. A bill has been introduced into Congress by Mr. Wood authorising the Secretary of the Navy to accept this vessel and take charge of her. The bill provides that it is to be fitted up with any material on hand, and authority is given to enlist the necessary crew and to provide the naval officers; and the expense, at least the pay of the men, will be subsequently refunded by Mr. Bennett. It is not improbable that this vessel will be made useful in the search for and relief of the *Vega*, Prof. Nordenskjöld's steamer.

NOTES

WE announce with the greatest regret the sad news, just received by telegraph, of the death of Prof. W. K. Clifford at Madeira. We can do no more this week than barely announce this national loss.

THE Emperor of Austria has bestowed upon Dr. Meyer, the director of the Royal Zoological Museum of Dresden, the well-known New Guinea traveller, the order of the Iron Crown.

THE death is announced at Berlin of the well-known chemist, Prof. Sonnenschein. He had reached the age of sixty-two.

THE Council of the Society of Arts have appointed Mr. H. Trueman Wood, Secretary, in the place of the late Mr. P. Le Neve Foster.

ACCORDING to the *Kreuz Zeitung*, Prof. Virchow intends, on the close of the current semester, to accept an invitation of Dr. Schliemann to join him in some excavations at Troy.

THE Italian Ministry of Agriculture, Industry, and Commerce offers a reward of 3,000 lire to the author of the most complete and best monographic essay on the structure, the vital functions, and the diseases of the acid fruits, or species and varieties of the genus *Citrus* and kindred genera, provided that the said work, by a sufficient collection of original observations and experiments, should succeed in furnishing an important addition to the present knowledge concerning such subjects, and thereby supply a scientific criterion for the improvement of the cultivation of these acid fruits and for the cure of their diseases. The date for sending in the works competing for the said prize is fixed for the end of May, 1881. Essays by Italians, or by foreigners written in Italian, are admissible to the competition; but if written in another language they must be accompanied by an Italian translation. The essays sent in for competition are to be sent to the Ministry of Agriculture, Industry, and Commerce, with the superscription, "Competition for the Prizes for the best Essay on the genus *Citrus*," and they must be distinguished by a motto, to be given also in a sealed cover containing the name and address of the author.

IT is intended to make a special effort to issue the Report of the Sheffield Meeting of the British Association at an early date after the meeting. To enable this to be done the Council request that all Reports and Abstracts of all Papers intended to be read in the Sections, may be sent to the Assistant-Secretary not later than July 15, in order that, if approved of by the Organising Committees, they may be put in type before the Meeting. Authors who comply with this request, and whose Papers are accepted, will be furnished *before* the Meeting with printed copies of their Reports or Abstracts. No Report, Paper, or Abstract can be inserted in the volume unless it is in the Assistant Secretary's hands *before the conclusion of the Meeting.*

DR. BOTTOMLEY has called the attention of the Manchester Literary and Philosophical Society to an interesting copy of the "Principia" of Newton. In addition to being an impression of the first edition, it contains the autograph of Edmund Halley. It was a present from Halley to the Abbot Nazari. Nazari was the editor of a scientific journal at Rome from 1668 to 1681. The following is the entry in Halley's hand-writing:—

Illustrissimo Dno
Dro Abbati Nazario
Romæ humillime offert
Edm. Halley.

Subsequently the book was in the possession of Dr. Dalton, and its value is enhanced by his autograph.

M. STEPHAN has been elected a Corresponding Member in the Astronomical Section of the Paris Academy, in place of the late Dr. Hansen, of Gotha.

WE have received one or two letters on the subject of migration of birds, referred to in Col. Donnelly's letter in NATURE, vol. xix. p. 289. Mr. H. Cecil thinks that the most hopeful way of carrying out Col. Donnelly's proposal would be through our consuls. "If a tabular sheet, noting in separate columns the points to observe—drawn up, say, by Mr. A. R. Wallace—were printed on thin paper and transmitted to our consuls abroad, with a request that they would fill them up and remit them to NATURE, my impression is that few would decline. In cases where the consul himself had not the inclination or the time accurately to fill in the paper, he could generally command the services of some one who could. This need in no way interfere with the independent notes of which your correspondent speaks." Mr. Cecil thinks that any funds required could easily be got by subscription. Mr. Allen Harker, of Gloucester, thinks that a student of migration has rather an *embarras de richesse* to contend with than a want of data. "The researches of Middendorf," he writes, "or the admirable work of Dr. Palmen, 'Om foglarnes flyttningsväger,' reviewed in NATURE, vol. xv. p. 465, would furnish your correspondent with much of the

information he desires, not to mention the many published observations of our own ornithologists, which are as reliable as they are voluminous." What is mainly wanted is, he thinks, not so much new records as a tabulation and systematic arrangement of those we already have. Mr. Harker refers to a comprehensive article on "The Migration of Birds," by Lieut.-Col. Drummond Hay, in the *Scottish Naturalist* of last year, which meets in some measure Col. Donnelly's suggestions.

GENERAL MYER has presented M. W. de Fonvielle with an improved weather-indicator in acknowledgment of the services rendered by him during the exhibition in popularising the principles adopted by the U.S. Signal Office. This weather indicator will be exposed in the shop of M. Secretan, the optician of Pont Neuf, and forecasts daily published according to the method adopted by the Signal Office in the several American farmers' post offices.

THE services rendered to meteorology by the observatories of Puy de Dôme and Pic du Midi are so great that it is proposed to establish two others, as we mentioned in our last impression, one on the top of Mont Ventoux, in the south of France, and the other on the top of Ballon de Servance (altitude 1,189 metres) in the department of Vosges. A fort and an electric telegraph having been established recently on the top of that mountain, the expenses will be very small. Two other mountains in the vicinity reach a somewhat higher altitude, but they are on the new frontier, and only a part of them belongs to France. M. Jules Ferry, the new Minister of Public Works, having been born in the Vosges, and being a representative of that department, it is pretty certain he will exhibit the same zeal for this enterprise as M. Bardoux, his predecessor, did for the Puy de Dôme Observatory.

TWO companies are in competition for exhibiting the monster Giffard's captive balloon. One directed by M. Tissandier contemplates continuing the ascents on the site of Cour des Tuileries, but there is some uncertainty whether the old palace will not be restored next summer. A German company offers to establish it at Berlin, on the occasion of the forthcoming International Exhibition.

MDME. SARAH BERNHARD, the celebrated actress, has published a volume on her captive balloon ascents.

IN his last report on the trade and commerce of New Caledonia, Mr. Consul Layard states that he has observed there a great variety of apparently valuable fibres, woods, and oils, which have yet to be developed. He also notes the discovery of a large deposit of good guano on some islands in the prolongation of the reef at the northern end of the island. He considers this somewhat remarkable, as it might have been imagined that the rain which falls in these latitudes would deprive the deposits of much of their valuable constituents. The guano, however, is said to have been tested in Melbourne and Sydney, from which places large orders for its supply have been received.

IN his just-published trade-report H.M.'s Consul at Islay, Peru, mentions that some very rich lodes have been discovered at Caylloma, a district about thirty leagues to the north of where the railway passes at Pampa de Arriero, on the road from Arequipa to Puno. Several parties have surveyed them, and they are supposed to be equal in riches to the famous Caracoles mines in Chile, which created so much excitement a few years back. It has been found, however, that there will be great difficulty in extracting the metal from some of the lodes, which are apparently very rich in silver ore, owing to the damp nature of the ground and the consequent necessity for powerful machinery to draw the water from the mines.

AREQUIPA, Peru, was visited on the night of January 9 by the most severe shock of earthquake that has been felt there since that which destroyed the city on August 13, 1868. The present shock occurred shortly before midnight and caused general consternation. The people quickly left their houses, and rushed frantically through the streets, fearing a repetition of the shock. No other, however, followed at that time, but three days later a long and violent single shock of earthquake occurred also at midnight.

WE are glad to notice that the Ancient Monuments Bill has passed through Committee of the House of Commons, with some slight modifications. We trust that the further stages necessary before the Bill can become law will be got through successfully during the present session.

THE German papers, the *Times* Paris correspondent states, give the following account of an occurrence at Rappelsdorf, in the Erfurt district, which, though dated the 21st of last month, is not uninteresting if true:—"Yesterday, at 5.45 P.M., the water of the Todten Lake suddenly rose with a violent bubbling, flooding the surrounding land for nearly 500 metres, and as suddenly returning to its natural bed. One Rappelsdorf inhabitant will have it that he saw a bubbling pillar of water rise fifty feet out of the lake, and that it burst asunder and spread on every side. During the rise of the water subterranean noises were heard. All that is now to be seen is a deep, gaping rent in the earth, from which from time to time vapour rises. The whole land over which the water spread is covered with innumerable small shells and dead fishes."

FURTHER experiments were made last Thursday in lighting the British Museum reading-room with the electric light. The result showed that by proper arrangement and at a comparatively moderate cost, there is good reason to believe that the end desired can be obtained. The Paris Société Générale d'Électricité have made the experiments at their own cost.

WE learn from the *Colonies and India* that experiments are still being carried on at the Peradeniya Botanic Gardens, Ceylon, with the view of discovering a cure for the leaf-disease in the coffee-tree. Mr. George Wall has devised an ingenious method of applying sulphur fumes to the trees for this purpose. A paper umbrella with a curtain hanging from it, is dropped over the tree, and fastened by the handle, a lighted sulphur fuse is then placed underneath, and it is said that the fumes are retained long enough to attack the spores of the fungus.

AN interesting account, *The Colonies and India* states, has been published in the papers of Barker's Cave, Rosella Plains, North Kennedy district, Queensland, to which place Sub-Inspector Armit had followed the track of a white woman and four black fellows. The tracks were followed for nearly three-quarters of a mile in the cave and then out again. The opening is about 25 feet high and 40 broad, and the average height 40 feet, with a breadth of 60 or 70. For about 180 yards from the mouth some degree of daylight is visible, but after that there is impenetrable darkness. The floor was almost perfectly level for hundreds of yards. A root of ficus was found growing 800 yards from the mouth of the cavern. Some hundreds of yards further on they came to water, and found that no further progress could be made without a canoe. An expedition, provided with a canoe and tools and other implements, is to be sent to make a thorough examination of the cave.

THE competition of Japan teas with those of China and Assam have quite recently received a new impetus. In a recent report on the tea trade of Hiôgo we are told that efforts are being made to stimulate this important native industry by the manufacture of black tea; this it is stated is of the greatest importance to Japan in view of the strong competition that exists between teas produced in the country and those known in trade as Formosa Oolongs, the only great market for both of these kinds being

America, the effect of which has been to reduce prices and consequently to impose a limit upon production. Several hundred piculs of imitation Congou were shipped to London from Hiôgo in the course of last year, and are said to have been favourably received in the market, both quality and flavour being of a high order; the only question remaining to be solved as to the success of these teas is whether they "can be produced at prices low enough to enable them to compete favourably in foreign markets with China and Assam teas."

In the December part of the *Transunti* of the Royal Academy *dei Lincei* of Rome, Prof. Cossa gives an interesting account of his researches on the occurrence of the three metals cerium, didymium, and lanthanum. It appears that although these metals occur always in but minute quantities, yet their occurrence is far more frequent than is generally supposed, Prof. Cossa having been able to trace them even in bones and in the ashes of plants, not to speak of a number of minerals, such as certain apatites, Carrara marble, scheelite, &c. In Carrara marble Prof. Cossa found about two centigrammes of the mixed oxalates of cerium, lanthanum, and didymium in every kilogramme of marble; there were also traces of yttrium.

We have on our table the following works:—"The Elements of Dynamics," second edition, James Blackie (Thin, Edinburgh); "Simple Lessons in Domestic Economy," Wm. Wyley Murly; "Education as a Science," A. Bain (Kegan Paul and Co.); "The Land of Midian," 2 vols., Capt. Burton (Kegan Paul and Co.); "Reise aus den Stillen Ozean," Max Buchner (J. N. Kerns); "The Study of Rocks," Text-Books of Science, Frank Rutley (Longmans); "Dictionary of Chemistry," vol. viii. part 1, Henry Watts (Longmans); "Report of the Recorder of the Botanical Locality Record Club," West (Newman and Co.); "British Burma and its People," Capt. Forbes (Murray); "Life in Asiatic Turkey," E. J. Davis (Stanford); "Geological Survey of Victoria, Report of Progress of the Secretary of Mines" (Trübner); "The Two Voyages of the *Pandora* in 1875 and 1876," Sir Allen Young (Stanford); "Practical Geology," W. J. Harrison (W. Stewart and Co.); "Animal Physiology," Dr. A. Wilson (W. and R. Chambers); "Manual of Practical Chemistry," A. W. Blyth (Chas. Griffin and Co.); "A Ministry of Health," B. W. Richardson (Chatto and Windus); "Morphology of Vertebrate Animals," A. Macalister (Longmans and Co.); "The Colour Sense," Grant Allen (Trübner); "Fuel, its Combustion and Economy," T. Symes Prideaux (Lockwood); "The Evolution of Man," 2 vols., Ernst Haeckel (Kegan Paul and Co.); "Experimental Culture of the Opium Poppy," John Scott (Calcutta Press); "Manual of Opium Husbandry," John Scott (Calcutta Press); "Sewage Poisoning," Edward T. Blake (Hardwicke and Bogue).

The additions to the Zoological Society's Gardens during the past week include a Common Hare (*Lepus europæus*), British Isles, presented by Mrs. F. Buckland; an Impeyan Pheasant (*Lophophorus impeyanus*) from the Himalayas, two Cheer Pheasants (*Phasianus wallichii*) from North India, received in exchange; a Nuthatch (*Sitta cæsia*), British Isles, purchased; a Sambur Deer (*Cervus aristotelis*), born in the Gardens.

THE PHYSICAL NATURE OF THE SUN[1]

The question whether all points of the sun are alike, in reference to the emission of light and heat, is not yet decided. As to the distribution of heat on the sun, many investigations have already been made with a view to answering this important question. Nervander seems to have been the first to discover (from temperature observations at Paris and Innsbruck) a temperature inequality of about ½° R, which has moreover the period of the sun's rotation (27·25 days). Simultaneously, Dr. Buys Ballot made a similar inquiry in Utrecht. Proceeding on the supposition that a kind of heat pole exists in the sun, and that accordingly the rotation of the sun must appear from long series of temperature determinations, he got from observations of temperature at Harlem, Zwanenburg, and Danzig, a period of 27·682 days. Since this result differs so much from that of Nervander, Buys Ballot subjected the calculations of Nervander to a thorough scrutiny, from which he concluded that that observer had "not only taken the moon for the sun, but had also mistaken the former." In his memoir Buys Ballot further showed, that to the colder side of the sun, which was presented to us on 1st January, 1846, a temperature corresponded that was, on an average, about 0·7° lower than that of the warmer side, which was presented to us on 15th January of that year. Carlini and D'Arrest got nearly the same result as Nervander. Airy, on the other hand, was unable to decide from the Greenwich observations. Since by the distinguished researches of Hornstein, Director of the observatory at Prague, and of Broun, it has been proved that the time of the sun's rotation may be deduced from variations of magnetic and barometric phenomena more accurately and from a short series (one year) of observations, and since both the period of Nervander and that of Ballot differ so much from Spoerer's and Carrington's rotation period, I submitted the Prague temperature-observations for 1876 to a closer examination, expecting a much shorter period from these than Hornstein got from magnetic and barometric obserations, as I supposed that it would correspond to the rotation, deduced from spot observations, of the thermal equator of the sun, which, it is known, does not coincide with the true equator. I worked the observations therefore by the method given by Hornstein in the *Sitzungsberichte* of the Vienna Academy (Bd. 67), as it is peculiarly suited for such researches. I here communicate the final result. The most probable value obtained for T was 25·56 days. According to Carrington's observations, the position of the thermal equator of the sun on the foregoing supposition would have the latitude 10° to 20°, according to Spoerer's observations, the latitude 13° to 40°. As recent researches seem to show that the influence of variation of the forces of the sun is reflected in the variations of meteorological phenomena, I further investigated the wind components of the year 1870, in their relation to the rotation-time of the sun. For the east-west-components I found a period of 26·71 days. Whether from this result may be concluded a correspondence between prevalent winds in the sun, such as Spoerer has deduced from his spot observations, will have to be decided by closer investigation.

[1] By Dr. Gruss, in the *Astronomische Nachrichten*.

THE STRUCTURE AND ORIGIN OF LIMESTONES[1]

After the obituary notices of eminent Fellows lost during the past year, who were more than usually numerous, the president confined his own special address to the consideration of the structure and origin of limestones, relying mainly on his own observations, but incorporating general facts derived from other sources. Since, in order to properly understand the nature of the various constituent fragments of which many limestones are composed, it is necessary to know the organic and mineral constitution of the various different living calcareous organisms, this question was first considered from a somewhat novel point of view, and they were regarded, not merely as living tissues, but also as mineralised organisms, much attention being paid to their special optical characters. Much attention had been also paid to their true mineral constitution, so as to ascertain in which groups the carbonate of lime exists in the form of calcite, and in which as aragonite. The results are in some cases remarkable, even in relation to biology, and are of great interest and importance in the study of limestone rocks and their included fossils, since subsequent changes depend mainly on whether the original material was calcite or aragonite. This is due to the fact that calcite is in a state of stable equilibrium, and cannot be changed to aragonite, whereas aragonite is relatively in a state of unstable equilibrium, can be changed to calcite, and usually has so changed in limestone rocks. This circumstance has given rise to a complete difference in the state of preservation of many fossils. When they were originally calcite, they may have been further consolidated, but retain their original structure and optical properties, whereas when they

[1] Abstract of Anniversary Address to the Geological Society by Mr. H. C. Sorby, F.R.S., President, communicated by the author.

were aragonite they have sometimes been completely removed by solution, and in other cases are usually changed into a mass of crystals of calcite, and have lost their original microscopical and optical characters. The general structure of various recent and fossil organisms was then considered, and it was shown how and to what extent they could be distinguished, when occurring as minute fragments in thin sections of limestones.

The various facts connected with the disintegration of shells, corals, and other organisms, are of great importance in studying limestones, since without an adequate knowledge of the manner in which they decay and fall to pieces, very inaccurate conclusions might be formed respecting the origin of calcareous deposits. The results mainly depend on original structure, and on whether they are composed of calcite or aragonite. The next questions considered were the manner in which the external form of minute fragments is preserved in limestone, and the various chemical changes occurring after deposition or consolidation; and, having thus established the general principles necessary for their accurate study, the President entered on a description of our various English limestones, in descending order.

The main object was to ascertain, as far as possible, the exact nature of the material from which each particular rock was derived. Some beds are mainly composed of definite fragments, so as to be analogous to sands, and then the true nature of the various organisms from which the fragments are derived can be ascertained, provided they were originally calcite, whereas, if they were originally aragonite, and their structure lost, very often all that can be said is that they were portions of aragonite shells or corals. Many associated beds are or were composed of fine granules, and analogous to clays. In many cases these have in all probability been derived to a great extent from aragonite organisms decayed down into small granules of calcite, and it is quite impossible to further identify the material.

The structure and origin of oolitic grains was dwelt upon at some length. Usually they are evidence of true chemical deposition. They occur in three distinct types, viz., those composed of aragonite, having a concentric structure without any radii, giving rise with polarised light to a black cross optically positive; those which are composed of calcite, having a radiate structure and giving rise to a negative black cross; and those which have recrystallised since their original formation. After describing the chief points of interest connected with the leading limestone rocks of our country, the president collected together the results into two tables, the more condensed of which may here be given.

Name of rocks.	Chief constituent fragments, &c., in descending order.
Cretaceous	Shell prisms, Foraminifera, Coccoliths.
Wealden	Freshwater aragonite mollusca, Entomostraca.
Jurassic	Chemical deposits, Aragonite mollusca and corals, Brachiopoda, Echinoderms, Shell prisms.
Permian	Original structure lost by dolomitisation.
Carboniferous ...	Encrinites, Brachiopoda, Foraminifera, Corals, and Polyozoa.
Devonian	Encrinites, Corals, and allied organisms.
Silurian	Encrinites, Corals and Polyzoa, Brachiopoda, Trilobites.
Metamorphic ...	Original structure lost, Quartz and Silicates formed *in situ*.

He concluded as follows:—

"On examining these tables, especially the more detailed ones, it will be seen how remarkably and characteristically our limestones differ from one another. There would usually be little difficulty in deciding the general age of any characteristic, somewhat coarse-grained, specimen. Though this difference must to a great extent have depended on the nature of the organisms living at each period, yet it must also have depended on the accompanying mechanical and chemical conditions of the water in which the deposits were formed. The structure of each rock was therefore dependent on two most important circumstances, and we need not be surprised to find the results so varied and characteristic. Passing upwards from the earlier rocks, we may often trace a gradual change, broken here and there by a complete contrast, which is in perfect agreement with results arrived at from a totally different class of facts. On the whole, this is perhaps the most important conclusion that we can at present draw from the subject before us. Possibly further research may teach us much more, since I am quite sure that much remains to be learned. In fact, long as I have studied these questions, and long as this address has been, I know quite enough of the facts to be convinced that it is only a sort of first attempt and rough sketch of a very wide and complex subject."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

MR. MILMAN, who for some years has acted as Assistant-Registrar, has been appointed to succeed Dr. Carpenter as Registrar of London University. It is stated that Mr. H. N. Moseley is a candidate for the Assistant-Registrarship.

MR. A. CRAIG-CHRISTIE, F.L.S., lecturer on botany, Edinburgh, is a candidate for the Chair of Botany in the University of Edinburgh.

IN a recent report by the British Consul at Hakodate, some account is given of the public buildings and other institutions of Sappora and Ishcari. Referring to the Agricultural College buildings, we are told that they consist of four distinct houses, as follows:—A two-storeyed house, comprising lecture and recitation-rooms, cabinets, and offices. A one-storeyed house, used for dormitories to accommodate from fifty to sixty students, attached to which is a similar building providing a large dining-hall, kitchen, bath-rooms, offices, and servants' quarters. In connection with this, again, is a two-storeyed building, which serves as a lecture-room and a general sitting-room and study. A two-storeyed house, which is the chemical laboratory; the ground-floor of this house is used as a general laboratory for the students, and on the second floor are the lecture and apparatus-rooms, and the rooms for collections in mineralogy, geology, and chemistry. Besides these there are several other buildings in European style, used for various scientific and industrial purposes. It is further intended to erect, at an early date, an Agricultural College, likewise two-storeyed, which will be another imposing building. Here will be zoological, mineralogical, geological, botanical, and agricultural museums, with separate halls for lectures and experiments in the above-mentioned branches. The Sapporo Agricultural College was founded by the Kaitakushi for the education and practical training of young men from all parts of the Empire, who are expected to remain in the Government service in Yesso, after graduation for a term of five years. The number of students is limited to sixty, and all their expenses while in college are defrayed by the government. Candidates for admission must be at least sixteen years of age, of sound constitution and good character. They will be examined orally and in writing in the Japanese and English languages (which they are expected to read, write, and speak correctly and fluently), arithmetic, geography, and universal history. If they succeed in this preliminary examination they will have to sign a prescribed contract with the government and furnish a satisfactory surety or guarantee. The course of instruction will occupy four years and embrace all the branches of a general education, with the study of the Japanese and English languages. Moreover, they will be thoroughly instructed in agriculture and horticulture, civil engineering, and chemistry, astronomy, botany, geology, zoology, military science and tactics, and before they leave college, in the fourth year, they will have to devote some time to political economy. As the students are destined to become practical agriculturists, including the use of hand implements and machinery, and the care and management of domestic animals, they have to work in the fields with their foreign instructor two afternoons of each week. There are at present three foreign professors or instructors, viz., one for mathematics and engineering, one for botany and chemistry, and one for agriculture, besides the native teachers, and it is expected that later will be added an instructor for military drill, and one specially for the English language, and a foreign doctor. The number of students at the time the report was written amounted to thirty, fifteen being added annually up to sixty in the fourth year of the foundation of the college, when the first batch of fifteen (the original number started with) will retire and graduate if they have completed their course of studies in a satisfactory manner, whereupon they will enter government employ. In another part of the report, speaking of the progress made by the students, the reporter says, "they are most assiduous at their studies, and it is indeed astonishing the progress they have already made. All their studies are conducted in English, and they speak and discuss in English without the slightest hesitation, making use of very good language. They also appear to enter fully into the different branches of study."

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 1, 1879.—This begins with a portion of an extended inquiry by Herr F. Kohlrausch into the electric conductivity of aqueous solutions of hydrates and salts of the light metals, as also of sulphate of copper, sulphate of zinc, and nitrate of silver. The paper is in three parts—an experimental, a practical, and a theoretical (the first two in this number). The practical part gives tables for use, and formulæ of conductivity, especially of dilute solutions; specifies bodies which show a maximum of conducting power at a degree of concentration of solution below saturation, indicates liquids which commend themselves as a standard for electric conductivity, &c.—Herr W. Kohlrausch furnishes an experimental determination of the velocities of light in crystals. He employed the new instrument called a total reflectometer, and he comes to the conclusion that Fresnel's theory of double refraction in optically uni- and biaxial crystals gives a form of light wave-surfaces, which, within very small errors of measurement, is in general experimentally confirmed for uniaxial crystals, and for the principal sections of biaxial crystals.—Herr Groshaus contributes some interesting observations on the densities of substances in the gaseous and liquid states, in relation to their chemical composition.—Herr Ritter calculates that the quantity of heat radiated annually from the sun 75,000 years ago must have been about 1 per cent. less than at present (700,000 years ago about 10 per cent. less), a result which is supposed to explain the "glacial period," while the previous tropical climate is accounted for by a less thickness of the solid crust of the earth. He also estimates that each kilogramme of the sun's mass contains on an average about 43,000,000 units of heat.—Herr Wiedemann declines to regard the oxide containing copper separated electrolytically from solutions of acetate of cupric oxide, as a peculiar allotropic modification of copper.—There are also papers on the thermo-electric properties of apatite, brucite, &c. (Hankel), the theory and application of electro-magnetic rotation (Margules), the influence of temperature on galvanic conductivity of liquids (Exner and Goldschmiedt), and two new fluorescent substances (Lommel).

SOCIETIES AND ACADEMIES

London

Royal Society, February 27.—"Studies in Acoustics. I. On the Synthetic Examination of Vowel Sounds." By William Henry Preece and Augustus Stroh.

The authors had studied the formation of vowel-sounds on Helmholtz's theory, and had succeeded in constructing an instrument which reproduced the principal vowels with greater distinctness and accuracy than the phonograph, and which fully confirmed that theory. In the pursuit of this study they constructed a new phonautograph, a machine for drawing harmonic curves either on paper or on smoked glass, compounded of one or many partials varying in phase and amplitude, called a "synthetic curve machine," a new syren, a new musical instrument dependent on the vibration of a diaphragm and several instruments for reproducing vowel-sounds. They had also studied the intensity of sound, and showed experimentally how loudness was dependent on the quantity of air thrown into vibration, and independent of the amplitude of vibration of the sonorous body which remained constant. They also introduced a new stereoscopic slide, which rendered very complicated curves perfectly perspective.

Linnean Society, February 20.—Prof. Allman, F.R.S., president, in the chair.—The Rev. G. Henslow exhibited portions of an elm bough having a pulley centrally imbedded; exteriorly all trace of its presence was obliterated, and the wood-growth indicated thirteen years subsequent to the entrance of the foreign body.—Mr. R. Bowdler Sharpe showed and commented on a series of rare birds. Among those from New Guinea were skins of *Paradisea raggiana*, collected by the Rev. Mr. Lawes; of others obtained in the Fijis by Baron von Hugel were species of the genus *Pinarolestes*, which also inhabit Tutuela, one of the Samoan Islands.—Mr. W. H. Fitch passed round a coloured drawing of a remarkable large crimson-coloured pitcher (twelve inches long by nine in circumference) of *Nepenthes sanguinea*, grown at Bury, by Mr. O. Wrigley.—A paper was read by Dr. H. Trimen, on the genus *Oudneya*, Brown. This is a small cruciferous shrub discovered by Dr. Oudney in the desert between Tripoli and Mourzuk. The genus has been obscure in consequence of Brown's short insufficient diagnosis. This latter Cosson has shown to be a *Moricandia*, hence Brown's genus has been doubtfully referred to the same. Dr. Trimen shows from an examination of Oudney's herbarium in the British Museum, that *Oudneya* is identical with *Henophyton*, Coss., which name will supersede, having the priority of thirty-one years.—The abstract of a communication on some South American genera of plants of uncertain position, by Mr. John Miers, was read by the Secretary. The author refers the *Pleoaginea* of Arruda da Camara, who mentions three species belonging to the Chryobalanaceæ, but of which two only should be retained in that genus, the third belonging to the true *Conepia*, Aublet. Among *Parinarium* the two species described and figured by Aublet alone ought to be retained. Those to be excluded are the two British Guiana species of Bentham, and seven others of Brazilian origin described by Dr. Hooker, and which from their floral structure and development of fruit do not differ from *Licania*. The Malayan species of Blume are now shown to belong to the *Petrocarya*, Jack., while the African species enumerated by De Candolle, together with five others yet undescribed, must be referred to *Griffonia*, Benth. and Hook., a genus notable for the conferrumination of the cotyledon of the embryo. The genus *Minquartia*, Aubl., belongs to the Crescentiaceæ, as does *Senapea*. Bentham's and De Candolle's *Kigelia* are widely different, the *K. africana*, Benth., properly belonging to *Tripinnaria*. The genus *Managa*, Aubl., Mr. Miers avers belongs to the Aurantiaceæ; *Racceria*, Aubl., does not come under Sapindaceæ, as De Candolle supposed, but to the Meliaceæ, and is allied to *Melia* and *Azenderachta*, Juss.—Dr. Maxwell Masters next gave the chief points of a paper on the inflorescence of Crassulaceæ. Though devoted chiefly to this group he discussed the schemes of classification proposed by Roeper, Bravais, and others, as also the emendations of Hofmeister, Sachs, and the modern German school of botanists. He proposed a re-arrangement under the heads of Monopodial, or indefinite; Choripodial, or dichotomous; and Pleiopodal, or definite; the latter comprising the Sympodial varieties. The modifications brought about by suppressions, adhesions, congenital or otherwise, real or apparent, and by displacement of varying kind and degree were alluded to, the general conclusion being that while suppressions and adhesions do occasionally occur, yet that in most instances the phenomena witnessed might easily be explained by displacement of parts, and especially by that process of elongation known as up-lifting. The history of development, as well as the internal structure, he believes are consistent with this latter view, but not, as a rule, with the theory of adhesion.—Messrs. Ed. A. Fitch, Laurence Scott, and Wm. Stone were elected Fellows of the Society.

Chemical Society, February 20.—Dr. Gladstone, president, in the chair.—The following papers were read:—On colouring-matters derived from diazo compounds, by Dr. O. N. Witt. For some time after the introduction of anilin dyes, though magentas, violets, and blues were obtained in profusion, no choice of yellow or green anilin dyes was to be had. A few years back, however, a beautiful yellow dye, chrysoïdin, was described by Hofmann, and since that time numerous patents have been taken out for the manufacture of similar substances. In the present paper the author gives an account of these various substances, which are oxy or amido derivatives of azobenzene, including the different tropæolins.—Investigations into the action of substances in the nascent and occluded conditions; hydrogen, continued by Dr. Gladstone and Mr. Tribe. The authors have investigated the actions of nascent hydrogen obtained by electrolysis and hydrogen occluded in palladium or platinum on nitric and sulphuric acids; they establish a close similarity of character, and therefore of condition between the so-called nascent hydrogen and the hydrogen occluded by metals.—On some methods of vapour density determinations, by Mr. J. T. Brown. The author criticises the methods and formulæ of previous experimenters, and suggests the determination of the vapour tensions of mercury by estimating the vapour tension of a substance over Wood's metal and over mercury at different temperatures.—On the decomposition products of quinine and the allied alkaloids, by Mr. J. J. Dobbie and Dr. W. Ramsay. The authors have oxidised the four principal alkaloids derived from cinchona bark, and find that they all yield, on oxidation, the same acid, tricarbopyridenic acid. They also point out that there is a close relation between the cinchona bark alkaloids and the bases of the pyridin series.

Geological Society, February 5.—Henry Clifton Sorby, F.R.S., president, in the chair.—Arthur Ernest Baldwin, James

Farie, Benjamin Neeve Peach, were elected Fellows of the Society.—The President announced the receipt of a legacy of 1,000*l*. bequeathed to the Society by the late Sydney Ellis, Esq., of The Park, Nottingham.—The following communications were read:—On the occurrence of pebbles with Upper-Ludlow fossils in the lower carboniferous conglomerates of North Wales, by Aubrey Strahan, F.G.S., and Alfred O. Walker, F.L.S. The authors described the mode of occurrence near Abergele of certain lower carboniferous conglomerates, best exposed in Ffernant Dingle, and especially of one containing numerous red and green sandstone pebbles, which inclose fossils of Upper-Ludlow forms, and lying above the so-called "bastard limestone." From the arrangement of the beds the authors believe that they may have been deposited against a bank or sloping surface of Wenlock shale; and they state that the great majority of the pebbles in the conglomerate are quite unlike any rock known in the district, but closely resemble the Upper-Ludlow beds of Kendal and Central Wales. The authors discuss the origin of the pebbles, and suggest "the probable extension of the Ludlow beds under Lancashire as the most likely source from which they can have been derived."—On a new group of pre-Cambrian rocks (the Arvonian) in Pembrokeshire, by Henry Hicks, F.G.S.; with an appendix on their microscopic structure by T. Davies, F.G.S. In some new areas of pre-Cambrian rocks, discovered by the author last summer in Pembrokeshire, some rocks of a character hitherto unrecognised in this country were made out. As they were found to hold there, and subsequently also in other areas, a very definite stratigraphical position, with a vertical thickness of several thousand feet, they have been separated by the author from the other pre-Cambrian groups under the distinctive name of Arvonian. They were also found to occupy an intermediate position between the Dimetian and Pebidian formations, and at all points, so far as could be made out, appeared to be separated from each of those formations by stratigraphical breaks. The new areas where they are chiefly exposed are situated some few miles to the north of Haverfordwest, where they form ridges running in a direction from north-east to south-west. They occupy an average width of about a mile, attain at some points to a height of nearly 600 feet, and together have a length of over nine miles. The rocks are flanked by Pebidian and Cambrian beds along their north-west borders, and on the south-east Silurian rocks have been brought against them by faults. In general appearance, as well as in their more minute lithological characters, they are easily distinguished from any of the rocks hitherto described by the author as characteristic of the Dimetian and Pebidian groups in Pembrokeshire. They are, however, so closely allied to some of the true "hälleflinta" rocks of Sweden, that it seems to the author and Mr. Davies that this is the name that should be applied to them in a petrological sense. The author and Mr. Davies believe the origin of the rock to have been a sedimentary one.—On the pre-Cambrian (Dimetian, Arvonian, and Pebidian) rocks of Caernarvonshire and Anglesey, by Henry Hicks, F.G.S.; with an appendix on their microscopic structure by the Rev. Prof. T. G. Bonney, F.R.S. In this paper the author gave the results of some further researches made in Caernarvonshire and Anglesey since his previous communication to the Society on December 5, 1877. A brief statement of some of the results was read at the last meeting of the British Association in Dublin; but much additional evidence was now brought forward, besides many important facts obtained since by microscopical examination of the rocks.—On the quartz-felsite and associated rocks at the base of the Cambrian series in north-western Caernarvonshire, by the Rev. Prof. T. G. Bonney, F.R.S. The great masses of quartz-felsite (or quartz-porphyry) which occur in the vicinity of Bangor, Caernarvon, and Llyn Padarn, are coloured in the Survey map as intrusive, and in the memoir regarded as most probably the result of an extreme metamorphosis of the lower beds of the Cambrian series. The author showed that these quartz-felsites exhibited, in places, all the characteristics of true igneous rocks.—On the metamorphic series between Twt Hill, Caernarvon, and Port Dinorwic, by the Rev. Prof. T. G. Bonney, F.R.S., and F. T. S. Houghton, B.A. In the Geological Survey map this district is coloured as "intrusive felsite," together with those spoken of in the last paper. It was asserted to be probably metamorphic rock by Prof. Hughes and Dr. Hicks in a communication made to the Society last year, and the first author confirmed that view by microscopic examination of a specimen collected by them. The authors had during the past autumn more minutely examined the district, and found: 1. That the general character of the series was that of a metamorphic one; 2. That the rocks of granitoid aspect were associated with well-marked beds of conglomerate; 3. That this series extended up to a little beyond Port Dinorwic, where the quartz-felsite set in. The paper described the microscopic structure of some of the rocks, and the author expressed the opinion that the more granitoid specimens were probably the results of alterations of felspathic grits.

Physical Society, February 22.—Prof. W. G. Adams, in the chair.—New Members: Rev. Coutts Trotter, Prof. G. D. Living, J. C. Adams, F. W. Paterson.—Dr. C. W. Siemens described his new electric current regulator. A necessary condition of the transmission of power to a distance by electricity along a single conductor and re-distributing it by means of branch circuits to separate electric lamps or motors, is that the current strength in each lamp shall be practically uniform. Otherwise the current flowing in the whole branch varies. Hence the necessity of a regulator to regulate the flow of current so as to keep it uniform, however the resistance of the circuit or the electromotive force of the source may vary. The author believes that by properly arranging a number of dynamo-electric machines, either in series or parallel (for intensity or quantity), at each end of the wire, a vast amount of power may be sent along a small copper conductor successfully, provided the distribution is properly regulated. He has designed a regulator based on the heating of a wire by the passage of a current through it. A fine strip of mild steel $\frac{1}{20}$mm. thick is stretched horizontally between two terminals. An upright spindle is supported by means of an insulating foot, upon the middle of this strip, in such a manner that, as the strip bends or sags by its expansion, the spindle sinks with it. Now this spindle carries at its top a table or plate of metal (or, as the case may be, a set of radial springs), and as the spindle rises or sinks to different heights, this plate or these springs make contact with other springs set radially round; and these contacts take out from or throw in resistance coils into the circuit of the current. The sensitive strip is so thin that it may be regarded as a radiating *surface* merely, and it may be assumed that its temperature, due to heating by the current, balances itself with the radiation instantaneously. After passing through the steel strip, the current flows through the coils thrown into circuit, and, by the arrangement we have described, if the current increase so as to over-heat the strip, the latter sags a little more, the spindle sinks, and the consequence is that one or more of the spring contacts is broken, and one or more coils inserted in circuit. A rise of 1° F. in the temperature of the strip is sufficient to liberate two or three of these coils. The fact that the temperature of the strip varies as the square of the current, favours the sensibility of the apparatus. An older form of this apparatus, having pendulous contacts, was also shown; also a regulator in which the expansion by heating of a sensitive wire caused the resistance of several carbon buttons in contact to vary through the pressure exerted on them by means of a bell-crank lever. Dr. Siemens had not been able to prepare carbons which gave the wide variations of resistance obtained by Mr. Edison. Siemens' regulator can also be used as a current meter by causing the sensitive strip to actuate a lever carrying at its end a pencil writing on a moving paper. Dr. Coffin said that he had thought of a regulator in which the heating of a wire spiral in a gaseous chamber would cause the gas to expand and drive up a mercury column past a series of contacts which would throw resistances in circuit. Dr. Guthrie suspected, from some experiments of his, that the conductivity of conductors was not strictly proportional to their sectional area.—Dr. Schuster then gave the results of some observations of his on the spectrum of lightning. These were made by a spectroscope with two prisms, one for the red and the other for the blue end of the spectrum, which were shifted into the line of sight by a chamber arrangement. Three observations were made: one at Las Animas, one at Manitou, and one at Salt Lake City last year. These showed the three nitrogen lines with three well-defined bands and one doubtful band. The nitrogen lines correspond to the spectrum of air, and the bands appear to Dr. Schuster to agree with the spectrum of the light round the negative pole of the spark in a tube containing oxygen with adulteration of carbonic oxide.—Prof. Ayrton then exhibited an exisothermal model of a cooling globe, the globe in question being a trachyte earth 8,000 miles in diameter. The model gives graphically the temperature of every single part of the earth from the moment when it was at the temperature of

molten trachyte down to 800,000,000,000 years afterwards, that is, long after the present era.

Royal Microscopical Society, February 12.—Annual meeting.—H. J. Slack, president, in the chair.—The report of the council was presented and read by the Secretary.—The president read his annual address, in which the oil immersion objectives and the recent discussion on fermentation were referred to.—Dr. L. S. Beale, F.R.S., was elected President.—Herr Petzold's slides of insects, kindly lent by the editor of NATURE, were exhibited, also catoptric immersion illuminator, by Mr. Stephenson, and microscopes by Mr. Crisp.

BOSTON, U.S.A.

American Academy of Arts and Sciences, February 12.—Hon. Charles Francis Adams in the chair.—Prof. Pickering announced the completion of the observations of the zone assigned to the Harvard College Observatory in the revision of the *Durchmusterung* of Argelander. The observer, Prof. W. A. Rogers, has devoted in this work a large portion of his time for the past eight years. The stars are all included between 49° 50′ and 55° 10′ N., and number over 8,000. The accuracy required has made the work very laborious, the total number of observing hours being greater than was required in the Catalogue of Argelander, containing the approximate places of over 300,000 stars. The deductions are still to be made, and will require some years.—Prof. N. S. Shaler read a paper on the explosion of coal-dust in mines, and suggested that a solution of calcium chloride, which is obtained at no other expense than the cost of preparation from the water of salt works, be driven in the form of spray against the walls of the mine in order to fix the dust in a deliquescent substance upon the walls.—Prof. Asa Gray read a paper on the characters of some new genera and species of plants, chiefly of California and Oregon.—Mr. Thomas P. James made some remarks upon American bryology and some new species of mosses described by himself and Leo Lesquereux.—Mr. N. D. C. Hodges presented a paper on a new absolute galvanometer. The current is measured by its effect in changing the time of vibration of a magnet with its axis parallel to that of the coil. Mr. Hodges also presented a method of determining the reduction-factors of a tangent galvanometer for all deflections when the value for any one is known. By measuring a current by the deflection and then with the same coil and magnet, by the change in time of vibration, the ratio between the reduction-factor of the instrument when the magnet makes any angle with the plane of the coil to its value when the magnet is perpendicular to the coil may be found.

PARIS

Academy of Sciences, February 24.—M. Daubrée in the chair.—The following papers were read:—On the induced currents resulting from movement of a bobbin across an electromagnetic system, by M. Du Moncel. *Inter alia*, the direction of induced currents caused by a given movement of a bobbin before a magnetic pole may be diametrically opposite, according as the movement is tangential or normal to the pole, and the magnetic core on which the bobbin glides is in contact with the inducing pole, or distant from it.—Observations on M. Planté's recent work, "Researches on Electricity," by M. Becquerel. These researches relate to secondary currents.—On the hemihedric forms of alums, by M. Lecoq de Boisbaudran.—Resistance to change of state of crystalline faces in presence of their mother-water, by the same. The passage from a state of very slow dissolution to one of very slow growth does not take place suddenly; each face rests intact, while the mother-water varies within certain limits. There is no mobile equilibrium or exchange of molecules between a crystalline face and its mother-water, but merely erosion or continuous deposit, and, between the limits of resistance to change of state, neither erosion nor deposit. The resistance to change of state is modified independently for each system of faces.—Experiments on a modification which has been made in the sluice of Aubois, permitting the suppression of the alternate motion of vessels in the lock, by M. de Caligny.—Reflections on M. de Lesseps' communication regarding contagion of the plague, by M. Bouillaud. Considering the plague contagious, he says the important problem now is to ascertain the nature of the *principle* or *agent* by which it is communicated.—M. de Lesseps presented the reports he had drawn up for the Alexandrian authorities during the plague in Egypt in 1834–35.—M. Stephan was elected Correspondent in the Section of Astronomy, in place of the late M. Hansen, of Gotha.—On latex during the germinative evolution of *Tragopogon porrifolius*, &c. (continued), by M. Faivre.—Observations of eclipses of Jupiter's satellites, made at the observatory of Toulouse in 1878, by M. Baillaud.—Direct photography of solar protuberances without use of the spectroscope, by M. Zenger. He puts on the sensitive plate, before a very short exposure, a solution of pyrogallic acid and citrate of silver, and uses a layer which absorbs all the rays composing the light of the corona and the solar protuberances.—Geometrical laws of deformations produced by a force applied at a point of an indefinite solid, and calculation of the error fallen into, when, according to the principles of classical mechanics, one conceives this point of application displaced by a certain quantity in direction of the force, by M. Boussinesq.—Projection of molecular shadows, by Mr. Crookes.—Action of different coloured lights on a layer of bromide of silver impregnated with different organic colouring matters, by M. Cros.—On plates sensitised with tincture of mallow or black-currant, the direct spectrum of the Drummond lamp is inactive in all the middle green, while the red and violet extremities are very active. With carthamine, again, the middle part is most active. With chlorophyll the action continues throughout the visible spectrum and a little beyond it, presenting several minima and maxima. M. Becquerel pointed out that these researches had been to some extent anticipated.—On the production of crystallised chromate of baryta, by M. Bourgeois. This gives some new results by the method of calcining an alkaline chromate with the chloride of the metal which is desired to enter into the saline combination.—On the composition of beer yeast, by MM. Schutzenberger and Destrem. Yeast contains complex compounds both hydrocarbonised and proteic, formed like glucosides, and easily decomposed by acids and alkalies.—On pyrogenic carburets of American petroleum, by M. Prunier. He has obtained carburets with considerably more carbon than any compounds hitherto known (97 per cent., the highest previously 95 per cent.).—On glycide, by M. Hanriot.—On the generation of aniline-black by chromates in presence of chlorates, by M. Grawitz. He disproves M. Witz's recent assertion against this.—On varions selenides of lead and copper from the Cordilleras of the Andes, by M. Pisani.—On the presence of a segmentary organ in endoproct Bryozoa, by M. Joliet.—On the segmentary organs and the genital glands of sedentary polychætous Annelides, by M. Cosmorie.—On the scales of osseous fish, by M. Carlet. He describes the effects of colouring with picro-carminate of ammonia, and of subjecting to polarised light.—On the mode of employment of telephones at the Artillery School of Clermont, by M. de Champvallier. The success realised is attributed to the method of regulating the position of the magnet. The milled button for turning the screw has at its base a pointer (at right angles to the axis) which moves over a circle of copper. The positions of this pointer for distinct transmission one way or the other are noted.

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THURSDAY, MARCH 13, 1879

THE UNITED STATES FISHERIES

United States Commission of Fish and Fisheries—Report of the Commissioner for 1875–76. (Washington: Government Printing Office, 1878.)

THE series of volumes now being issued by Mr. Spencer Baird, Fishery Commissioner of the United States, of which the fourth has just reached us, is in every way remarkable. As a much required contribution to our scanty knowledge of fishery economy and the natural history of American food fishes, it presents a mass of very valuable information; the details incidental to the propagation of carp and salmon are particularly interesting, not only because they show the gigantic scale on which these things are done on the other side of the Atlantic, but also because of what they teach as to the mode of doing them. The historic and economic sketches of the fish and fisheries of various other nations are likewise full of interest, Mr. Baird having allowed nothing to escape which he thinks will illustrate his subject or render testimony to the value of the work in which he is engaged.

The contents of the present volume, if not so varied as those of some of its predecessors—over 750 of its pages being devoted to a historical account of the American whale fishery—are certainly not less interesting. There is a report on the fisheries of Chicago and vicinity, which, we are told, yield annually about 12,000,000 lbs. weight of fish; there is also an article on the condition of the salmon fisheries of the Columbia River, from which we have recently been receiving enormous consignments of canned salmon; the present volume likewise contains notes on the fishes of the Delaware, together with an abundant supply of information regarding the propagation of various of the minor food fishes of America.

We are not in the least surprised to learn that a diminution of the supplies of Columbia River salmon (*Salmo quinnat*) has taken place. These fish, consequent on being free to all who choose to capture them, have given constant employment to about fifteen "canneries" during ten or twelve years, each of which, if we strike an average, will turn out a million pounds weight of fish per annum. There are various kinds of salmon in the Columbia River, but there seems to be some confusion regarding their proper identification; that we have mentioned is the one which is captured for the canneries. It will probably be found on further investigation that one or two of the so-called varieties are simply the same fish in different stages, as in the case of *Salmo salar* and the "grilse;" fish of all sizes being, we are told, found together in the Columbia and its tributaries. It is rather singular that no very large salmon are found in that great stream. Mr. Livingstone Stone who reports to the Commission on the subject, says that the largest salmon he ever saw had a girth of 31 inches, and was 35 inches in length and weighed 65½ lbs. One which was *said* to have weighed 83 lbs. was reported to Mr. Stone by a fisherman who saw it. The average weight of the Columbia salmon (*Salmo quinnat*) is from 22 to 23lbs. whole, but when dressed for market only about 17lbs. The ratio of salmon growth has never been accurately determined. Some large salmon (*Salmo salar*) are captured in the salmon streams of Scotland, but the age of such fish cannot be set down with any certainty. In a report made by Mr. Stone given in a previous volume of Mr. Spencer Baird's reports (1872-73) regarding the Sacramento salmon, it is mentioned as a "theory" that salmon are full-grown at the age of about three years. We commend this question of salmon growth to the farther notice of Mr. Commissioner Baird. In the United Kingdom the evidence obtained on this point has hitherto been of the most contradictory nature. Our own opinion is that salmon are never "full grown," or rather that as long as they are alive they continue, under proper conditions of food and living room, to increase in weight and size. After much weighing of evidence we recently came to the conclusion that a fish which we handled in August last (1878), and which weighed 54 lbs., was at least nine or perhaps ten years old. From some inquiries which we are making, we hope to be provided with sufficient data for a settlement of the question during next fishing season. Returning for a moment to *Salmo quinnat*, the following notice of the periods and strength of its migration may be useful for comparison with the habits of *Salmo salar*:—"The salmon make their first appearance in February, though in very small numbers; the main body arrives in May, June, and especially in July, when the run is enormous. The May salmon are largest. Perhaps the most correct view to take of the running of the salmon is to consider all the salmon as included in one run, beginning in February, increasing in May and June, and culminating in July, though they might also be legitimately divided into three runs, the first or meagre run coming in February, March, and April, the second or full run in May and June, and the third or maximum run in July. After July they diminish very rapidly, and soon almost entirely disappear from the river."

It is at present being considered, we believe, whether or not it will be necessary to resort to "pisciculture" on the Columbia river. Would it not be better to limit for a time the spoliation of the water? At present no check whatever is placed on the fishery, and each "cannery" captures and tins all the salmon that it can find, and with each establishment working up a million pounds' weight per annum, the complete exhaustion of the supply is only a work of time, as has been demonstrated in our own rivers.

The artificial hatching of salmon is conducted in the United States on quite a gigantic scale. A table is given in the report of "operations on the McCloud river in 1876," from which we learn that on one day, Sept. 4, over a million salmon eggs were obtained by a process of artificial spawning. During the years 1874-75 and 1876, the number of ova taken for despatch to different rivers was 21,877,300. In 1875, 8,629,300 eggs were secured, which, at the British rate of a thousand eggs for each pound of salmon weight, represents the handling of a large number of fish. The cost of collecting the spawn is about four shillings per thousand. The eggs taken in the McCloud river are widely distributed, some being sent to Canada and a portion to New Zealand.

Much valuable information is given in the present series of reports on the best modes of salmon hatching, and the

careful observations of temperature made during the manipulation are tabulated for future use. With reference to *Salmo quinnat* of the Sacramento, it may be confidently affirmed that there is almost no difference between it and *Salmo salar*, which, in form and habits, it closely resembles; it has, however, if we mistake not, more rays in the anal fin, and is of course able to endure a higher temperature: the eggs mature in summer and hatch in the autumn. In ascending to their spawning grounds the *gravid* fish must frequently pass through river water having a temperature of over 76°. We were not prepared for the following remarkable statement from New Zealand, made in the present volume (p. 989):—"*So far as yet observed, the adult fish all die after spawning, and never return to the sea.*" This fact is stated in a "memorandum respecting the American salmon and white fish recently introduced in New Zealand by Dr. James Hector, of the Colonial Museum, Wellington." We shall be glad to have some authoritative statement with regard to the above fact, as without some explanation it seems too extraordinary for belief.

An interesting account of the distribution and habits of the "Shoodic salmon" is given in the present volume. These fish, *Salmo sebago*, are known as "land-locked salmon," at one time probably in communication with the sea, but now shut out from it altogether, and thus forced to pass their lives in fresh water for ever. There is, however, no evidence that "the supposed change of habits—the abandonment of the seaward migrations—came about in such a way as the term, *land-locked*, implies." Mr. Atkins, in his report to the commissioner, makes a statement which we should like to have explained. In collecting the spawn of the "Shoodic salmon," there was taken, he says, a single female specimen of *Salmo salar*; "she yielded 10,000 eggs, which were impregnated with the melt of the 'Shoodic salmon;' they developed well, and hatched into vigorous fish." But how did a female specimen of *Salmo salar* come to be among the "land-locked" fish? and what became of her?

The operations of the United States Fish Commission commenced eight years ago, and are still being prosecuted. The work has been varied, as we have indicated, and, so far, it has been well done, and the information accumulated will form a quarry which will yield a lasting supply of fishery knowledge to all inquirers.

THE PACIFIC ISLANDS

Reise durch den Stillen Ocean. Von Max Buchner. (Breslau: J. U. Kern, 1878.)

THE author of this pleasantly written and very interesting book of travel became, as he tells us, a ship's medical officer, in order to gratify his desire to see the world. He sailed in an emigrant ship from Hamburg to New Zealand and returned home by Fiji, the Sandwich Islands, San Francisco, and the Pacific railroads. He made a considerable stay at New Zealand, Fiji, and the Sandwich Islands, and the chief interest of his book lies in the accounts of what he saw at these places. Though there is little new information in the work the descriptions are extremely good.

On board the emigrant ship there devolved on him not only the usual medical work but also the entire charge and government of the 397 emigrants and the division of rations amongst them. He draws a very unpleasant picture of the dangers and sufferings incurred by passengers in such a vessel. The captain seems to have known little of his business. After a narrow escape of collision the ship appears to have incurred a still narrower risk of running on the Goodwin Sands, and on the open ocean there seems to have been constant doubt as to longitude. A terrible picture is drawn of the sufferings of the emigrants—a mixture of Poles, Scandinavians, Germans, and Dutch—in a storm. The captain, who in such voyages receives a small percentage on the profits of the voyage from the owners of the ship, made constant attempts to cut short the allowance of food to the passengers. The author, who acted in the interests of the New Zealand Government and their emigrants, had great difficulty in making the captain, who cursed and swore and hammered the table with his fist when appealed to, act up to the details of the contract in the matter of rations. Even then the food seems to have been insufficient, and the salt beef was constantly being stolen, the beef tub being forcibly broken open for the purpose. The barbarous old custom of shaving on crossing the line was carried out, but we are glad to find that only volunteers amongst the emigrants were operated on. An outbreak of typhoid fever occurred on board before New Zealand was reached and caused nine deaths.

The author gives a most interesting and lively account of the present condition of the Maoris. At Lake Taupo there is now a very good clean hotel kept by a German and an Italian; and a Maori, who goes by the name of Mr. Jack, has established himself as guide of Taupo. He has constructed a bathing-place at the hot springs with a room over the bath and dressing-room, and charges a shilling for each bath. Close by, but hidden in the vegetation, our author discovered an equally good or better natural hot bathing-place, but the crafty Maori had filled it with dirt for fear it should compete with his own. At Ohinemotu on the south shore of Lake Rotorua, where are the principal hot springs, there are two good hotels for tourists and a population of about 300 Maoris. Every evening the greater part of the population turn out and bathe together in a small bay of the lake which is kept constantly warm by the hot springs, and whites and browns of both sexes swim about and sit in the warm mud together, conversing for hours at a time. Close by the bathing-place are a group of huts, the owners of which are dead, and which are tabu, and are described as full of ancient native implements, spears, adzes, wood-carvings, and other desiderata for ethnological museums, but which no one dares to touch. A performance of the "haka," the old New Zealand dance, was got up for the author and his friends, on their paying a sovereign a-piece, but the young Maoris seem not to care for the dance any longer, and to perform it only for the benefit of tourists for money, and the performance lacked spirit, and soon came to an end. The hula, hula, in the Sandwich Islands, seems to be dying out in the same manner, and in Tahiti, when the old lascivious dances are performed, they are usually got up for the benefit of European visitors, through the agency of the native washermen, who combine such offices with their legitimate business. The young Polynesians in New Zealand, as elsewhere, prefer the waltz

and other European dances. The author enjoyed himself so much in Ohinemotu that he spent all his money and had to travel to Tauranga on foot. Here he saw the ceremony of nose-rubbing performed with great solemnity; it is, however, now kept up almost only by the old; the young Maoris have taken to kissing as a substitute. There is still a wide tract of the northern island of New Zealand 1,000,000 acres in extent, known as King Country, inhabited by about 10,000 Maoris under King Tawhiao, who keep themselves free from British rule, and do not permit whites to enter their country. King Country seems to be a thorn in the side of the Government, and the city of refuge of murderers and thieves, who are there out of harm's way.

At the Fiji group, the author visited only one island, that of Kandavu, at which the mail steamers call. He, however, explored a great deal of this island, in company with the natural history collector employed by Goddefroy Brothers, Herr Kleinschmidt, who is laboriously exhausting the fauna of the Fiji group, collecting carefully for several months in each island. The author's account of Kandavu, and especially of the natives, is full of interest. One of his observations may be cited here. A Fijian youth, employed as an assistant in collecting, whose body was already beautified by many cicatrisations, was devoting his attention to two groups of small suppurating wounds on the outer side of each upper arm. When visitors from neighbouring villages were present, he used to open these wounds anew and inflame them with a burning stick, or sand, or by scratching them with glass, in order to show his fortitude, never moving a muscle of his face in public, but making very wry faces afterwards in private. Many other youths had similar wounds on their upper arms, and it turned out that vaccination was being carried out by the Government in Kandavu, village by village, and that pustules on the upper arm were hence the fashion. The dandies would not wait till the turn of their village arrived, or perhaps the natives wished to avoid the actual operation by giving their arms the appearance of having been already vaccinated.

The author very rightly denounces the absurd method of spelling the Fijian language introduced by the missionaries. Because, in Fijian words, before the sounds *d g k* and *m*, an *n* nearly always is sounded, and before *b*, an *m*, the missionaries in first writing the language chose to omit the *m* and *n* in all cases in spelling before these letters, which complicates matters unnecessarily, and must eventually give great trouble to Fijians when they come to read English. Thus Thakombau, the name of the former king of Fiji, is spelt Thakobau. Kandavu is spelt Kadavu. Some writers have carried useless confusion still further, and have rendered *th* by *c*, so that Thakombau becomes Cacobau, and so it was most often spelt in newspapers at the time of the annexation of Fiji, so that English readers derived very little impression of the real sound of the name.

The author proceeded to Honolulu by Pacific mail steamer. Amongst the passengers was a San Francisco concert company and a reverend Yankee travelling lecturer. The concert company hoped to give a performance on the day on which the steamer stopped at Honolulu on its way to San Francisco, but the lecturer had been too sharp for them, and had engaged the only available hall long beforehand, and they found the town posted all over with advertisements of his lecture on the Tower of London. The author visited Hilo, in Hawaii, and the volcano of Kilauea. He returned from Hawaii to Honolulu in an open whale-boat, touching at the island of Maui on the way, an exploit which seems to have astonished the people of Honolulu extremely, since they have come to rely upon schooners and steamboats entirely for such long passages, and no longer make the voyage, as of yore, in war canoes. The author's account of San Francisco and its Chinese quarter is hardly so interesting as the earlier part of the book, as this quarter has been done to death in so many books of travel, and after all Chinese life at San Francisco is in all essentials identical with Chinese life at home. Perhaps before many years we shall have a Chinese quarter in London.

The official account of the Pacific railroads, sold on the line, "Williams' Pacific Tourist," which is got up in the interests of the railroad companies, is very properly denounced by the author. It is, indeed a shameless puff of the supposed beauties of the scenery on the line of the railroad, which exist for the most part only on paper and in the fervent imagination of the writer. An account of Salt Lake City and Niagara close Dr. Max Buchner's very pleasant volume. Some passages in the book are rather free in their tone; a case of midwifery on board ship is described with needless detail; many of the doings of Polynesians are also described with little reserve.

OUR BOOK SHELF

Index Medicus. A Monthly Classified Record of the Medical Literature of the World. Compiled under the Supervision of Dr. John S. Billings, Surgeon U.S. Army, and Dr. Robert Fletcher, M.R.C.S. Eng. (New York, F. Leypoldt; London, Trübner and Co.)

FOR some time back Dr. Billings, of the United States Army, has been engaged in the preparation of an Index-Catalogue of the library of the surgeon-general's office at Washington. To those who do not understand what this work is, this may not seem to be at all extraordinary, but those who know that the work is really an universal catalogue of medical literature, giving not only the names of the authors, but the subjects of the papers which have appeared in all medical periodicals throughout the world from the time of their first issue until the present, will be astonished that any man has had the courage to undertake such a task, and still more to learn that the MS. of this catalogue is now nearly ready for press, and is only awaiting the authority of Congress to print it. For the sake of medicine throughout the world we trust that this authority will be granted without delay, for to every man who has the interests of medicine at heart this work will be an invaluable boon. It has been suggested that such a catalogue should be supplemented by some current publication, which should show all recent works, together with articles and periodicals arranged by subjects, and the present publication has been issued to supply this want.

"In its pages the practitioner will find tables of parallels for his anomalous cases, accounts of new remedies, and the latest methods in therapeutics. The teacher will observe what is being written or taught by the masters of his art in all countries. The author will be enabled to add the latest views and cases to his forthcoming work, or to discover where he has been anticipated by other writers; and the publishers of medical books and periodicals must necessarily profit by the publicity given to their productions."

Dr. Billings very sensibly suggests that all medical men who approve of the objects of this Index, will put their approval into practical shape by subscribing promptly for the Index and taking care that a copy of every book, pamphlet, &c., of which they are the authors, is forwarded to the editors. This recommendation we heartily endorse, and trust that so useful a publication may receive the support it deserves.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Tides at Chepstow and Fundy

In NATURE, vol. xix. p. 363, Mr. Moseley, of Exeter College, Oxford, quotes a passage from Lyell's Principles, to the effect that the tides at Chepstow have a range of 72 feet.

This statement is continued in the last edition (1875), in Chap. XX., on Tides and Currents, p. 492; and the tidal range in the Bay of Fundy is set down at 70 feet (p. 564). I do not believe either of the statements.

The Admiralty Tide Tables give the following:—

	Spring Range. Feet.	Neap Range. Feet.
1. Chepstow	38	28·5
2. Bay of Fundy (Noel Bay) ...	50·5	43·5

I may add that Lyell's whole treatment of the subject of the tides is loose and inaccurate. Thus, for example, he says (p. 491): "In any given line of coast the tides are greatest in narrow channels, bays, and estuaries, and least in the intervening tracts where the land is prominent."

He then proceeds to illustrate this proposition by giving the ranges of tide from the mouth of the Thames to Flamborough Head (including, of course, the well-known *tidal node of minimum range* off Yarmouth); but he is utterly unconscious that these ranges depend altogether on the tidal motion of the water, and have no relation whatever to the form of the coast.

Trinity College, Dublin, March 3 SAML. HAUGHTON

Magnetic Storms

It is surprising that an accomplished telegraphist like Mr. Mance (NATURE, vol. xix. p. 409) should not see the necessity and advantage of expressing earth-currents in webers. It is precisely because every one can, if he likes, appreciate the magnitude of an earth-current so expressed, and no one but himself can do so if Mr. Mance's plan were adopted, that I advocate the weber, or rather, its more convenient sub-multiple, the *milliweber*. A milliweber is the current produced by one Daniell's cell (strictly one volt), through 1,000 ohms. Currents can be reduced to this unit from any galvanometer. The tangent galvanometer is, perhaps, the simplest to use—it is that which we employ in England. Supposing for simplicity that your constant, viz., one Daniell cell through 1,000 ohms (including cell and galvanometer) gives 45°, then the tangent of any other reading will give you the current in milliwebers. Then, knowing the resistance of your circuit and its geographical position, you have all the data necessary to determine the elements of earth-currents.

I will act on Mr. Mance's suggestion, and bring the matter before the Society of Telegraph Engineers with a view of organising a systematic mode of observation in different parts of the world. W. H. PREECE

Wimbledon, March 8

Atmospheric Pressure and Solar Heat

May I be permitted to supplement the table given by Mr. Allan Broun in NATURE, vol. xix. p. 7, by the following figures for Calcutta. The pressure anomaly at Bombay for each year, as given by Mr. Broun, is here compared with Calcutta, and the table is extended down to 1877:—

Table Showing Difference of Mean Pressures at Calcutta as Compared with Bombay for the whole Year and the Summer Months, the Sign + Indicating an Excess at Calcutta, and − a Defect; and Character of the Rainfall in the North-West Provinces and Behar.

Year.	Year.	Summer.	—
1847	−37	+ 5	Excessive.
1848	+ 7	−13	Defective.
1849	+14	+ 5	Excessive.
1850	+21	+ 5	Average.
1851	+ 8	+11	Excessive.
1852	+16	+18	,,
1853	−19	−31	Defective.
1854	+ 1	− 2	Excessive.
1855	−10	−30	Defective.
1856	− 2	− 1	Average.
1857	− 9	−10	Defective.
1858	− 3	− 9	,,
1859	+ 8	+26	Excessive.
1860	−15	−13	Defective.
1861	−12	− 3	Average.
1862	+12	+15	Excessive.
1863	− 7	−16	Defective.
1864	−14	−27	,,
1865	+15	+13	Excessive.
1866	−10	−20	Defective.
1867	+ 5	+ 4	Average.
1868	− 6	−13	Defective.
1869	+ 1	+ 5	Excessive.
1870	+ 3	+ 4	,,
1871	−37	− 3	,,
1872	+13	+18	,,
1873	+ 3 ?	−11	Defective.
1874	+ 5 ?	+24	Excessive.
1875	−15	− 2	Average.
1876	−24	− 5	Defective.
1877	+14 ?	+ 3	,,

Since from 1847 to 1852 there are frequent blanks in the registers, sometimes exceeding ten days in length, the figures are of somewhat doubtful value.

It will be seen on examination that the decennial period is nearly as distinctly marked at Calcutta as at Bombay, though the minor fluctuations are more frequent, and that, as Mr. Chambers supposed, the amplitude of the anomaly, like that of the diurnal and annual variation, is greater at Calcutta than at Bombay.

Mr. Broun considers the discovery of the decennial period of barometric pressure to be one of great importance, as forming a link in the chain of evidence which connects the variations of rainfall with those of the sun's heat. As an illustration of the way in which the variations of pressure influence the distribution of rainfall, I have entered a word descriptive of the character of the rainfall of each year in the North-West Provinces and Behar. In the great majority of cases a relatively low pressure at Calcutta, especially during the summer months, April to September, means defective rainfall over the valley of the Ganges, which is watered by easterly winds from the Bay of Bengal, and a relatively high pressure at Calcutta means excessive rainfall. Had the station with which Calcutta is compared been situated seven or eight degrees to the north of Bombay, the rule would probably have been without exception. The influence of what he calls the "relative barometric anomalies" upon the distribution of rainfall in India has been pointed out by Mr. Blanford on more than one occasion since 1868, and the examples here given will illustrate and enforce this point.

Mr. Broun's remarks regarding the relations between the range of the monthly means of barometric pressure and that of the monthly mean temperature, are interesting as confirming the views put forward by General Strachey, as long ago as 1850, in an unpublished work on the physical geography of the Himalaya and the neighbouring countries. The range of the monthly mean pressure at Calcutta is 0·488 inch, and that of the monthly mean temperature 18°·6 F. The range for one degree is therefore ·026 inch, a figure which differs little from those for Madras, Bombay, and Trevandrum. S. A. HILL

Allahabad, December 7, 1878

Intellect in Brutes

IN answer to the objections raised by W. P. Buchan and Henry Muirhead to the case of rats gnawing water-pipes for the express purpose of obtaining water, as described in my letter to NATURE, vol. xix. p. 365, I propose to give particulars of the situation of the pipes so gnawed in two instances. No. I. At Poplar. Pipe laid on second floor, between flooring-boards above and ceiling below, between joists the usual distance apart; plenty of room all round pipes to obtain access to any part of the floor, also rat-holes in woodwork to facilitate communication. As the pipe lay above the ceiling of the floor below, there was no necessity to attack the pipe in order to get through that ceiling—if that were desired. A hole could easily have been made at any point on either side of the pipe. The upper floor was a corn loft, and it is inferred from the circumstances, that the rats, feeding upon this very dry food, had tapped the water-pipe to obtain a supply of water close at hand. No. II. On Haverstock Hill. Cistern in scullery about six feet from the floor, pipe (lead), comes thence down wall, passes under floor of scullery to kitchen to supply kitchen boiler. Pipe laid on the *earth*, between joists, raised, of course, on bricks. Pipe gnawed on upper side. Plenty of room all round for rats to pass freely under all parts of the floor, as any practical builder will readily understand. They could have tunnelled *under* this $\frac{3}{4}$-inch pipe from either side through the soft earth. Of the two holes in the pipe one will admit a small pin, the other is about $\frac{1}{20}$th of an inch in greatest diameter, and about an inch from the first. These are transverse perforations, the ineffectual attempts are in a longitudinal direction. It is of course quite possible, as in Mr. Buchan's cases, that rats occasionally find gas-pipes in their way and are *compelled* to attack them, but I do not think any of your readers will imagine that his question, "Now, are they cut to get at the gas?' needs a moment's consideration. Possibly some of your correspondents may be able to corroborate the following—A ship's carpenter told me that, in the old days before the use of iron tanks on board ship became general, the rats used to attack the water casks, cutting the stave so thin that they could suck the water through the wood, without actually making a hole in it. If any one could substantiate this it would have an important bearing on the question under consideration.

ARTHUR NICOLS

I AM glad my remarks have elicited others, for I want to have my difficulty solved, which I will put thus:—Why is it that no dog ever (to my knowledge, of course) observed a person ring a bell, noticed that the bell brought the servant, and then went through the process of reasoning—"Because such was the result I will ring the bell too"? This I call abstract reasoning. On the other hand, why is it at all necessary to teach the dog to ring the bell? for it is not necessary to teach a boy. Boys do acquire sooner or later abstract reasoning, but it is with them practically a feeble power, as I have shown, and with rustics it seems, sometimes, to be totally in abeyance, as the following illustration will show. An entertainment was given to some hundred labourers and their wives, and a Mr. Cross told them to spend the afternoon in a field, but hoped none of them would be *his name*. Not one understood him, or could go through the simple process of reasoning—"We are not to be his name, his name is Cross, therefore we are not to be cross." It is this *mental reflection* which seems to me to be wanting in animals; thus, monkeys will warm themselves by a fire of burning sticks, but do not seem capable of thinking—"Because sticks burn, therefore, if I put more sticks on the fire I shall get more warmth." I should be extremely glad to hear of any cases of such purely *abstract mental reflection* in animals, for at present, there seems to me to be a hiatus here.

GEORGE HENSLOW

On the Freezing of Lakes

WE wish to draw your attention to a statement contained in an article under the above heading which appeared in NATURE last week, p. 412.

The author, Mr. J. Y. Buchanan, says:—

"Most of the observations were made with one of Negretti and Zambra's 'half turn' deep-sea thermometers, which proved a useful instrument for this species of inquiry. It was necessary, however, to fit it with a suitable inverting contrivance, as the apparatus supplied for this purpose by the makers is quite useless."

Fortunately this thermometer and apparatus has now been in use for some time, and its performance fully tested by persons whose opinion is of the highest value. We can only say that had Mr. Buchanan used the apparatus according to our printed direction, viz., lowered the instrument to the required depth, let it remain a few seconds, and then pulled it up as fast as practicable and without stopping, he would not have failed to have obtained correct results. We cannot possibly imagine how he could have failed; had we not ourselves tried the inverting apparatus in every possible way, and had we not received the highest testimony as to its efficiency, we would have remained silent, but as it is, and knowing we have invented as good an instrument as has ever been contrived for the purpose, we cannot allow the statement to pass unchallenged that we have supplied a useless apparatus. If Mr. Buchanan could not obtain satisfactory results with our apparatus it must only have been because he did not use it as we directed.

HY. NEGRETTI AND ZAMBRA

THE MIGRATION OF BIRDS

MORE than four years ago an article, headed as above, appeared in this journal (vol. x. p. 415) giving rise to some comments of more or less importance (*tom. cit.*, pp. 459, 520, and xi. p. 5). Since that time two very remarkable treatises on the subject have come forth, the one by Dr. Palmén, on the routes taken by birds in their migrations, and the other by Dr. August Weissmann, dealing generally with the whole question.[1] The first, originally published in Swedish,[2] was translated into German soon after, and in due time was reviewed in these columns (vol. xv. p. 465). An excellent English version of the second has recently appeared in the "Contemporary Review" (February, 1879, p. 531), and therefore my readers may be presumed to be acquainted with the views of both authors. It cannot be denied that each of them has to some extent enlarged the boundaries of our knowledge of the subject, and still more widely those of our speculations upon it.

As regards Dr. Palmén's work, the opinion held by his reviewer in these columns as to the assignment of routes to the migratory birds of North-Western Europe being "purely conjectural" is one that I wholly accept. I should even be inclined to go further, and say that it might be called rash, as it is evident that no such observations as would justify its adoption exist. Still I concur with the reviewer in that I would not at present term it entirely erroneous, though I venture to express my entire disbelief in the route "X." This is supposed by Dr. Palmén to start from Greenland and Iceland, and to pass by the Færoes to the Hebrides, when, after coalescing for a short distance with one or more lines from the north-east, it either loses itself on the West Coast of Ireland, or, running down St. George's Channel, skirts Scilly and crosses to the shores of the Bay of Biscay. Space forbids my entering into details which would show, I think, that this route is altogether imaginary. I will only say that what we know of the movements of two very characteristic summer visitants to Iceland, *Motacilla alba* and *Limosa ægocephala*, points to that conclusion, and I would leave it for Irish ornithological observers to prove whether Dr. Palmén is right or not.

Dr. Weissmann, happily for him, has no such compunction. He fully accepts Dr. Palmén's conjectures as absolute truths. Suppose, however, we assume them to be established—and there seems no reason why further observation should not establish most of them—they would show in Dr. Weissmann's opinion that the migrating birds of to-day in crossing the sea follow what once were "land-bridges" (*Landbrücken*), that is, isthmuses of dry land separating seas or oceans under which they are now submerged. The existence of such former terrestrial communications between continent and con-

[1] Sammlung gemeinständlicher wissenschaftlicher Vorträge. XIII. Serie Heft 291. "Ueber das Wandern der Vögel." Berlin, 1878.

[2] Om Foglarnes flyttningsvägar. Helsingfors, 1874.

tinent, at the present time revealed to us by shallow soundings and some mountain-peaks changed into islands, may be indubitable. I, at least, in regard to Europe and Africa do not question it, and it may be true all the world over. But this same hypothesis has been more or less hinted, if not absolutely promulgated, by Prof. Baird (*American Journal of Science and Arts*, May, 1866) and Capt Hutton (*Trans.* N. Zealand Inst., v. p. 235). So far, then, there is nothing novel in the Doctor's views.

In like manner Dr. Weissmann seems to me to have been anticipated by Capt. Hutton (*ut suprà*) and by Mr. Wallace (NATURE, vol. x. p. 459) in his explanation of why birds migrate at all. The only material difference between the last of these authors and the Doctor is that, while Mr. Wallace most rightly (as it seems to me) regards migration as originating with the bird at its breeding-quarters, Dr. Weissmann considers it to begin with the bird in its winter retreat. Perhaps this does not much matter, but it is as well not to put the cart before the horse if you want to prosper in your journey, and so long as lack of food be admittedly the strongest incentive to migration, it seems preferable to look on migration as beginning where that incentive is strongest. This, it scarcely needs to be said, is when, towards the close of summer, the supply of food grows scarce.

However, the most important part of the whole business is the question how the birds find their way to the places whither they repair, whether for the purpose of breeding or for that of procuring sufficient sustenance. Dr. Palmén regards it simply as a matter of "experience," and Dr. Weissmann hardly differs from him. It is "practice" (*Uebung*), says the latter—not indeed the practice acquired by the single bird, but the practice acquired by the whole species. "This faculty (*Virtuosität*) of finding the way has not arisen suddenly, but most gradually, in the course of many thousands of generations." Now with all my faith in the marvellous results which are doubtless produced by the hereditary transmission of certain qualities, I think some caution is needed before we accept "practice" as the true explanation of the puzzle. Dr. Weissmann says that he does not see what more is needed than a fine power of observation and a keen eye to take in every thing of importance for a knowledge of the way, and then a very remarkable memory for places by means of which all details of the long route shall be retained. The knowledge of direction (*Orientirung*) will then follow of course. Subsequently he takes the instance of a woodpecker being able to find the tree containing its nest, though surrounded by hundreds of similar trees, and declares that this knowledge or sense of direction must, in the case of birds when migrating, be wholly analogous. He suggests also that the height at which birds sometimes fly, referring apparently to a remark by Mr. Tennant (NATURE, xiii. p. 447), would enable them to cross the Mediterranean, and seldom or never lose sight of land.

This may be; but migration goes on in other parts of the world, and a good explanation ought to apply elsewhere. Will Dr. Weissmann's hold good for our Antipodes? In regard to New Zealand Capt. Hutton has remarked (*Trans.* N.Z. Inst., v. p. 235):—"That we should have two cuckoos which migrate regularly to other countries, each more than a thousand miles distant, is a fact that deserves special attention, for I know of no parallel case in any other part of the world, the distance across the Mediterranean being less than half that travelled over by our summer visitors." These two cuckoos are *Chrysococcyx lucidus* and *Eudynamis taitensis*, the former, it is supposed, making its annual journeys to and from Australia,[1] and the latter to and from the Friendly Islands or the Fijis, it being found in both groups, to say nothing of other places further off. Let us consider the case of the *Eudynamis*. Due north of New Zealand there appears to be no land until Fiji is reached, but a little to the westward of the direct line lies Norfolk Island and its companions, and about as far to the eastward are the Kermadecs. Of these, the most southerly is 450 miles from New Zealand, and the most northerly about the same distance from Pylstaart, an outlier of the Friendly Islands. The *Eudynamis* starting from New Zealand for the northward would have nothing to supplement its inherited "sense of direction" save the landmarks offered to the right and left by the Kermadecs and Norfolk Island respectively. To see the former it would have to mount to the height of some twenty miles,[1] and to again mount about as high on leaving the Kermadecs on its way to Pylstaart. It might be urged that the bird having by "practice" a sense of the direction in which it ought to go, might fly half the distance, keeping the land of departure in sight—though, considering the position of birds' eyes, this would not be easy—and then, without exceeding the level of six or seven miles, it might behold the Kermadecs, but even this is an elevation far beyond Dr. Weissman's 20,000 feet. The route by Norfolk Island being longer, need not here be discussed.

[1] I should rather suspect to and from New Caledonia.

It is much to be desired that something positive were known as to the height at which it may be possible for birds to perform their passages, but on this point we have (so far as I am aware) little information. The experiments made by Mr. Glaisher on the six pigeons taken up in his celebrated balloon ascent, September 5, 1862 ("Rep. Brit. Ass.," 1862, p. 385), unfortunately admit of no definite deductions. One pigeon thrown out at the height of three miles "extended its wings and dropped as a piece of paper." A second at four miles "flew vigorously round and round, apparently taking a dip each time." A third between four and five miles "fell downwards as a stone." A fourth at four miles, in the descent, "flew in a circle," and then alighted on the balloon. The two remaining pigeons were brought down, and one was found to be dead! Perhaps a little more "practice" or "experience" was wanted, but at any rate the results do not seem to favour the notion that birds can fly comfortably at those heights. Nor is this surprising, considering the well-known effects of the rarification of the air at great heights. I of course pretend to no special knowledge of this subject, but Mr. J. W. L. Glaisher, F.R.S., kindly informs me that at an elevation of five miles the density of the air is about $\frac{1}{2\cdot7}$ of what it is on the earth's surface, at an elevation of seven miles about $\frac{1}{4\cdot1}$, and of ten miles about $\frac{1}{7\cdot5}$. I know not whether experiments have been made to test the endurance of a bird's life under such a condition as the last, but it could of course be easily produced under an air-pump. It would not be so easy to test the power of flight under the same condition. It is only obvious that the power would be very greatly diminished, and I should be glad to learn the results of any investigation of this kind. Physicists and physiologists might here give ornithologists great help.

But to return to the question of distance and sight. How comes it that the American Golden Plover (*Charadrius virginicus*) passes regularly every year in large flocks over the Bermudas, 600 miles from the nearest point of land, and that a point whence these flocks certainly do not take their departure. If the islands are "still vext" by stormy weather, the flocks alight and afford the inhabitants a good deal of sport. If the weather be fine, the flocks seem to continue their southward course. Nor is this plover the only *regular* visitant. The American Night-Hawk (*Chordiles popetue*) is as constant in its appearance at spring and fall, and so are

[1] Of course the exact height would depend on the elevation of the land, concerning which I have no information.

more species of *Limicolæ*, and perhaps of other groups, than I can here name. Is it to be supposed that all these birds, some of them flying by night, make Bermuda by the means which Dr. Weissmann considers sufficient? If his explanation is good it must be good for New Zealand and Bermuda, as well as for the Mediterranean. But there is yet a stronger case to be cited. The Sandwich Islands, as I have learned on authority I cannot doubt (though I know not any mention of the fact in print), are yearly visited like Bermuda, but with even greater punctuality, by large flocks of Golden Plovers—whether *C. virginicus* or *C. fulvus*, is undetermined, but that does not much matter. If the birds belong to the first of these forms they must come from the west coast of America, if to the second from the east coast of Asia. Now there is no land between the Sandwich Islands and the Californian or the British-Columbian coast, but between them and the Aleutians, as I learn from Mr. Rye, there is one islet, Roca de Plata or Crespo. This, however, does not lie in a straight line, and is some 720 miles north-west of the Sandwich Islands, and 1,200 south-west of the Aleutians. Running generally westward of the Sandwich Islands is a series of islets, at distances, perhaps, not exceeding 150 miles, which no doubt might serve as guide-posts for the plovers did they but make for them, but the series comes to an end in about long. 178° W., and though by turning suddenly to the north-east from Morell Island towards Mellish Bank (300 miles), the Aleutians again appear as the nearest land in a northerly direction, the distance of 1,020 miles has to be covered! On the supposition that the birds are of Asiatic birth, and therefore come by another course, we find that due west of Morell Island is Ganges Island, but that is 930 miles off, and thence to reach the easternmost of the Japanese group is 690 miles further! Thus, whichever form of golden plover it be that visits the Sandwich Islands, its regular advent there needs, I think, some fuller explanation than that afforded by Dr. Weissmann's theory.

Then again, there is another set of facts which seem to me irreconcilable with the theory of mere "practice" or "experience." It must be remembered that though Dr. Weissmann relies most on the inherited practice of the *species*, still he does not neglect the *individual*, and both he and Dr. Palmén make considerable use of the observation that adults, and male adults in particular, lead the migratory flocks. This fact, so far as I am aware, has only been noticed in the northward movement in spring, and elsewhere I have endeavoured to account for it ("Encycl. Brit.," ed. 9, iii. p. 767). In autumn it may be doubted whether there is anything of the kind, and we have in many species, the young of the year—birds that are but three months old, or even less, migrating southward with the greatest regularity unaccompanied by adults. This seems to happen with nearly all the *Accipitres*, nearly all the *Limicolæ*, and perhaps, some others, that are bred in arctic or sub-arctic districts. It happens also with our own Cuckoo (*Cuculus canorus*), and this case is still more wonderful, for the young Cuckoo has had no communication whatever with its progenitors (who have already taken their departure from our shores some weeks earlier), and its foster-parents with us are generally species which do not migrate to any great extent—the Hedge-Sparrow (*Accentor modularis*), Titlark (*Anthus pratensis*), and Pied Wagtail (*Motacilla lugubris*). Yet our young Cuckoos, starting alone and travelling over utterly unknown country, must, on the whole, successfully reach their destination, or the breed would become extinct here.[1] Dr. Weissmann may indeed well say of migrating birds, that the young, when it cracks the shell, possesses "great geographical talent"!

I might easily prolong this article, for there is much more to be said on the subject, and in some details, by no means unimportant, Dr. Weissmann seems to have fallen into errors that I have not here noticed, but my chief object in making these remarks has been to hinder persons who have not previously thought on the matter from taking his easy explanation of the mystery of ornithological mysteries to be sufficient. Believing, as I once before said, that its solution is probably simple in the extreme, and having a strong faith in the hereditary transmission and accumulation of faculties so as to become a wonder-working power, I yet cannot think that he has succeeded in making known the secret and in satisfactorily explaining how birds cross "the sacred spaces of the sea."

ALFRED NEWTON

[1] Since I wrote this I have heard from Mr. Gätke, so well known for his observations on migratory birds, that young Starlings pass over Heligoland during July by hundreds of thousands, "without a single old bird accompanying them," while the old birds begin to migrate at the end of September and continue for the next two months.

THE DIMENSIONAL PROPERTIES OF MATTER IN THE GASEOUS STATE

BY assuming a sufficient number of sufficiently insignificant individuals to constitute a group, it is possible to imagine a state of things in which as far as it can be observed from a certain distance, all evidence of individual action is entirely lost. In this way has been framed the molecular hypothesis or kinetic theory of gas. But it must be obvious to every one who has considered this molecular hypothesis, that the apparent uniformity in the actions we perceive must be the result of the distance (so to speak) from which these actions are observed, and that could we sufficiently localise (as regards time and space) our observation, we must perceive in all their varieties the individual actions of the molecules. And even failing this, between the actions of the individuals and the absolute mean action there must be local or parochial actions which would be distinguishable at greater distances than the purely individual actions.

In order that the properties of a gas may appear perfectly uniform in all directions and quite independent of the shape and volume of the space which the gas is constrained to occupy, the number of molecules must be countless, and the temporary action of each individual must be confined to an imperceptibly small portion of the space observed. If these conditions are not fulfilled then the properties of the gas will not be uniform, and we must have dimensional properties depending on the dimensions of the constraining surfaces.

The idea of our being able actually to perceive such properties does not appear to have been entertained hitherto. Until fifty years ago, all the mechanical properties of gases were regarded as quite uniform, the only mechanical distinction between one kind of gas and another being that of weight. Since that time in the phenomena of diffusion, and the phenomena attending the passage of gases through minute channels, properties of gases have been recognised which readily distinguish between one kind of gas and another, and even more than this, for Graham found that there was a difference in the relative behaviour of different gases in differing through porous plates of different coarseness. Still, neither Graham nor any one else appears to have recognised evidence of dimensional properties.

Neither did the development of the mathematical theory lead to the revelation of dimensional properties. Since 1864 it has been known that this theory included the explanation of all the uniform properties of gas. But in developing this theory attention appears to have been paid exclusively to the mean of the motions. And although Prof. Maxwell points out that there must ultimately be dimensional properties, he has not pursued the investigation, so as to reveal their character.

In 1874 a very remarkable phenomenon was brought to light by the experiments of Mr. Crookes—that in ex-

tremely rare media light bodies are apparently repelled by hot and attracted by cold bodies. At first this was not recognised as a gaseous phenomena—in fact the non-presence of gas was supposed to be essential to the occurrence of the highest form of the action.

That such motions might result from the inequalities in the pressure of the residual gas caused by the communication of heat between the objects and the gas, was first shown by the author in May, 1874 (*Proc.* Roy. Soc. 1874, p. 402).

It was shown that when heat is passing from a surface to a gas whether by direct communication or by evaporation the reaction from the receding molecules causes an excess of pressure proportional to the heat communicated.

The reasoning was definite so far as it went and, although manifestly incomplete, the conclusion arrived at, viz., that the phenomena resulted from the heat communicated to the residual gas appears very soon to have been generally accepted as it was found to be verified in various ways. Several papers appeared in which attempts were made to render the explanation more complete, but these attempts were for the most part based on a misconception of the phenomena to be explained and were altogether wide of the mark.

In November of 1878 the clue to the step that was wanting to complete the explanation occurred to the author. It was then seen that although the surfaces with which the phenomena occurred were of limited extent, no account had been taken of this fact in the attempts to explain the actions. Having once hit on this point the deduction of a complete theory of the phenomena was only a matter of labour. It was found that although the excess of pressure is proportional to the quantity of heat communicated, it also contains a factor which is proportional to the divergence of the lines of heat flow; and hence the reciprocal of the density of the gas at which the phenomenon would occur should vary inversely as the size of the vanes. So that by using vanes of comparatively small size the phenomena should be obtained at proportionally greater densities of the gas.

On considering how this conclusion might be experimentally tested it appeared that in order to obtain any results at measurable pressures the vanes would have to be very small indeed, too small almost to admit of experiment. It was while searching for some means to overcome this difficulty that it became apparent that if the vanes were fixed, then, instead of the movement of the vanes, we should have the gas moving past the vanes —a sort of inverse phenomenon—and then instead of small vanes small spaces might be allowed for the gas to pass. Thus the probable existence of phenomena of *Thermal Transpiration* was suggested, and it was obvious that the porous plug would furnish the means of verifying the conclusions. This probable connection between the phenomena of the radiometer, which may be called *Impulsion*, and the phenomena of transpiration through porous plugs raised the question whether the same extension of the dynamical theory of gas which explained the radiometer would not include the results obtained by Graham not then explained. This idea was followed up, and a method was devised of extending the dynamical theory of gases so as to take into account the forces tangential, and normal, arising from a varying condition of molecular gas.

This theory appeared to explain fully all the results established by Graham as well as the then known phenomena of impulsion (the radiometer), besides definitely indicating the phenomena of thermal transpiration to be expected, as well as the effect of employing small vanes in the radiometer. That this step had been accomplished was intimated by the author in the following passage in a letter published in NATURE, vol. xix. p. 220.

"Now, however, I have arrived at a result, which, although somewhat unexpected and striking, will, I hope, be found to reconcile what has hitherto appeared to be anomalous in the phenomena already known, and to have suggested certain hitherto unexpected phenomena which now only await experimental verification."

This experimental investigation which was at once commenced, proved to be a much more serious undertaking than had been anticipated, and was not completed until the end of August, this was not so much on account of difficulties, although these were considerable, but because it was found possible to do so much more than had been expected.

One of the results of the investigation was to show that a difference of pressure is maintained whenever two chambers of the same gas are separated by a porous plate, one face of which is hotter than the other.

With a plate of meerschaum ·44 inch thick, one side of which was something less than 212°, while the other side was about 50°, the difference of pressure was ·25 inch of mercury with air at the atmospheric pressure, and ·88 of an inch with hydrogen at the same pressure.

The existence of this thermal transpiration, although a new phenomenon, is not considered to be the most important result of this part of the investigation, for it appeared on comparing the results obtained with different plates, and different densities of gas, that there was a constant relation between the pressures for different plates at which proportional results were obtained. Thus comparing a plate of stucco with a plate of meerschaum, it was found that the ratios of the difference of pressure to the mean pressure, was the same for both plates so long as the mean pressure with the meerschaum was six times greater than with the stucco, and that this law held although the mean pressures with the meerschaum ranged from 30 to 1·25 inches of mercury. This ratio of the pressures at which corresponding results were obtained, was the same whether the gas used were hydrogen or air; and for plates of different thickness, and hence it was clearly shown to depend only on the coarseness of the plates.

Thus in thermal transpiration we have a phenomenon of gaseous motion depending on a relation between the density of the gas and the dimensions of the space which it is constrained to occupy. The discovery of this relation between the density of the gases and the coarseness of the plates at which corresponding results of thermal transpiration were obtained, suggested the possibility of obtaining a like relation for corresponding results when the gases were forced through the plates by a difference of pressure. Graham had found that the relative rates at which different gases transpired through plates differed very considerably with the coarseness of the plates and no explanation had been given of the phenomena. On trying the experiments not only was it found that with plates of different coarseness corresponding results were obtained whenever the pressures of the gas have a constant ratio but it was also found that with the same plates the ratio was the same as in the case of thermal transpiration.

A successful attempt was then made to verify the conclusion that the phenomena of the radiometer might be obtained at higher densities by using smaller vanes. By suspending fibres of silk and spider lines the repulsive action of heat was rendered apparent at pressures ranging up to the pressure of the atmosphere.

These results, as well as the theory from which they were deduced, have been fully described in a paper, an abstract of which was read before the Royal Society on the 6th inst.

As regards transpiration and impulsion, the investigation appears to be complete; most, if not all, of the phenomena previously known have been shown to be such as must result from the tangential and normal stresses consequent on a varying condition of a molecularly constituted gas; while the previously unsuspected

phenomena to which it was found that a variation in the condition of gas must give rise, have been found to exist.

The results of the investigation lead to certain general conclusions which lie outside the immediate object for which it was undertaken; the most important of these is that gas is not a continuous plenum.

The experimental results considered by themselves bring to light the dependence of a class of phenomena on the relations between the density of the gas and the dimensions of the objects, owing to the presence of which the phenomena occur. As long as the density of the gas is inversely proportional to the coarseness of the plates, the transpiration results correspond; and in the same way, although not so fully investigated, corresponding phenomena of impulsion are obtained as long as the density of the gas is inversely proportional to the linear size of the objects exposed to its action; in fact, the same correspondence is found with all the phenomena investigated.

We may examine this result in various ways, but in whichever way we look at it, it can have but one meaning. If in a gas we had to do with a continuous plenum, such that any portion must possess the same properties as the whole, we should only find the same properties, however small might be the quantity of gas operated upon. Hence, in the fact that we find properties of a gas depending on the size of the space in which it is inclosed, and on the quantity of gas inclosed in this space, we have proof that gas is not continuous, or, in other words, that gas possesses a dimensional structure.

In virtue of their depending on this dimensional structure, and having afforded a proof thereof, it is proposed to call the general properties of a gas on which the phenomena of transpiration and impulsion depend, the *Dimensional Properties of Gas*.

Although the results of the dimensional properties of gas are so minute that it has required our utmost powers to detect them, it does not follow that the actions which they reveal are of philosophical importance only; the actions only become considerable within extremely small spaces, but then the work of construction in the animal and vegetable worlds, and the work of destruction in the mineral world, are carried on within such spaces. The varying action of the sun must be to cause alternate inspiration and expiration, promoting continual change of air within the interstices of the soil as well as within the tissue of plants. What may be the effect of such changes we do not know, but the changes go on; and we may fairly assume that, in the processes of nature, the dimensional properties of gases play no unimportant part.

OSBORNE REYNOLDS

OUR ASTRONOMICAL COLUMN

THE BINARY STAR α CENTAURI.—Dr. Doberck, with the aid of measures made during the last few years has calculated elements of α Centauri, which, though given as only provisional, will doubtless approach nearer to the true ones than any previously published. They are as follows:—

Passage of the peri-astre	1875·12
Node	25° 32′
Angle between node and peri-astre	45° 58′
Inclination	79° 24′
Excentricity	0·5332
Semi-axis major	18″·45
Period of revolution	88·536 years.

Comparing this orbit with weighted means derived from Mr. Gill's measures at Ascension in 1877, the following differences are shown:—

1877·614	Position	+ 2°·32	Distance	− 0″·23
1877·858	,,	− 0·39	,,	+ 0·05

For 1879·5 the elements give position, 173°·4; distance, 3″·47; and for 1880·5, position, 185°·2; distance, 5″·30; the smaller star will be due south of the larger one at the beginning of 1880, distant 4″·37. The above value for the semi-axis major, taking the annual parallax of α Centauri, a mean between the values of Maclear and Moesta, indicates that the mean distance between the component stars is rather greater that the mean distance of Uranus from the sun. Frequent measures of α Centauri during the next few years are much to be desired.

A NEW VARIABLE STAR IN SAGITTARIUS.—The following case appears a singular one, if the star is not variable to a considerable extent:—On April 28, 1783, D'Agelet observed a star which he estimated of 4·5m., and which is No. 4,627 in Gould's Catalogue. It does not occur in Piazzi or Lalande, nor in Bode, but it is found on Harding's Atlas as 6m. It is wanting in the Uranometries of Argelander and Heis, but the former observed it three times in his Zones. In Z. 218, July 2, 1849, it is rated 5m.; in Z. 225, on July 13, only 7m.; and in Z. 391, June 30, 1851, it is 5·6m. The mean position for 1850 from Argelander's observations is in R.A. 17h. 59m. 6·57s., N.P.D. 107° 10′ 9″·9, or reducing to 1880 in R.A. 18h. om. 51·1s., N.P.D. 107° 10′ 10″. It is difficult to account for such an object having escaped the notice of other observers, except upon the supposition of variability; perhaps, like some other variables, it is only conspicuous for a short time. The star follows 6 Sagittarii 6m. 26s., in 1′·4 greater N.P.D. It is proper to state here that Mr. J. E. Gore, in his "Southern Stellar Objects," p. 104, has a reference to this star amongst stars possibly variable, but the observations of D'Agelet and Argelander do not appear to have been known to him.

NEW MINOR PLANET.—No. 193 was discovered by M. Coggia at Marseilles on March 1, not far from the place of No. 192, detected at Pola by Herr Palisa, in the previous month.

INTRA-MERCURIAL BODIES.—In a letter addressed to M. Mouchez, communicated to the Academy of Sciences at Paris on March 3, Padre Ferrari, Director of the observatory of the Collegio Romano, mentions that, having had occasion to institute researches respecting the observation of a rapidly-moving spot upon the sun's disk by De Cuppis on October 2, 1839, at the instance of Prof. Oppölzer, he had met with particulars of a similar observation by De Vico in 1837. Reference is made to this observation in "Memoria intorno ad alcune osservazioni fatte alla Specola del Collegio Romano, 1838," p. 15, but the year only is there mentioned. De Cuppis, a friend and frequently co-operator of De Vico's has, however, preserved the date in the journal *L'Album* for 1838, July 7, where the observation is thus described: "In una osservazione del 12 luglio, 1837 parve al sullodato astronomo (De Vico) del Collegio romano veder rinnovato il fenomeno, in una piccolissima macchia perfettamenta rotonda e senza traccia della cosi detta penombra, la quale nel breve spazio di 6 ore trascorse buona parte del disco solare."

This observation does not occur in Haase's Collection, nor is there here more than a reference to the observation of Decuppis, which is thus given in a note by Arago at the sitting of the French Academy on December 16, 1839:—"M. Decuppis annonce que le 2 Octobre, en continuant des observations qu'il faisait sur les taches du soleil, il a vu une tache noire, parfaitement ronde et à contours nettement terminés, qui s'avançait sur le disque de l'astre d'un mouvement propre rapide, de manière à ce qu'elle a dû en traverser le diamètre dans environ six heures. M. Decuppis pense que les apparences qu'il a observées ne peuvent s'expliquer qu'en admettant l'existence d'une nouvelle planète."

There are other observations upon record in January

and July which might apply to the object seen by De Vico, assuming it to have been an intra-Mercurial planet. Leverrier did not attempt to discuss these observations, confining his attention to those made about the equinoxes, clearly belonging to a different body.

GEOGRAPHICAL NOTES

SOME of our contemporaries have been a little premature in appointing the Earl of Northbrook to the presidency of the Royal Geographical Society, for at the time when the announcement was first made his lordship was not even a fellow of the Society. The fact, we believe, is that Lord Northbrook has expressed his willingness to accept his nomination by the Council, but there is no likelihood of any election taking place till the anniversary meeting on May 26th.

AT the meeting of the Geographical Society on Monday evening Dr. James Stewart, of the Livingstonia Mission, East Africa, read a paper on the "Second Circumnavigation of Lake Nyassa." The voyage was undertaken in the little steamer *Ilala* in the latter part of 1877. It was found that Lake Nyassa has hitherto been laid down on our maps too far to the east, and that its position ought to be shifted at the north end as much as thirty miles to the westward. Dr. James Stewart was also fortunate in discovering two harbours such as were needed for the safe navigation of the lake; the one Rombashi inlet or river on the northern coast, and the other the Kambwe lagoon a little to the south of it on the west coast. Mr. James Stevenson afterwards gave a few particulars respecting the explorations now being carried on by Dr. Laws and Mr. James Stewart, of the Bengal Civil Service, in the country on the western side of Lake Nyassa.

THE Abbé Debaize, the leader of the French expedition to Central Africa, sends to M. Richard Cortambert a letter, rejoicing at the success of his expedition so far. He writes from Taboro, in Unyamwezi, and states that hitherto he has been completely successful; there have been no desertions, little expenditure, and no misfortunes of any kind. This good fortune he ascribes to his own excellent health, to his firm discipline, and his personal superintendence of all arrangements. He has a very poor opinion of Miramboa, and thinks the English are striving hard to become masters in Central Africa; indeed he hints that "annexation" is not far off. At Mpwapwa, where the sole white population are an English "reverend, a mason, and a carpenter," there are already four stone houses, while there are English stations in Ukerewe, Uganda, and at Ujiji.

THE *Daily News* Lisbon correspondent states that the Portuguese Minister of Marine has informed the Cortes that he has received a telegram stating that the Portuguese African explorer Pinto, who was separated from his companions at Bihe, has succeeded in traversing Africa from west to east, having reached the Transvaal.

FRANCE and the United States will soon establish a comparison of the longitude of Paris and Washington by cable. As is known, these operations lead to a determination of the velocity of propagation of electric waves. Commander Perrier and M. Lœwy have recently published a volume giving the details of the comparison between Paris, Marseilles, and Algiers.

MR. STANFORD has just published a very fine stereographical map of Zululand, with portions of the adjoining territories. On the bases of what observations there are as to the physical conformation of the country, together with the pretty fair notions we have of the courses of the rivers, a good general notion is conveyed of the character of the surface and its various levels. The map conveys, moreover, a great amount of information in a clear and striking manner, that will be extremely useful to those who wish to understand the history and bearings of the Zulu difficulty.

UNDER the title of "Le Maroc" the current number of the *Tour du Monde* contains the commencement of a translation of M. Edmondo de Amicis' account of his experiences in Morocco in 1875. The present instalment deals with Tangier, and is accompanied by several illustrations.

WE understand that Prof. Geikie, of Edinburgh, will probably deliver a lecture on Geographical Evolution before the Royal Geographical Society on Monday, March 24.

THE Emperor of Austria has presented Captain von Oestreicher with the medal for letters and arts (*literis et artibus*) in recognition of his recent highly interesting geographical work, "Aus fernem Osten und Westen."

DURING last year the following journeys were made by Russian explorers in Central Asia:—Generals Stolyetoff and Razgonoff were accompanied during their mission to Kaboul by several topographers and explorers; M. Oshanin has made explorations in Karategin and Hissar; M. Matséeff in Badakshan and Eastern Afghanistan; M. Grodékoff in Western Afghanistan and Herat; M. Bykoff has explored the Amu darya River from Kobadian to Khiva; M. Yavarsky has traversed for the fourth time the region between Tashkend and Kaboul; M. Mayeff has visited for the second time the hilly track between Karshi, Keliff (on Amu darya), Kobadian, and Hissar; and, finally, the steamer *Samarkand* has navigated the Amu, from Petro-Alexandrovsk to Khodja-Sale.

UNDER the title of Société de Géographie de l'Est, there has been founded at Nancy a new geographical society.

A STUDY IN LOCOMOTION[1]

IF the interest of a scientific expositor ought to be measured by the importance of the subject, I shall be applauded for my choice. In fact, there are few questions which touch more closely the very existence of man than that of animated motors—those docile helps whose power or speed he uses at his pleasure, which enjoy to some extent his intimacy, and accompany him in his labours and his pleasures. The species of animal whose co-operation we borrow are numerous, and vary according to latitude and climate. But whether we employ the horse, the ass, the camel, or the reindeer, the same problem is always presented: to get from the animal as much work as possible, sparing him, as far as we can, fatigue and suffering. This identity of standpoint will much simplify my task, as it will enable me to confine the study of animated motors to a single species; I have chosen the horse as the most interesting type. Even with this restriction the subject is still very vast, as all know who are occupied with the different questions connected therewith. In studying the *force of traction* of the horse, and the best methods of utilising it, we encounter all the problems connected with teams and the construction of vehicles. But on a subject which has engaged the attention of humanity for thousands of years, it seems difficult to find anything new to say.

If in the employment of the horse we consider *its speed* and the means of increasing it, the subject does not appear less exhausted. Since the chariot races, of which Greek and Roman antiquity were passionately fond, to our modern horse-races, men have never ceased to pursue with a lively interest the problem of rapid locomotion. What tests and comparisons have not been made to discover what race has most speed, what other most bottom,

[1] "Moteurs animés; Expériences de Physiologie graphique." [Lecture by Prof. Marey at the Paris meeting of the French Association, August 29 1878.

what crossings, what training give reason to expect still more speed?

Lastly, as to what is called the exterior of the horse, and his varied paces, specialists have for long devoted themselves to this department. The horseman is trained to distinguish between these different paces, to correct by the education of the horse those which seem to him defective, to fix by habit those which give to his mount more pleasant reactions or a much greater stability. The artist, in attempting to represent the horse, seeks to transfer his attitudes more and more faithfully, to express better and better the force, the suppleness, and the grace of his motions.

These questions, so complicated, I wish to bring before you by a new method, and I hope to show you that the *graphic method* makes light of difficulties which seem insurmountable, discerns what escapes the most attentive observation; finally, it expresses clearly to the eyes, and engraves upon the memory the most complicated notions. The graphic method was almost unknown twenty-five years ago; to-day it is wide-spread. Thus, in almost all countries, recourse is had to the employment of graphic curves as the best mode of expression to represent clearly the movement of administrative, industrial and commercial statistics. In all observatories apparatus known as *registering* or *recording*, trace on paper the curves of variation of the thermometer, the barometer, rain, wind, and even atmospheric electricity. Physiology utilises still more largely recording apparatus; but I shall only require to show you a very small number of these instruments, those which serve to record forces, rates of speed, or to note the rhythms and the relations of succession of very complicated movements.

1. *Of the Force of Traction of the Horse, and the best Means of Utilising it.*—When a carriage is badly constructed and badly yoked the traveller is jolted, the road is injured, the horse is fatigued more than is necessary, and is often wounded by parts of the harness. Science and industry have long sought to discover these inconveniences, to find out their causes in order to get rid of them. But it is only in our own time that great progress has been made in this respect. When we complain of being jolted in a humble cab, we ought to go back in thought to the time when people knew nothing of the hanging of carriages. No roughness of the road then escaped the traveller. A Roman emperor mounted on his triumphant chariot was, in the midst of his glory, as ill at ease as the peasant in his cart. Except some improvements, such as the use of softer cushions, things went on thus till the invention of steel springs such as are now employed, for the leather braces of old-fashioned carriages still left much to desire.

Does this mean that the present mode of suspending carriages by four and even eight springs is the final step of progress? Certainly not. Our present springs diminish the force of jolts, transform a sudden shock into a long vibration; but the perfect spring ought always to maintain a constant elastic force, to allow wheels and axles all the vibrations which the ground demands of them, without allowing any of these shocks to reach the carriage itself. The search for this ideal spring has engaged the attention of one of our most eminent engineers. M. Marcel Deprez has found happy solutions to the problem of perfect suspension; he will, doubtless soon apply these in practice.

A good suspension also saves the carriage by suppressing the shocks which put it out of order and destroy it in a short time. Finally, suspension saves the wheel itself. On this subject let me recall a remarkable experiment of General Morin. On a high road, in good condition, he drove a diligence with four horses at the trot, and laden with ballast instead of passengers. The springs of the vehicle were raised so that the body rested on the axles. After the diligence had passed and repassed a certain number of times, it was found that the road on which it was running was notably deteriorated. The springs of the carriage were replaced and the same movements were repeated on another part of the road; the marked deterioration was no longer produced. It is thus clearly proved that a good suspension is favourable to a good condition of the roads.

But with non-suspended vehicles, in order thus to shock the passengers, disjoint the carriage, and abuse the road, force is necessary. It is the horse which must supply this; so that, independently of the useful work which we demand of them, the animal supplies still other work which gives rise to a multitude of shocks, and has only injurious effects. The employment of suspending springs has rendered the double service of suppressing injurious vibrations and of collecting into a useful form all the work which they represent.

Is this all? Do there not remain, even with the best carriages, other vibrations and other shocks which must be pursued and destroyed in order to render more perfect the conditions of traction? You have all experienced, at the moment of the sudden start of a carriage, and even at each stroke of the whip on a living horse, horizontal shocks which sometimes throw you to the bottom of the carriage. In a less degree, shocks of the same kind are produced at each instant of traction, for the speed of the horse is far from being uniform, and the traces are subjected to alternate tension and slackness. Here are veritable shocks which use up part of the work of the horse in giving only hurtful effects which bruise and contuse the breast of the animal, injuring his muscles, and, in spite of the padding of the collar, sometimes wounding him. To prove the disadvantages of this kind of shocks, some experiments are necessary. I have borrowed one from Poncelet; it is easily made, and any one may repeat it. I attach a weight of 5 kilos to the extremity of a small string; taking hold of the free extremity of this, if I gently raise the weight, you see that the cord resists the weight of 5 kilos and holds it suspended. But if I attempt to raise the same weight more rapidly, I bruise my fingers, the cord breaks, and the weight has not budged. The effort which I have made has been greater than the preceding, since it has exceeded the resistance of the cord; but the duration of this effort has been too short, and the inertia of the weight not being overcome, all my exertion has been expended in injurious work. If, instead of an inextensible cord, I had attached to the weight a cord a little extensible, the sudden effort of elevation which I made would have been transformed into an action more prolonged, and the weight would have been raised without breaking the cord and bruising my fingers. To render the phenomenon more easy of comprehension, I shall make a new experiment under conditions a little different.

You see on a vertical support (Fig. 1) a sort of balance-beam, which bears on one of its arms a weight of 100 grammes, on the other a weight of 10 grammes suspended at the end of a cord one metre long. Between these two unequal weights the beam is maintained by a spring-catch, which prevents it from falling to the side of the heavier weight, but which, on the other hand, permits the beam to incline in the opposite direction, if we bring to bear on the end of the cord an effort greater than the weight of 100 grammes. But, by letting the smaller weight fall from a sufficient height, at the moment when this reaches the end of its course, it will stretch the cord which holds it, and will develop what is called a *vis viva*, capable of raising the weight of 100 grammes to a certain height; but this elevation will only take place on condition that the application of this force does not give rise to a shock. If the cord which sustains the weight of 100 grammes is inextensible, and if that which bears the weight of 10 grammes is the same, at the moment of the fall of the latter, you will hear a snap; a shock agitates

the whole apparatus, but the weight of 100 grammes is not raised.

Now suspend this weight of 100 grammes to an india-rubber cord or an elastic spring, and repeat the experiment. You see each time that the weight falls that the 100-gramme weight is raised to a certain extent. But this elevation is effected under peculiar conditions. At the moment when the weight falls and the cord is stretched, the balance inclines, stretching the elastic spring, but the mass of 100 grammes does not yet move; it is only when this spring is stretched that the mass, obedient to the prolonged action of this elastic spring,

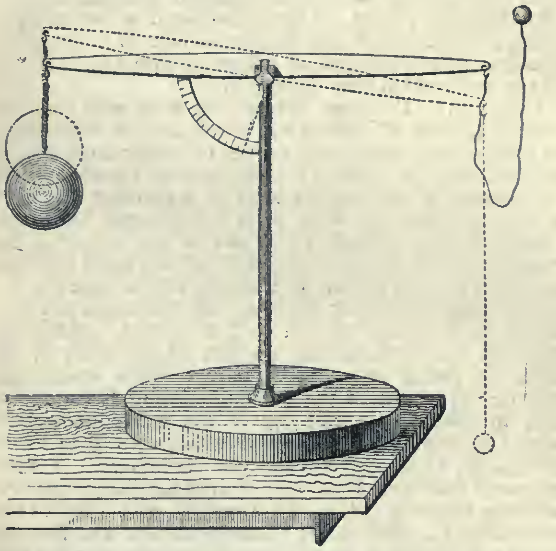


FIG. 1.—Apparatus to show that a *vis viva* directly applied to the displacement of a mass is lost in a shock, while the same force transmitted by an elastic medium may perform work.

begins to move and rises, representing a certain amount of work accomplished.

Thus, the suppression of shock in traction, economises a certain part of the moving labour; it is *then* advantageous to give to the traces of a carriage a certain elasticity. One of the most simple methods consists in interposing between the trace and the carriage an elastic medium. Here are some of these elastic pieces, which I call *tractors*. One of the patterns has been made by M. Tatin; it is composed of a spring which is compressed by traction and deadens the shock. The other is formed of a similar spring placed in the very inside of the carriage-trace.

If you wish to be convinced of the advantage of this mode of traction, yoke yourself to a hand-barrow by

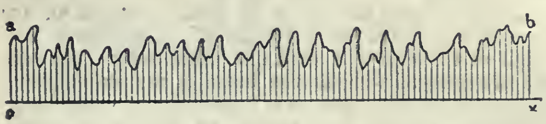


FIG. 2.—Tracing of the dynamograph for a vehicle drawn by a horse.

means of a rigid leather strap, such as you see used in the streets of Paris or London, where too often man is employed to drag burdens. When you have well noted the painful shocks which this mode of traction transmits to the shoulders, place between the strap and the barrow the elastic tractor and repeat the experiment. After that no doubt is possible; the shoulders are no longer bruised by the shaking of the pavement, and a comfort is experienced which will evidently be experienced in the same degree by a horse placed in conditions of elastic traction.

To obviate suffering to men and animals is unfortunately not a motive sufficient to induce everybody to modify the old system of harnessing. To certain minds known as *positive*, it is necessary to prove that elastic traction has economical advantages, and that a horse thus harnessed is able to draw heavier loads. This fact, which results from the experiments which you have seen, requires, to be rigorously proved, the aid of the graphic method. It is to the genius of Poncelet that we owe the record of work expended by different motors.

Everybody knows what a dynamometer is, viz., a spring which, yielding to tractions exerted upon it, is deformed in proportion to the efforts developed. Let us adapt to a spring of this kind a pencil which touches a strip of paper, and let us so arrange things that the movements of the wheel of a carriage shall impress upon the paper a motion of translation. While the effort of traction of the horse will communicate to the spring movements more or less extended, the progress of the carriage will

FIG. 3.—Tracing of the dynamograph for a vehicle drawn with an elastic intermediary.

draw out the paper, and from these combined movements will result a curve (Fig. 2), which can be resolved into a series of ordinates or vertical lines in juxtaposition, expressing by their unequal heights the series of efforts resulting from each element of the road traversed. The sum of these elementary efforts, otherwise the surface of paper limited in height by the flexures of the curve, will be the measure of the work expended. If we record in a comparative manner the work done by the same vehicle harnessed with rigid traces or supplied with elastic tractors, we see (Figs. 3 and 4) that the *area* of the curve is greater, that is, that there has been more work expended, while rigid traces have been used. In the most favourable cases that I have met with, the economy of work by elastic traction has been 26 per cent.

But, it may be objected, the recording dynamometer itself constitutes an elastic intermediary which suppresses

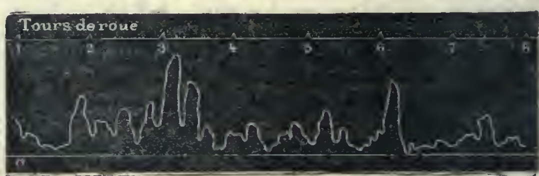


FIG. 4.—Tracing of the dynamograph for a hand-barrow drawn by a rigid trace.

the shocks. But it is not the ordinary dynamometer which I have used in my experiments, but a special dynamometer which undergoes under the strongest tractions only an almost insignificant elongation. This elongation, amplified by certain organs and transmitted to a distance by a lever fitted with a pen, is recorded in the form of a wavy curve in conditions referred to above. To sum up, in the employment of animated motors for the drawing of burdens, to find out wherever they produce shocks and vibrations, and to absorb them in elastic springs which restores to useful work a force that seemed only to destroy vehicles, tear up the roads, cause the animals to suffer—such is the direction in which much progress has been realised, and much more may still be realised.

2. *Of the Speed of Animated Motors.*—I shall per-

haps astonish many of you by saying that the speed of a vehicle is one of the things most imperfectly known. It is generally believed to be sufficiently expressed by stating how much way has been made and how much time has been occupied for that. I have come, you may say, from the Pont de Sèvres to the Madeleine in $41\frac{1}{4}$ minutes; the road is well mile-stoned, I possess a good watch; what greater precision do you require? Assuredly you have measured accurately the space traversed and the time employed, but that constitutes only the expression of a mean speed resulting from a series of variable speeds, of accelerations, of retardations, and sometimes of stoppages

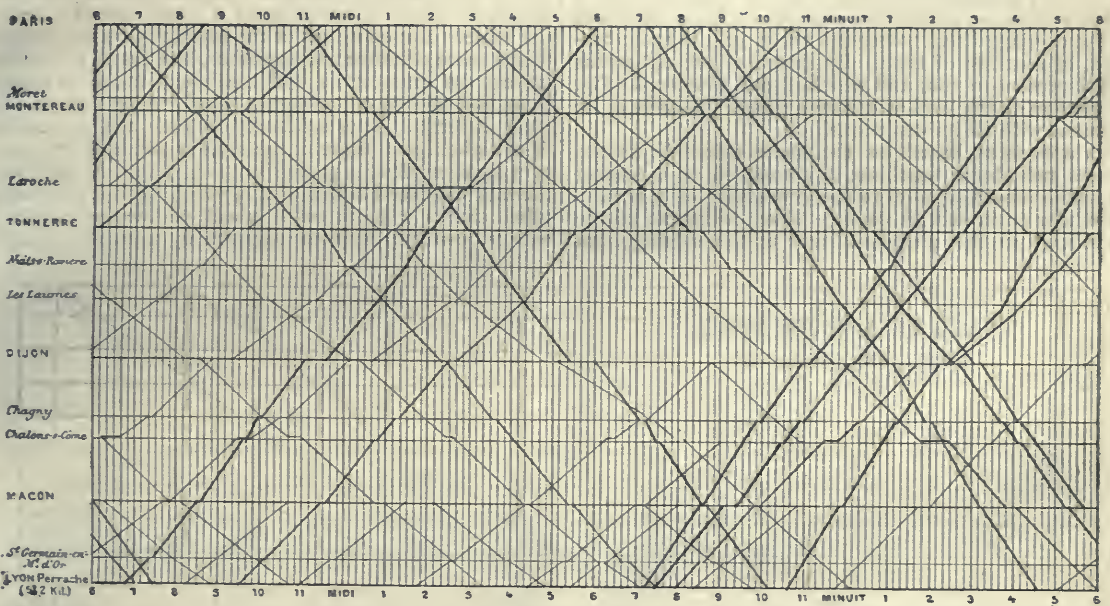


FIG. 5.—Graphic of the progress of trains upon a railway, after Ibry's method.—When we place the figure before us we read from the left, on the axis of the ordinates, the series of stations, that is, the divisions to be run over; the distance between the stations on the paper is proportional to the kilometric distances which separate them. In the horizontal direction, that is, on the axis of the abscissæ, are counted the divisions of time in hours, themselves subdivided into spaces of ten minutes each. The breadth of the table is such that the twenty-four hours of the day are represented on it, commencing at 6 A.M., and ending next day at the same hour. If we wish to express that a train is on a certain point of the line at a certain hour, we shall point out its position on the table, opposite the station or any point of the line which it occupies and on the properly chosen division of time. A single point of the table satisfies these conditions. At successive instants the train will occupy points on the table always different; the series of these points will give rise to a line which will be descending and oblique from left to right for trains coming from Paris, while it will be ascending and oblique in the same direction for trains going to Paris. The line which corresponds to each of the trains expresses the hours of departure and arrival, the relative and absolute rates of the trains, the instant of passing each of the stations, and the duration of stoppages. In fact, if we consider any particular train, we see that a train starts from the station at Paris at 11 A.M.; if we follow this train in its progress, we find that it has seven stoppages (during which it is not displaced in space, but only in time). These stoppages are translated by the horizontal direction of the line, opposite the station where they take place; the length of this horizontal line measures the duration of the stoppages. The line of the train, followed to the end, shows that the arrival takes place at 6 P.M.; but, if we reckon the distance on the axis of the ordinates, we see that 512 kilometres have been traversed in eleven hours ten minutes, stoppages included, which gives a mean rate of about 46 kilometres per hour.

where time is quite unknown. A rigorous measurement of rates supposes the road traversed by the vehicle at each instant; in other words, the position which it occupies upon the road. It is thus that physicists have determined the accelerated motion of the fall of bodies—Galileo and Atwood, by means of successive measurements, Poncelet and Morin by means of that admirable apparatus which traces by a single stroke the curve of a movement.

This machine is now too well known to need description; however, I shall make it work before you in order to interpret its language and to show how a graphic curve translates all the phases of a movement. The parabolic curve traced expresses for each of its points the position in which the body is found at each of the instants of its fall; it thus supplies the most complete information on the nature of the movement. But if, knowing only the space run over and the time employed, we join the two extreme points of departure and arrival by a straight line, that line, which will express the mean rate of the fall, will not correspond to any of the rates which the body has successively possessed.

The expression of movement by a curve has been put into practice. An engineer named Ibry has devised a method of representing graphically the progress of trains upon a railway. This mode of representation, incomparably more explicit than the tables of figures of our railway *indicators*, has not yet got into the hands of the public; and this is to be regretted, for it gives a genuine interest to a journey, as you may see by inspection of one of these graphics.

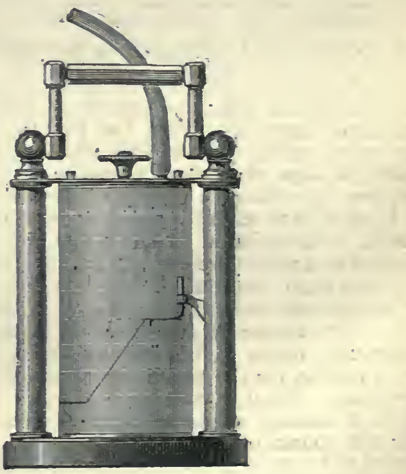


FIG. 6.—Odograph, reduced to one-third of its diameter.

The table which you see (Fig. 5) is prepared by engineers according to the regulation progress of trains

a progress supposed uniform; we see, in fact, that the lines of progress are all straight, joining to each other the two points which express the place and time of departure, the place and time of arrival. It does not then take into account the real movement of the train, which is accelerated or retarded under a great number of influences. The problem which we seek to solve, that of a graphic expression of the real rate of a vehicle, supposes that the carriage itself traces the curve of the roads traversed, in function of time. By means of the apparatus which I present to you, and which I call *Odograph* (Fig. 6), a waggon or any kind of carriage traces the curve of its movement with all its variations.

This apparatus, based on the same principle as the Poncelet and Morin machine, is composed of a tracing style which moves parallel to the generatrix of a revolving cylinder covered with paper. The movement of the style follows all the phases of that of the carriage, but on a very reduced scale, in order that the tracing of a distance of several myriametres may be contained in the dimensions of a sheet of paper. As to the movement of the cylinder, it is uniform, and commanded by clockwork placed in the interior. In order that the movement of the style may be proportional to that of the vehicle, things have been so arranged that each turn of the wheel causes the style to advance by a small quantity, always the same. But as a turn of the wheel always corresponds to the same distance accomplished, the faster the vehicle travels the more turns will the wheel make in a given time, and the more movements of progression will the style undergo. This solidarity between the movements of the wheel and those of the style is obtained by means of a small excentric placed on the vane. At each turn there is produced a puff of air, which, by a transmitting tube, causes a tooth of a wheel of the apparatus to escape, and the style to advance by a small quantity.

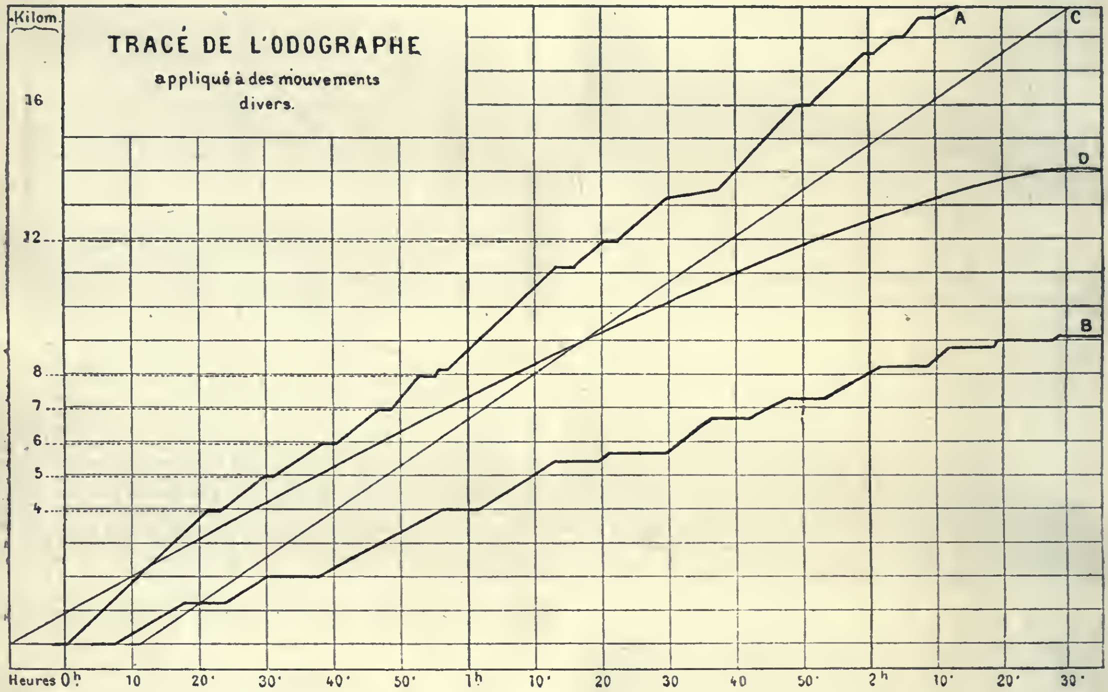


FIG. 7.—Tracings of the Odograph. A, rapid coach with stoppage; B, slow coach; C, gas meter, frequency of turns of the wheel; D, curve of the turns of a clock wheel-work with fly.

Similar effects may be obtained by means of electromagnetic apparatus. Thus the swifter the vehicle goes the more rapidly will the line traced ascend; the comparative slope of various elements of the tracing will express the variations of rate, as seen in Fig. 7.

If we wish to learn the absolute value of time and distance, it is sufficient to know that each minute corresponds to a millimetre counted horizontally on the paper, and that each kilometre corresponds to a certain number of millimetres traversed by the style in the vertical direction. The course of the style, which corresponds to a kilometre, ought to be experimentally determined for each vehicle, for the perimeter of wheels is not always the same. But it is clear that, if from each kilometrestone to another we obtain five millimetres, for example, for the course of the style, this length will always be found to be traversed each kilometre by the same vehicle. Our apparatus is then a measurer of distances, and dispenses with the necessity of attending to the existence of kilometre-stones; it enables the distance traversed on any road whatever to be estimated, and even when there is no beaten track. Thus in a journey of discovery we may measure the distance traversed by a cart. To remain in the conditions of ordinary life, have we not sometimes, in the country, a choice of two or three roads to go from one place to another? To know which is the shortest, we appeal to the watch, as if the least duration of a walk corresponded to the least distance. The odograph will give in this respect very precise information.

There is again a great number of questions which we ask daily without being able to solve them. Does such a draught horse go quicker than such another? Does this trot better to-day than yesterday? By increasing the ration of oats do we increase speed? Compare the slope of two curves of rates, and you will have the reply to all these questions without being obliged to make special experiments on a measured road, watch in hand.

It is not only to the speed of vehicles that the registering apparatus applies; it traces, though with less precision, the rate of progress of men and animals. We

slip into our boots a bellows sole, which is connected by a tube with a portable odograph. Each pace impresses on the style a small movement, as does each turn of the wheel of a carriage; and if the paces be absolutely equal, we may measure with certainty the distances travelled. In walking on level ground we take steps of astonishing regularity; but if the ground rises, the step gains in length; in descents, on the contrary, the steps are shortened. There may result from this slight errors in the distances traversed. Notwithstanding this, the employment of this apparatus will effect a great progress; it may be substituted with many advantages for the pedometer, which gives, at the end of a certain time, only the paces accomplished, without taking count of the stoppages or the changes of rate.

In short, when we make an experiment on a measured road, if there are produced variations in the length of the tracing represented by a kilometre, we conclude therefrom variations in the length of the pace. Such variations are observed under the influence of the slope of the country, the nature of the soil, the boots we wear, the rate of walking, or the weight carried. These studies in applied physiology have, I believe, a great practical importance, and numerous applications to the march of troops in a campaign.

(*To be continued.*)

WILLIAM KINGDON CLIFFORD

IT was with feelings of the deepest regret that we last week recorded the sad loss the scientific world and the country at large had sustained by the death, at the early age of thirty-three, of one of the deepest thinkers and most brilliant writers this century has seen. W. K. Clifford was the eldest son of the late Mr. William Clifford, J.P., of Exeter, and was born on May 4, 1845. Receiving his earlier education at the school of Mr. Templeton of that city, he proceeded to King's College, London. Here he at once gave evidence of his great powers by obtaining in his first year, 1861, the Junior Mathematical and Junior Classical Scholarships, as well as the Divinity prize. In the two succeeding years he gained the Classical and Mathematical Scholarships of the year, and in addition to the Inglis Scholarship for English language an extra prize for the English essay. Even at this time, whilst pursuing with such success so many branches of study, he sought a more genial occupation for his active mind in constantly reading in the college library the higher mathematical works to which he could obtain access, and towards the end of his school life, as also during his time as an undergraduate at Cambridge, he took great delight in solving and propounding problems in the *Educational Times*. While still in his eighteenth year the "Analogues of Pascal's Theorem" was written, and constitutes the first of his papers recorded in the Royal Society Catalogue. Passing from his school life, we find him entered at Trinity College, Cambridge, securing a Foundation Scholarship, college prizes in each academic year, and the English Declamation prize. Early in his career at the University he read such portions of the Tripos subjects as possessed any interest for him, and soon turned his attention to the study of the original writings of Sylvester, Cayley, Salmon, and some of the great Continental masters. In vain did his private tutor, the Rev. Percival Frost, who always had the highest admiration for him, and was anxious that he should attain his proper place in the Mathematical Tripos, urge him to devote a little more attention to examination subjects; his mind could tolerate no such restraint; nothing but the fresh and original thoughts of the greatest mathematical writers could satisfy his wants.

His neglect of the examination subjects was such that it is said he only once wrote out a paper of bookwork questions, and that under the impression that he was solving problems; many also, well qualified to judge, were agreeably surprised when he obtained the position of second Wrangler in the Mathematical Tripos of 1867, while his success in obtaining the second Smith's prize was doubtless anticipated from the wider scope for his talents afforded by that examination. During this period the course of his future work is clearly seen; divinity and classics, at one time so ardently studied, are laid aside, and the writings of the great philosophers divide his attention with the study of higher geometry. One of his longest and most fully worked out papers, "Analytical Metrics," published subsequently in the *Quarterly Journal of Mathematics*, was written at this time, 1864.

Of the circle of intimate friends Clifford formed at this time, nothing need here be said; two or three have gone before him, the remainder have watched with the deepest interest and pleasure his widening reputation and growing influence, and are now left with a blank no one can fill, and all bear in affectionate remembrance his ready sympathy, delicate sense of humour, and sweetness of disposition. His success in the Trinity Declamation prize, and his popularity in the debates at the Cambridge Union Society, showed him to be a speaker of no common order, but it was not until he delivered his first Friday evening lecture at the Royal Institution of London, the year after he took his degree, and subsequently at the Sunday Lecture Society, that crowded audiences bore testimony to his extraordinary power of lucid exposition. The Royal Institution lecture, "On some of the Conditions of Mental Development," delivered March 6, 1867, was the first time he addressed a large public audience, which included many of the leading thinkers of the time, and from that day he took a recognised position amongst them. A short extract from this lecture reflects the habit of his mind on leaving the University, and indicates plainly that the course of study pursued while an undergraduate, probably by many thought misguided, was, in reality, the expression of a deep inward conviction. Speaking of the mind, he says, "still less must it tremble before the conventionalism of one age, when its mission may be to form the whole life of the age succeeding. No amount of erudition, or technical skill, or critical power, can absolve the mind from the necessity of creating, if it would grow. . . . . The first condition of mental development, then, is that the attitude of the mind should be creative, rather than acquisitive;" and again, "It is quite possible for conventional rules of action, and conventional habits of thought to get such power that progress is impossible." Two other Friday evening lectures were given later by Clifford, on "Theories of the Physical Forces," Feb. 18, 1870; and on "Babbage's Calculating Machines," May 24, 1872; in the latter case no pains were spared by him to thoroughly master all the mechanical details of those intricate machines, and for years afterwards he would occasionally discuss schemes for the completion of the analytical engine. In public lecturing his greatest success was probably the evening lecture at the meeting of the British Association at Brighton, August, 1872, "On the Aims and Instruments of Scientific Thought." Throughout this lecture the key-note of so much of Clifford's most powerful writing can readily be detected, as may be shown by a short extract:—"If you will allow me to define a reasonable question as one which is asked in terms of ideas justified by previous experience, without itself contradicting that experience, then we may say as the result of our investigation, that to every reasonable question there is an intelligible answer, which either we or posterity may know. . . . By scientific thought we mean the application of past experience to new circumstances, by means of an observed order of events. . . . Remember then that it (scientific thought) is the guide of action; that the truth which it arrives at is not that which

we can ideally contemplate without error, but that which we may act upon without fear; and you cannot fail to see that scientific thought is not an accompaniment or condition of human progress, but human progress itself."

Several lectures delivered at St. George's Hall for the Sunday Lecture Society, such as those on "Ether," "Atoms," the "Sun's Place in the Universe," were all characterised by his extraordinary power of explaining some of the most difficult physical conceptions to a popular audience, and were invariably listened to with the greatest attention and pleasure. In his numerous contributions to the *Fortnightly*, *Contemporary*, and *Nineteenth Century Reviews*, his outspoken earnestness of purpose plainly showed the conscientious conviction of the writer, and riveted the attention of his readers even where they failed to convince. As many years secretary and afterwards a vice-president of the Mathematical and Physical Section of the British Association, he read many short original papers on mathematical subjects at the meetings, but it is to be regretted that no record of most of these remains. In the last few months of his active work, Clifford published the first part of a text-book he had long contemplated, entitled the "Elements of Dynamic." Bursting the bonds of the old method of treatment of the subject by the cumbrous rectangular coordinates, which had been so uncongenial to him in his college days, he draws the student lightly and softly into the toils of quaternions, thus almost imperceptibly introducing this simple and powerful method of analysis; sad it is indeed that his failing health did not allow him to complete this work which would have long remained as a valuable record of his method of teaching. Soon after taking his degree he was elected to a fellowship at Trinity College, and filled the post of assistant-tutor until his election to the chair of Applied Mathematics and Mechanics at University College, London, in August, 1871, which he held until his death. He was elected a Fellow of the Royal Society, June, 1874.

A critical examination of the value of his mathematical labours cannot be attempted; few, indeed, could do justice to them; but it is to be hoped that it may not long remain undone. All that can be said in concluding this brief sketch of his short and brilliant life is to give expression to the regret so widely felt that it was so prematurely cut off. He was—some of his friends may think unfortunately—most generally known for his philosophical and polemical writings. That his fame will rest on no such narrow basis, the following list of papers from the Royal Society Catalogue abundantly testifies:—

1. "Analogues of Pascal's Theorem" (January, 1863), *Quart. Journ. Math.*, vi. 1863, p. 216.
2. "Jacobians and Polar Opposites," *Messenger Math.*, ii. 1864, p. 229.
3. "Analytical Metrics," August 30, 1864, *Quart. Journ. Math.*, vii. 1866, p. 54; viii. 1867, pp. 16, 119.
4. "On the Principal Axes of a Rigid Body," *Messenger Math.*, iv. 1868, p. 78.
5. "On the Theory of Distance," Brit. Assoc. Report, 1869, p. 9.
6. "On the General Theory of Anharmonics," Lond. Math. Soc., ii. 1869, p. 3.
7. "On a Generalisation of the Theory of Polars" (1868), Lond. Math. Soc., ii. 1869, p. 116.
8. "On some of the Conditions of Mental Development" (1868), Roy. Inst., v. 1869, p. 311.
9. "On Syzygetic Relations among the Powers of Linear Quantics" (1869), Lond. Math. Soc., iii. 1869-71, p. 9.
10. "On a Case of Evaporation in the Order of a Resultant" (1870), Lond. Math. Soc., iii. p. 80.
11. "On a Cononical Form of Spherical Harmonics," Brit. Assoc. Report, 1871, p. 10.
12. "Synthetic Proof of Miquel's Theorem," *Messenger Math.*, v. 1871, p. 124.
13. "On a Theorem Relating to Polyhedra Analogous to Mr. Cotterill's Theorem on Plane Polygons" (1872), Lond. Math. Soc., iv. 1871, p. 178.
14. "Geometry on an Ellipsoid" (1872), Lond. Math. Soc., iv. 1871, p. 215.
15. "Preliminary Sketch of Biquaternions," Lond. Math. Soc., iv. 1873, p. 381.
16. "On Mr. Spottiswoode's Contact Problems," Roy. Soc. *Proc.*, xxi. 1873, p. 425.
17. "Graphic Representation of the Harmonic Components of a Periodic Motion" (1873), *Messenger Math.*, iii. 1874, p. 153.

DAVID PAGE

FEW names have been more familiar to general readers in geology than that of this practised writer. Born in Fife, his early years were spent in literary work of an unambitious kind. Among other occupations he edited for a time a newspaper in his native county. There used to be a story told of his having temporarily edited also the opposition paper during its editor's absence, and having carried on a most lively warfare in the rival pages. Whether well-founded or not, the story shows the estimation in which he was held as a facile writer. He afterwards entered into the employment of Messrs. W. and R. Chambers, and for some years took an active part in the preparation of their comprehensive series of educational publications. It was while in this capacity that he wrote his first introductory text-book of geology—a little volume which had a large sale and proved singularly useful in diffusing an elementary knowledge of the science. It was also during Page's connection with the Messrs. Chambers that the celebrated "Vestiges of the Natural History of the Creation" appeared anonymously. Looking back upon this now half-forgotten and superseded volume, it is hardly credible that it should have excited such keen feeling and passionate controversy. Having been the production of more pens than one, it was never formally owned by any one of its several authors. Robert Chambers was always credited with the lion's share of it, but there can be little doubt that he had powerful assistance from Page.

Quitting the service of Messrs. Chambers, Mr. Page embarked on a career of successful authorship. He re-wrote his "Introductory Text-Book of Geology," and prepared an Advanced Text-Book on the same science. He likewise published manuals on Physical Geography, and from time to time issued various popular works on geological subjects. These were always well written. He had little original power as an observer, though some of his work, particularly among the crustaceans and fishes of the Upper Silurian and Lower Old Red Sandstones, shows considerable acumen, and raises a surprise that he should not have done more in that department. His great merit—and it is one which professed students in science, immersed in their own original inquiries, are apt to overlook and undervalue—was that he had the power of seizing on the leading features of scientific progress and discovery, and presenting them clearly and vividly before non-scientific readers. He has done good service in widening the circle of sympathy with research, and for this chiefly he deserves to be gratefully remembered by geologists. On the establishment of the Newcastle College of Physical Science, in connection with Durham University, he was chosen to lecture on geology. He had already, however, had premonitions of the paralytic affection which has at last proved fatal. He was eventually relieved of the duties of practically instructing his students in the field, this part of the work of the college being undertaken by Mr. Lebour. His failing health has for several years prevented him from appearing at the meetings of the British Association and elsewhere, as was his wont. He has at last been removed from among us in his sixty-fifth year.

NOTES

FIFTY-THREE candidates for the Fellowship are "up" at the Royal Society.

THE sum proposed in the Civil Service Estimates to be spent on the main fabric of the new Natural History Museum at South Kensington during the next financial year is 47,476*l.*, being the balance of a sum of 409,466*l.*, the "revise estimate" of the total cost of the erection of the building. It is also proposed to spend 20,000*l.* on "internal fittings." From the reply lately given in the House of Commons to Lord Arthur Russell, it seems that the Botanical and Mineralogical Departments will be transferred into the new building before the close of the year.

AT a meeting on March 7 at Cambridge of the general committee of the Darwin Memorial Fund, to decide whether the memorial should assume the form of a bust or a picture, and to select either the sculptor or painter willing to execute such memorial, it was stated that the funds promised amounted to over 400*l.*, which sum would be sufficient to procure either a bust or a picture. After some discussion it was resolved by a large majority that the memorial should take the form of a picture, and Mr. W. M. Richmond was selected as the artist to execute the same.

WE are informed that the committee appointed to receive subscriptions for presenting a bust of Mr. Wm. Spottiswoode, P.R.S., to the Royal Institution, as a testimonial of his valuable services as its treasurer and secretary successively, have engaged Mr. Richard Belt as the sculptor.

SINCE Parliament reassembled the finest example of practical telegraphy that has probably ever been witnessed has been going on between London and Ireland. Two news wires have been worked simultaneously and continuously between London, Dublin, Cork, Belfast, and Londonderry at the unprecedented speed of 130 words per minute. The Post Office authorities have recently been making very great improvements in their fast speed apparatus. The forthcoming International Conference and the success of the American quadruplex system are probably stirring them up to maintain their pre-eminence in this field. England cannot run second to any nation in telegraphy.

WE are greatly disappointed and much surprised to learn that the application of the Scottish Meteorological Society for assistance to establish an observatory on Ben Nevis has been rejected both by the Meteorological Council, with its yearly 15,000*l.*, and the Government Grant Committee, with its 5,000*l.* The former had other matters to attend to, the latter handed the application over to the Council of the Royal Society, labelled "highly commendable." We have recently shown our readers with what heartiness and cosmopolitanism such nationally beneficial undertakings are managed in France; yet in this disgracefully wealthy country, with 15,000*l.* a year expressly devoted to meteorology, a plan of promoting meteorological research that would lead to results of the highest consequence must collapse for want of a paltry 500*l.* to start it. It would be a shame if so really national an enterprise were to depend entirely on private subscription.

MR. MONCURE CONWAY proposes to hold a "Memorial Service" on the late Prof. W. K. Clifford at South Place Chapel, Finsbury, on Sunday morning next. After the service, Mr. Conway will deliver a discourse, taking Prof. Clifford for his subject.

IT is stated that the Botanical Exchange Club will have to be dissolved after the next distribution, in consequence of the difficulty of finding any one with the requisite critical knowledge of British plants and the leisure to enable him to perform the duties of curator. The club has been of great service in furnishing a medium of intercommunication between British botanists; and the annual reports of the curator have frequently been essays of considerable value. It is greatly to be lamented that one result of the increasing attention paid by botanists to the physiological side of the science should be the discontinuance of so useful an institution.

AT the annual meeting of the Geological Society, the Wollaston gold medal was awarded to Prof. Bernard Studer, "the father of Swiss geology;" the Murchison medal to Prof M'Coy, of Melbourne; the Lyell medal to Prof. E. Hébert, of Paris: the Bigsly medal to Prof. E. D. Cope, of Philadelphia; the balance of the Wollaston Donation Fund to Mr. Samuel Allport; the proceeds of the Murchison Geological Fund to Mr. J. W. Kirkby; a moiety of the balance of the proceeds of the Lyell Fund to Prof. Alleyne Nicholson, and the other moiety to Dr. Henry Woodward, F.R.S.

IT is stated that Capt. Sir George Nares, K.C.B., has been appointed to be the chief of the Harbour and Marine Department of the Board of Trade, in succession to Rear-Admiral Bedford. Sir G. Nares is now in command of Her Majesty's ship *Alert*, which is on a scientific cruise in the Straits of Magellan. Capt. J. F. L. P. Maclear has been appointed to the *Alert*.

A COMMITTEE has been organised to obtain subscriptions for erecting a statue to Nicephore Niepce, the inventor of photography. A circular has been published and will be sent to all scientific societies over the world.

THE movement for the propagation of electric lighting in Paris has not abated. A new paper entitled *La Lumière électrique* will be started in a few days under the editorship of M. de Parville. M. Regnier, the inventor of a lamp working by contact will begin experiments at Breguet's workshop. M. Ducretet is busy with his new lamp with a floating positive-carbon in mercury, and the Alliance Company will try Werdermann's on a large scale. The gas company and Jablochkoff are preparing to illuminate the spaces which have been allotted to them. Although the scientific question may be considered settled except under improved electrical conditions the Paris electricians are sanguine that the final verdict will not be given against the electric light.

THE experiments with the electric light recently made in the reading-room of the British Museum have satisfied the trustees of its applicability for the purposes of the room as far as the amount and distribution of light are concerned, although the full number of lamps was not employed. On three occasions the light was turned on at dusk, in order to enable readers to continue their studies without interruption for another hour. As far as could be ascertained they were enabled to work by it without difficulty, even at the tables where the light was weakest. The experiments are discontinued for the present, but a further trial of the light will probably be made some months hence, with the view to utilise it on dark days, and for extending the hours for using the reading-room in the winter.

THE Report by the Regius Keeper of the Edinburgh Botanic Garden for 1878 complains of the want of accommodation in various ways, and of the insufficiency of the present grant. These complaints are not now made for the first time, and we trust they will meet with speedy attention in the proper quarter.

RECENT explorations of the lake-dwellings of the Lake of Geneva prove that they were destroyed by fire during a spring, when the waters of the lake stood at the same level as now. A layer of charcoal from the burnt dwellings is to be found along the whole coast, beneath a layer of sand and gravel.

REMAINS of lake dwellings of the highest scientific interest have been discovered by members of the Donaueschingen Historical Society under the guidance of Dr. B. Spuren. The

dwellings are situated in the so-called Pfohrener Ried, near Donaueschingen. Numerous objects have been brought to light, such as remains of textures, and implements dating from the stone, bronze, and iron ages.

IN the present year the eighteen centuries will be complete, which have elapsed since Pompeii, Herculaneum, and some neighbouring cities were destroyed by a rain of ashes and torrents of lava from Mount Vesuvius. The directors of the excavations at Pompeii intend to commemorate the event in a scientific manner in November next, and have issued invitations to the most eminent Italian archæologists to participate in the celebration.

DURING the night of January 7-8, an earthquake was felt at Alaghir, Caucasus; it consisted of five shocks which had a direction from north to south. An earthquake visited Laibach on February 12. Two severe shocks were felt at 2.42 P.M. within about three or four seconds. The phenomenon was observed in the whole of Carniola and Lower Styria, also in some part of Carinthia and at Trieste. The direction of the undulations was from south to north, and they were preceded by loud subterranean noise; their duration was about five seconds. It is remarkable that this earthquake was observed in Southern Austria on the same day, as one of the Teplitz sources ceased to flow. On February 14, at 2.45 P.M., more shocks were observed at Laibach and Krainburg, but they were extremely weak. Subterranean shocks were also felt at Riva on the Lake of Garda on February 14, as well as at Bischofslaak in Carniola on February 16.

A REMARKABLE phenomenon is reported from Neufchatel. On February 10 the Lake of Neufchatel suddenly assumed a motion like the sea with its tides, only with the difference that the rise and fall of the water succeeded each other in much shorter intervals. The phenomenon began at noon and lasted until 2 P.M. Boys who were playing on the shore were so suddenly surprised by the rising lake that they were up to their knees in water before they had time to escape. In the evening there was a violent thunderstorm, which also visited Berne at the same time.

ROMAN antiquities have been recently discovered in the open space in front of the Votive Church at Vienna. The space is in course of transformation into a public garden. Among the objects found are some rare coins, toga clasps, urns, as well as the remains of tombs and of a bath.

IN the basin of the Teplitz Stadtbad, the recent stoppage of which has caused so much alarm in the charming Austrian watering-place, Roman coins and antiquities have been found. On one of the coins a female bust is represented with the circumscription "Sabina Augusta" (wife of the Emperor Hadrian, A.D. 117-138). The source must therefore have been well known to the Romans, and it is quite possible that even before the year 762, when it is first mentioned in Bohemian history, it may have temporarily ceased to flow. Besides the Roman coins, Bohemian and German coins (up to the year 1740) were discovered.

THE *China Overland Trade Report* mentions that a scheme is about to be carried out for establishing a woollen manufactory at Lanchow-fu, in North-Western China. Though Kansuh, the province of which it is the capital, is not populous, the locality has been chosen on account of raw material being plentiful in the neighbourhood. Machinery is said to have been already shipped from Europe, and two German gentlemen have been engaged to superintend the preliminary operations and to start the enterprise.

AT the last meeting of the St. Petersburg Society of Russian Naturalists, M. Grimm made a very interesting communication on the crustaceans of the Caspian Sea. The crustacean fauna of that sea has some likeness with the faunas of Lakes Baikal and Titikaki, especially as to the richness of both in amphipods, and as to the nearly total want of decapods. But the likeness is closer with the faunas of the European lakes, as well as with the faunas of the Black Sea, and yet more, with the Arctic Ocean and Lake Aral. Altogether, the study of the Caspian crustaceans proves that at a recent epoch the Caspian Sea was in connection with these lakes and seas, and that the connection of the Caspian with the Arctic Ocean and Lake Aral continued until a more recent period than the connection with the Black Sea.

AT the same meeting Prof. Bogdanoff proposed to the Society to undertake the publication of a work comprising all trustworthy data about the ornithology of the northern parts of Russia in Europe. The proposal met with great approval, and a commission consisting of MM. Bogdanoff, Polyakoff, Pleske, and Keppen, was appointed for the preparation of the said work.

AT the last meeting of the Paris Geographical Society, a communication was read by M. Sconzac, a French officer, belonging to the Chinese service on the origin and propagation of the Russian plague. The lecturer contended that this epidemic originated in the province of Yunan, and was carried by travellers *viâ* Mesopotamia. The lecture will be published *in extenso* in the Society's *Bulletin*.

A VERY satisfactory report was presented at the recent annual meeting of the Royal Microscopical Society, which has now been in existence forty years. The members number 437, and the funds are in a flourishing state.

VOL. VII., part 1, of the *Proceedings* of the Yorkshire Geological and Polytechnic Society contains a number of important papers on local geology, and one or two of more general interest.

IN the just issued number of the *Proceedings* of the Geologists' Association is the continuation of Mr. W. H. Huddleston's valuable paper on the Yorkshire oolites, while Prof. Bonney contributes some interesting observations on the igneous rocks of Arthur's Seat.

M. FERRY, the new French Minister of Public Instruction, has visited the Sorbonne, the School of Medicine, and other buildings devoted to science, for the purpose of deciding what repairs must be done immediately.

THE Swiss Palæontological Society has just published the fifth volume of its *Abhandlungen*, which contains several valuable papers. Prof. Rütimeyer gives the conclusion of his most interesting researches into the deer of the tertiary period. Prof. P. de Loriol continues his researches into the Swiss fossil crinoids, and gives the conclusion of a very valuable monograph on the fossils of the Baden formation, a subdivision of the recent Jura formation. Dr. Wiedenheim publishes for the first time a complete description of the *Labyrintodon rütimeyeri*, discovered in 1864 in the sandstone of Rieben, at Basel, and M. J. Bachmann describes the fossil eggs from the neighbourhood of Lucerne. Twenty-five fine coloured plates illustrate the papers. The Society was founded in 1874, with statutes and aims much like those of the British Palæontological Society, especially for the study of Swiss palæontology; and the five volumes already published by the Society contain a great diversity of very valuable papers by Professors Rütimeyer, in Basel, De Loriol, in Geneva, and Renevier, in Lausanne.

THE Swiss meteorological stations have adopted a system of weather warnings. All the territory of Switzerland will be divided for that purpose into eight regions, each of which will have its central station. The information on the state of weather

in Europe, received at these stations, will be graphically represented on maps, and these maps will be exhibited between four and five P.M. at a central point of the town, under glass, for public use. Besides there will be given a prognostic of weather for the following day; and this information will be sent to each commune and person who will pay monthly the sum of 15 francs. This system is already introduced in Zurich and Berne, and the prognostics are correct in eight cases out of ten.

WE have received from Mr. Downing, of Whiskin Street, a neat little cabinet of twenty specimens of rocks, fossils, and minerals to illustrate Geikie's *Geology Primer*. Considering the number and quality of the specimens, and their suitability for the purpose, the cabinet is a wonder of cheapness, and ought to have a wide sale.

IN view of the apparently insurmountable difficulties which attend the completion of the New National Opera House on the Victoria Embankment, the *Globe* understands that all the agents-general for the different colonies have entered into negotiations for the purpose of securing this site for the proposed Colonial Museum.

INTERESTING correspondence has appeared in the British Guiana *Royal Gazette*, we learn from the *Colonies and India*, relative to the qualities assigned to the fruit of the papau-tree. It has been recently asserted, in an article in the *Pharmaceutical Journal*, "that the most interesting property attributed to it is the power of its juice to render bad flesh tender." Mr. Monro, of Georgetown, furnishes certain facts which he says are commonly known to the natives of British Guiana relative to this fruit. A horse tied near one of these trees rapidly loses health, and a stud horse becomes useless. Any pressure on the body of the animal leaves an inelastic indentation. The sap of the tree will soften steel, and before the process of tempering was known in the Colony, the blacksmiths used to drive their brittle chisels and plane vices into the wood, leaving them there for a day or two; and tough meat wrapped in the leaf for only a few minutes becomes tender, and the same thing happens if it be suspended against the tree itself. The seed of the ripe fruit is an excellent vermifuge, and children have a great partiality for it.

IN a recent paper in the *Journal de Physique* complementary of the theory of dew, M. Jamin points out that moist surfaces are subject to two superposed causes of cooling, one radiation (like dry substances), the other evaporation. The difference between the two actions is that the former persists at every temperature, while the latter, at first considerable, decreases and becomes *nil* when saturation is reached (it does not produce dew, but contributes to prepare and accelerate it, for it renders the air both moister and colder). The quantity of heat borrowed from the air by evaporation is very considerable: 1 gramme of evaporated water lowers about one degree the temperature of 2,553 grammes of air (or nearly two square metres' volume). Thus is explained how moist bodies, like plants, especially herbaceous, are cooled much more quickly than dry bodies. The dew forms on them more quickly; once it has commenced it continues by the sole effect of radiation. In driving rapidly down from a plateau into a valley one is often struck with the sudden cold. This cold is probably the effect of more rapid evaporation from the herbs, aquatic plants, and all moist surfaces of the valley. Other facts illustrate the double effect of radiation and evaporation, *e.g.*, the danger of plants in early spring after being moistened by a shower, and the well-known mode of manufacture of ice in Bengal. The *rôle* of dew is that of moderating and sometimes arresting the nocturnal cooling, and preserving plants from the early frost.

AN interesting lecture on certain enigmatical phenomena of astronomy has been recently delivered by M. Houzeau before the Belgian Academy of Sciences, of which he is president. The points he takes up are, the apparent enlargement of heavenly bodies near the horizon (not adequately explained by a weakening of the rays, or interposition of terrestrial objects); the supposed satellite of Venus, observed seven times in 119 years, by eminent astronomers, but quite unobserved during the 114 years since; the phenomena connected with Biela's comet; the effects of the earth encountering a comet (may such a thing occur? has it occurred?); and the zodiacal light. The lecture appears in the Academy's *Bulletin* (No. 12 of 1878).

WE have on our table the following books:—"Text-Book of the Steam Engine," T. M. Goodeve (Crosby, Lockwood, and Co.); "The Aborigines of Victoria," 2 vols., R. Brough Smith (Trübner); "Report of the British Association for the Advancement of Science, 1878," Dublin (Murray); "The Circle and Straight Line," John Harris (Wertheimer, Lea, and Co.); "Moore's Columbarium," reprinted by W. B. Tegetmeier (*Field* Office); "Practical Treatise on the Manufacture of Sulphuric Acid," A. G. and C. G. Lock (Sampson Low and Co.); "Atlas of Histology," Part i., E. Klein and E. N. Smith (Smith, Elder, and Co.); "Geologische Uebersichtskarte des Tyrolisch-Venetianischen Hochlandes zwischen Etsch und Piave," 6 Maps, Dr. Edmund Mojsisovics, (Vienna: A. Holden); "Die Dolomit-Riffe, von Sudtirol und Venetien," Heft i. to vi., E. V. Mojsisovics (Vienna: Alf. Holden); "Meteorological Observations made at the Adelaide Observatory, years 1876 and 1877," Ch. Todd; "Journey through Khorassan," 2 vols, Col. C. M. M'Gregor (Allen and Co.); "Lakes and Mountains of Africa," J. F. Elton (Murray); "On the Annelida Chætopoda of the Virginian Coast," H. E. Webster; "Fécondation des Fleurs," E. and G. Gevaert (G. Mayolez).

THE additions to the Zoological Society's Gardens during the past week inclnde three Japanese Deer (*Cervus sika*) from Japan, presented by the Viscount Powerscourt, F.Z.S.; a Syrian Bear (*Ursus syriacus*) from West Asia, presented by Dr. J. Huntley; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Miss E. A. B. Payton; a Pig-tailed Monkey (*Macacus nemestrinus*) from Java, presented by Mrs. J. E. Fenton; a Coati (*Nasua nasica*), an Acouchy (*Dasyprocta acouchy*) from British Guiana, two Mountain Finches (*Fringilla montifringilla*), British Isles, purchased.

SCIENCE IN RUSSIA

WE take from the just issued Annual Report of the St. Petersburg Academy of Sciences for 1878, the following information as to the work done by the Academy during the year in the mathematical and physical sciences.

Prof. Chebysheff has continued his researches into the properties of parallelograms which consist of three elements, and are symmetrical with respect to one axis, these researches already having led him to important results; among them we notice his general formula for determining what are the simplest combined systems which, when set in motion, give a straight line.

Prof. Minding has published two papers, one of which is an important addition to his former researches on curves.

The Pulkova Observatory has published the ninth volume of its Memoirs, which contains the micrometrical measurements of double stars by Prof. O. Struve, during forty consecutive years. The value of these very numerous and precise measurements is much increased by the circumstance that they were made during so long a period by the same person, with the same instruments, and on the same methods; the comparison of M. Struve's observations with those of several known astronomers affords a means of reducing all of them to one system.

The transit of Mercury was observed at Pulkova with fourteen telescopes. It is worthy of notice that the observations proved that Mercury does not have such a dense atmosphere as that discovered around Venus.

The interesting researches of the late Prof. Asten into the

motion of the comet of Encke were already well prized by all men of science. The *Memoirs* of the Academy contain this year a new work by Prof. Asten, on all the appearances of that comet from 1819 to 1875. M. Asten has published also new ephemerides of the comet for this year, and their precision was already proved by the numerous observations made in the southern hemisphere. Unhappily he could not see this brilliant confirmation of his researches, death having taken him from the ranks of men of science.

Prof. Bredikhine was engaged in very interesting researches into the tails of comets, and he established that there are three quite different kinds of tails, according to the different relations between the attractive and the dispersive force of the sun, which last is modified by the different properties of the particles of which the tail is formed. Spectral analysis will probably confirm this hypothesis.

As to spectral analysis in its application to heavenly bodies, the report states that it is not based, as yet, on sufficiently positive data, and that to elaborate these data with the accuracy usual to other astronomical observations, is the special aim of the astro-physical department of the Pulkova Observatory. Thus M. Hasselberg has published two works, one of which reduces the observations of Kirchhoff to wave-lengths, and the other deals with a precise description of the spectra of absorption of NO_2, and bromine. The same author has recently proved that the displacement of spectral lines does not depend upon the density of the gas.

Observations on the variations of luminosity of stars were pursued by M. Lindemann, who has added 42 other stars to the 280 variable stars, included in the catalogue of M. Sjölerup. Prof. Savitch has made observations on oppositions of planets and on the transit of Mercury. He has also read at the Academy a paper on corrections on the pendulum of Repsold. Finally, we notice the observations of M. Njuren on earthquakes, as noticed by vibration of the sensitive levels of astronomical instruments.

In physical science the report begins with the work of the Central Physical Observatory at St. Petersburg. The Director of the Observatory, Prof. Wild, has a memoir on the determination of the real temperature of the air by a thermometer, and on the precautions to be taken for avoiding various secondary influences on the instrument, especially those of radiation. Thermometers which allow the determination of the temperature of the air with a precision of one-tenth of degree, have already been introduced into Russian meteorological stations. Besides, Prof. Wild has proposed very satisfactory improvements in the siphon barometer which allow easily the transportation of the barometer dismounted, without changing its error, and thus afford a means for comparing with great accuracy the normal barometers of different stations. After having given, during the preceding years, special attention to terrestrial magnetism, the Central Physical Observatory has occupied itself with atmospheric pressure, and Capt. Rykatcheff has published a paper on the diurnal changes of barometrical pressure, based on the observations of eleven Russian and twenty-four foreign stations. The theory for explaining the diurnal changes of pressure, proposed by M. Rykatcheff, seems to be more probable than that of Dove, usually accepted until now. Among other meteorological papers, published by the Academy, there are worthy of notice those by Prof. Lenz, on the Galvanic Resistance of Solutions of Salts of Kalium, Natrium, and Ammonium; by Capt. Rykatcheff, on Winds on the Baltic; by M. Dorandt, on Anemometers; by M. Hellmann, on the Comparison of the Normal Barometers at St. Petersburg, Helsingfors, Dorpat, Stockholm, and Upsala; and by M. Stelling, on the Intensity of Day-Light at St. Petersburg.

In chemistry Prof. Menshutkin has continued his researches on the Influence of the Isomerism of Alcohols and Acids on the Formation of Compound Ethers; and the Report notices among the chemical papers which have appeared in the publications of the Academy, those by Mrs. Lermantoff, on the Influence of the Tertiary Sodic Butyl on Isobutylene, and by M. Paoloff and Jawein, on Organic Chemistry.

In geology the year 1878 is marked by the appearance of the most important work, by Prof. Frederick Schmidt, on Silurian Trilobites of the Eastern Coasts of the Baltic, being the result of twenty years' researches, and of a thorough comparison of Russian trilobites with those of other countries. The work will contain the description of about 140 species; the first fascicule, actually in the press, contains a general description of the silurian rocks in the governments of St. Petersburg and Esthonia. Prof. Meller has described the collections brought from the Manyeh by M. Danilcosky, and has proved the former extension of the Caspian very near to the Sea of Azov.

In botany the first place is occupied by the work on the Algæ of the White Sea, by M. Gobi. Prof. Famintzin has continued his researches into the embryology of plants, and by means of preparations made on the method of Nestermeyer, he has arrived at important results as to the formation of tissues in the embryons of plants. M. Klinge has made very interesting histological researches on roots of several Monocotyledons.

In zoology Prof. Brandt has published important additions to his former work (appeared in 1877) on the Rhinoceroses, living and extinct. But the most interesting of his works is certainly that on the Elasmotherium. Judging by the few teeth, found until now, of this extinct mammal, which formerly inhabited Europe from the Rhine to the Kirghiz Steppes, Prof. Brandt had described the elasmotherium as a form of the family of rhinoceros, but very near akin to the horse. A complete skull of this animal, which was recently found at Sarepta, on the Volga has quite confirmed the opinions of this zoologist. The elasmotherium is most nearly related to several kinds of rhinoceros, having at the same time several features common with the horse, and forms a separate sub-family of rhinoceros; he lived at the same time as the bison, the rhinoceros, tichorhinus and man. Prof. Kessler gives interesting descriptions of new fishes from Central Asia. M. Mereshkofsky has explored the sponges of the White Sea, and M. Boetger is publishing an important monograph on the *Clausiliæ*. Finally, the Report mentions the works of Prof. Ovsiannikoff on the anatomical structure of the cerebrum of the dolphin and other vertebrata, and the papers by Prof. Gruber, on variations of the muscles and the bones of man.

In the report of the historico-philological section of the Academy we mention the works of Prof. Dorn on inscriptions in the Pelegvian language and on the Semnan language, a now nearly extinct branch of the Iran branch; and memoirs, by Prof. Schiffner, on Buddhism; and by Prof. Wideman, on Finnish languages, namely, of that of Zyryans.

As to the premiums awarded by the Academy, those bearing Lomonosoff's name, for works in chemistry, physics, and mineralogy, and Buniakofsky's, for works in mathematics, were not awarded this year. A new premium, bearing the name of Helmersen, was founded this year for works in geology.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

PROF. JAMES STUART'S efforts to develop a school of Applied Science at Cambridge, have produced further fruit in the shape of a report of the Board of Mathematical Studies, recommending an alteration in the scheme of examination in mechanism and applied science for the ordinary B.A. degree. It is proposed now that papers on mechanics and on heat shall be obligatory on all candidates, while they shall choose one only of three other subjects, viz., (1) Mechanism. (2) Theory of Structure, Strength of Materials, and Principles of Surveying and Levelling. (3) Electricity and Magnetism. The examination is to be made as far as possible a satisfactory basis for a professional career by its practical character. Thus every candidate will be required to show his ability to write an accurate description or specification of an instrument, machine, or model exhibited, and to make a working sketch to scale. Evidence of actual capacity to use the tools or instruments belonging to the subject selected by the candidate will be demanded. The first class in the class list is to consist only of those who have distinguished themselves in one of the three alternative subjects. We do not doubt that it will soon be realized that a student who has to pass an examination in Greek and Latin in his second year will not have time to master his Science subjects by the end of the third. There will be much more chance of attracting students of Applied Science to Cambridge if they are excused from the classical portions of the "General" Examination for the ordinary degree.

IN the new scheme for the Classical Tripos at Cambridge, the advanced portion will include several groups of subjects, of which one only is to be selected by a candidate. Three of these groups are History, Archæology, and Language, and the knowledge and treatment required will be thoroughly scientific in character. Archæology will include papers on Greek and Roman

mythology and religion, on topography and monuments, and on art and handicraft and the inscriptions of the Greeks and Romans, in relation to their domestic and national life. The Language group will include elementary Sanskrit, the comparative grammar of the Indo-European languages, and the history of alphabets.

FORTUNATELY the demands of science are sometimes so urgent, that they cannot be resisted, as in the case of the School of human anatomy at Cambridge, which has so outgrown its accommodation that a new dissecting room must be immediately provided, pending the expected erection of really adequate buildings for the medical schools some years hence. There are sixty students dissecting this winter, under Dr. Creighton, and several assistant demonstrators. In the interests of health and education, a large room is to be built over the present lower rooms at a cost of 600*l*., which will supply the pressing need of space.

IT is satisfactory to learn that physical science will be represented among the three representatives of Trinity College sitting with the Cambridge University Commissioners, by Mr. Trotter, who has taken a very influential part in framing the new statutes of his college, and that Prof. Bonney is one of the representatives of St. John's College. We wish that we might hear that other colleges sent equally accredited men of science to strengthen the cause of physical science before the commissioners.

A VACANCY having occurred in the professorship of botany at the University of Innsbruck by the transference of Kerner to the important chair at Vienna as successor to Fenzl, Dr. Peyritsch, director of the botanic garden at Vienna, has been appointed to the professorship at Innsbruck.

THE Zurich University has just granted the diploma of Doctor of Philosophy to Miss Helene Druschkovitsch, from Vienna, after a brilliant defence of her dissertation on the "Don Juan" of Byron.

THE Geneva University numbers 391 students, of whom 208 are not matriculated. Out of the 391 students 212 are Swiss, and the others are strangers; there are forty-five ladies, of whom five, Russian, are matriculated.

SCIENTIFIC SERIALS

Zeitschrift für wissenschaftliche Zoologie, vol. xxxii., part 1. —J. Brock on the sexual organs of cephalopoda, first memoir, dealing with sepia, loligo, sepiola, eledone, pp. 116, 4 plates. F. E. Schultze, researches on sponges, sixth part, the genus spongelia, 41 pp., 4 plates.—L. Löwe, on the anatomy of the gills of serpula, 30 pp., 1 plate.

Morphologisches Jahrbuch, vol. iv. part 4.—Contribution to the anatomy and histology of the sexual organs of osseous fishes, by J. Brock, Erlangen, 68 pages, describing very many forms, 2 plates, with figures from eleven genera.—The folds of the mucous membrane in the human palate, by C. Gegenbaur, with 1 plate, giving a comparison between the palatal markings of human fœtuses and those of the orang, cercopithecus, and ateles. —On the female sexual apparatus of *Echinorhynchus gigas*, by A. Andres, 1 plate.—The extensor muscles of the calf and foot of mammals, by G. Ruge, a very extensive inquiry, 52 pages, 4 plates, followed by another from the same author on the deep muscles of the sole of the foot, with 2 plates.—On *Labyrinthodon Rütimeyri*, by R. Wiedersheim.—On the homology of segmental organs in annelids and vertebrates, by Max Fürbringer.

Kosmos, November.—On Bacon of Verulam, the founder of modern realism, by Fritz Schultze.—The origin and evolution of sensory organs (eye and ear), by Ernst Haeckel.—Colour in animals and plants, by A. R. Wallace, translated from the English.—On the political constitution in primitive grades of culture, by M. Kulischer.—Report of the meeting of the German Anthropological Society.

December.—On the discovery of the soul, by G. Jäger.—Colour in animals and plants, by A. R. Wallace, concluded.—The elasmotherium of the diluvium.

January.—Philosophic reflections on the nebular hypothesis, by Carl du Prel.—The mathematical basis of the structure of the plant body, by Dr. S. Gunther.—Dissimilarity between the male and female of *Epicalia acontius* by Fritz Müller.—Primitive constitution; part 2, federation, by M. Kulischer.

THE *Bulletin de l'Académie impériale des Sciences de St. Pétersbourg* (t. xxx. No. 3) contains the following papers of interest:—On an application of the finite differential calculus, by F. Minding.—On some extraordinary muscles, tensores fasciæ suralis, in man, by Dr. W. Gruber.—On a new species of ossiculum supernumerarium carpi in man, by the same.—Palæontological observations regarding M. Danilewsky's journey to the Manytsch, by V. Möller.—Supplementary observations to a monograph of the *Rhinoceros tichorhinus*, by J. F. Brandt.—New researches on the ichthyology of Central Asia, by K. Kessler.—On the tail of comets, by M. Bredikhine.

THE *Sitzungsberichte* of the Vienna Academy of Sciences (Physical, Chemical, and Astronomical Section, vol. lxxvi. parts 2–5, vol. lxxvii. parts 1–3) contain the following papers of interest:—On some maxims and proofs of the theory of the resultant, by Dr. B. Igel.—On a relation corresponding to the linear differential equations of the second order, by Dr. A. Winckler.—Researches on the componnds of the camphor group, by J. Kachler.—On the state of heat equilibrium of a system of bodies with reference to gravitation, by J. Loschmidt. —On a new radiometer, by Dr. J. Puluj.—On the substances found in crude anthracene, by Dr. O. Zeidler.—On the behaviour of camphor towards chloral hydrate, by the same.—On the orbit of Loreley (165), by Dr. G. Gruss.—On the behaviour of acetylene towards concentrated sulphuric acid, by S. Zeisel.—Researches on the heat-conducting capacity of cotton, sheepswool, and silk, by J. Schuhmeister.—On the action of ammonia upon isatine, by Dr. E. von Sommaruga.—On idryl, by Dr. G. Goldschmiedt.—On the action of hydrochloric acid upon resorcine, by L. Barth and H. Weidel.—On the behaviour of certain resins and resinous acids when distilled over zinc dust, by G. Ciamician. On the smallest absolute number of sound impulses which is necessary for the production of a tone, by Prof. Pfaundler.—Report on Egger's electro-magnetic motor, by Prof. R. Handmann.—On the stand-aneroid-barometer, by Dr. Anton Schell. —On citramalic acid, by Th. Morawski.—On the temperature of Vienna, deduced from observations during a century, by Dr. J. Hann.—On a partial differential equation of the first order, by Dr. Franz Hocevar.—On the connection of n different straight lines in a plane, and certain other mathematical maxims, by S. Kantor.—On the magnitude and position of the optical elasticity axes in gypsum, by V. von Lang.—On mononitropyrocatechine, by Dr. R. Benedikt.—On some problems in the theory of elastic after-effects, and on a new method of observing oscillations by means of mirrors, without incumbering the oscillating body with a mirror of considerable mass, by Dr. L. Boltzmann.—On the orbit of Laurentia (162), by K. Zeller.—On the application of Doppler's principle on the progressing motion of luminous gas molecules, by Prof. Pfaundler.—On some mathematical maxims relating to cone projections, by E. Weyr.—On the chemical nature of peptone and its relation to albumen, by Dr. E. Herth.—On the barometrical pressure at Vienna, by Dr. J. Hann.—On a new apparatus for the direct volumetric determination of the moisture of the atmosphere, by Prof. Fr. Schwackhöfer.—On the heat-capacity of mixtures of methylic alcohol and water, by E. Lecher.—On the electric after-currents of transversely magnetised iron rods, by Prof. H. Streintz.—On the velocity of transmission of spark-waves, by E. Mach, O. Tumlirz, and C. Kögler.—On the behaviour of propylglycol at a high temperature, by E. Linnemann.—On the direct transformation of isobutyl iodide into trimethylcarbinolamine, by B. Brauner.—On the artificial malic acid made from fumaric acid, by F. Loidl.—On Maxwell-Sympson's synthesis of acroleïne from acetone diiodide, by Dr. O. Voelker.—On the behaviour of β bibromopropionic acid towards iodide of potassium, by V. von Zotta.—Determination of the orbit of comet II 1874, by E. Wenzel.—On Ampère's electrodynamic fundamental experiments, by A. von Ettingshausen.—On bixine, by C. Etti.—On the decomposition products obtained by fusing hydrate of potash with an ammonia gum resin from Marocco, by Dr. G. Goldschmiedt.—On the action of bromine upon phenoldisulphonic acid, by M. von Schmidt.—On the construction of tangents to a rotation plane, by H. Drasch.—On some oxidation products of protocatechic acid, by Dr. M. Gruber.—On trisulfoxybenzoic acid, by Dr. M. Kretschy.—On the variation tone observed by Dvorak, by A. Haberditzl.—On the reduction of ellagic acid by means of zinc dust, by L. Barth and G. Goldschmiedt.—On a fluorescine-carbonic acid, by Dr. J. Schreder.—On the galvanic polarisation of platinum in water, by Franz Exner.—On trinitro- and trinitrophloroglucine, by Dr. R. Benedikt.—On the determination of the focus of the outlines of planes of the second degree, by C. Pelz.—On a synthesis

of pimelinic acid, by A. Bauer and J. Schuler.—New experiments to test Doppler's theory on the change of tone and colour by motion, by E. Mach.—On the magnetic declination and inclination at Vienna, by J. Liznar.—On the component parts of coralline and their relation to the colouring-matters of the rosaniline group, by C. Zulkowsky.—On the diffusion of carbonic acid by water and alcohol, by J. Stefan.—On the electromotive power of metals in the watery solutions of their sulphates, nitrates, and chlorides, by Dr. F. Streintz.

THE *Sitzungsberichte* of the Vienna Academy of Sciences (Physiological and Anatomical Section, vol. 76, parts 1-5) contain the following papers:—Observations on the origin of the cell nodule, by S. Stricker.—On the nerves of the cornea and its vessels, by Dr. L. Königstein.—On the properties of dialysed albumen, by Dr. M. Laptschinsky.—On the occurrence of two different knots of vessels in the kidney, by Dr. O. Drasch.—On some peculiar products of mycotic keratitis giving the amyloid reaction, by Dr. A. Frisch.—On the chemical reaction of the retina and the visual nerve, by Dr. A. Chodin.—On the laws of nerve irritation, by Dr. E. v. Fleischl.—On the termination of the olfactory nerves, by S. Exner.—On optional and cramp movements, by E. Brücke.—Researches on the perception of locality and its relation to the idea of space, by S. Stricker.—On the anatomy of the thalamus opticus and its surroundings, by Dr. F. Schnopfhagen.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 6.—"Preliminary Report upon the *Comatulæ* of the *Challenger* Expedition." By P. Herbert Carpenter, M.A., Assistant Master at Eton College. Communicated by Sir Wyville Thomson, F.R.S. Published by permission of the Lords Commissioners of the Treasury.

The collection of *Comatulæ* made by the staff of the *Challenger* includes specimens from 45 different localities, but few of which are deep-water stations. *Comatulæ* were only obtained seven times from depths exceeding 1,000 fathoms.

At lesser depths, 200—1,000 fathoms, *Comatulæ* were met with at 13 stations; but by far the greatest number both of species and of individuals were dredged at depths much less than 200 fathoms, and often less than 20 fathoms, at 26 widely distant stations.

The collection contains 111 species, mostly new; but as the work of examination and description progresses, it is not unlikely that forms now considered different may turn out to be merely local varieties of one and the same species, so that the number given above may be subject to alteration.

Of these 111 species, 59 belong to the genus *Antedon*, 48 to *Actinometra*, 1 to *Ophiocrinus*, and 3, which are peculiar in having ten rays to the calyx instead of only five, to a new genus for which is proposed the name *Promachocrinus* (*πρόμαχος Challenger*).

The distribution of *Promachocrinus* is as follows:—

P. Kerguelensis (20 arms).	Balfour Bay, Kerguelen,	20—60 fath.
	Royal Sound ,,	28 fath.
	Cape Maclear ,,	30 ,,
	Heard Island	75 ,,
P. abyssorum (10 arms).	Station 147	1,600 ,,
	,, 158	1,800 ,,
P. Naresii (10 arms).	,, 214	500 ,,

Ophiocrinus was obtained at four localities at depths varying from 565 to 1,070 fathoms, two in the South Pacific off South Australia and New Zealand respectively, and two in the North Pacific, one off Japan, and one just north of the Philippine Islands. All the specimens belong to one species, which is by no means so slender and graceful as Semper's Philippine species from shallower water, but has a much more massive arm skeleton.

The comparative distribution of the other *Comatulæ* is very striking. Relatively speaking, *Actinometra* is extremely limited in its range, both geographical and bathymetrical. It is almost exclusively a tropical genus, its northern limit being about 30° N. lat. and its southern 40° S. lat. Isolated species are known from the Cape of Good Hope, Natal, South Australia, and Port Jackson, but its chief home is Oceania, especially the Philippines and Moruccas. A few *Actinometra* species are also known from the west coast of the Atlantic, as South Carolina, the West Indies, Bahia, and St. Paul's Rocks.

The bathymetrical limit of *Actinometra* is likewise very slight. Nearly all the *Challenger* species are from depths less than 20 fathoms, while only three come from a greater depth than 100 fathoms. The individual species of *Actinometra*, like the genus itself, are very local in their distribution. Each of the forty-eight species of the *Challenger* collection has its own locality.

With *Antedon*, however, the case is different. Not only do nearly all the deep-sea *Comatulæ* belong to this genus, but some species of it have a fairly wide range. *Ant. rosacea* ranges from the north of Scotland to the Mediterranean, while *Ant. Eschrichtii* is found over a much wider area. It is well known on the American coast, and was dredged by the *Challenger* off Halifax, while the *Porcupine* met with it in the "cold area" of the North Atlantic.

Some *Antedon* species occur in duplicate from different localities. Two species from near the Kermadec Islands (S. 170), also occur in the neighbourhood of the Fijis (S. 174, 175). A third species was dredged at Stations 147 and 160, two localities in the Southern Sea, in nearly the same latitude, but separated by almost 90° of longitude. A fourth species came up from 1,070 and 775 fathoms, off the Admiralty Islands and Japan respectively.

The above facts would seem to show that, with few exceptions, the geographical range of the individual members of the family *Comatulidæ*, is exceedingly limited, nearly every species having its own locality, and that not a very extensive one.

The voyage of the *Challenger* has settled a curious question in connection with the Crinoids, the origin of which is due to Lovèn. It refers to *Hyponome Sarsii*, a so-called recent Cystid, which turns out to be nothing more than the disk of a *Comatula*, minus its skeleton. The anambulacral plating may be very extensive, forming a complete pavement over the ventral surface of the disc as in many *Pentacrini*; and the ambulacra are not wide and open as is usual in most *Comatulæ*, but almost entirely closed by the approximation of the marginal leaflets at their sides, so that the food-grooves radiating from the mouth are converted into tunnels.

The plates in the marginal leaflets are probably movable as unplated leaflets are in *Antedon rosacea*; so that they can be erected when the arms are spread out, leaving the grooves open for food particles to travel towards the mouth. On the other hand, when the arms are all contracted over the disk, the marginal plates fold over the grooves and cover them in. This is the condition of most spirit-specimens, but it is not in any way comparable to that of the palæozoic crinoids, in which the mouth is truly subtegminal while the ambulacra become real tunnels beneath the upper surface of the vault.

Sections through one of these plated *Hyponome*-disks show that all the various structures which underlie the grooves of ordinary *Comatulæ* are present and exhibit their usual characters.

The examination of the *Challenger Comatulæ* has entirely confirmed the opinions held by Dr. Lütken and the author respecting the distinguishing characters of *Antedon* and *Actinometra*. Both agree in referring forms with a (sub) central mouth, five equal ambulacra, and no terminal comb on the oral pinnules, to *Antedon*. On the other hand, species with an eccentric mouth, a variable number of unequal ambulacra, and a terminal comb to the oral pinnules, belong to *Actinometra*.

It will be seen at once that these characters are of no use in distinguishing the genera of fossil *Comatulæ*. But, there are very considerable differences in the shape of the radials and centrodorsal piece in *Antedon* and *Actinometra* respectively, and as these are exactly the parts which are most met with as fossils, the generic determination of a fossil form is almost as easy as that of a recent one, which has given up its disk to produce a *Hyponome*. The author has shown elsewhere that in *Act. polymorpha* and *Act. solaris*, half, or even more than half, of the arms may have neither ventral groove, tentacles, ambulacral epithelium, nor ambulacral nerve. No less than 23 out of the 48 species of *Challenger Actinometræ* may have more or fewer of such ungrooved arms, in which the ambulacral nerve is entirely absent. These arms are usually those which come off from the hinder part of the disc, but in one gigantic Philippine species with over 100 arms, there are several ungrooved arms on each radius. Evidence of this negative character appears to the author to be a serious objection to the German view, that the ventral bands constitute the sole nervous apparatus of the crinoids; and on the other hand, to strengthen the opinions held by Dr. Carpenter, and by the author, that the axial cords of the skeleton are also nervous in character.

"Observations on the Physiology of the Nervous System of the Crayfish (*Astacus fluviatilis*)," by James Ward, M.A., Fellow of Trinity College, Cambridge. Presented by Michael Foster, M.D., F.R.S., Prælector of Trinity College, Cambridge.

The experiments, of which this paper gave a brief account, consisted mainly in severing (1) one or (2) both of the supra-œsophageal commissures, (3) both the sub-œsophageal commissures, or (4) in dividing the supra-œsophageal ganglia longitudinally. From these experiments it was inferred:—

(*a*) That there is no decussation of the longitudinal fibres in the nervous system of the crayfish.

(*b*) That on the presence of the supra-œsophageal ganglion depend (1) the spontaneous activity of the animal as a whole, or what might be called its volitional activity; (2) the power to inhibit the aimless and wasteful mechanical activity of the lower centres; (3) the power to maintain equilibrium; and (4) the use of the abdomen in swimming.

(*c*) That the sub-œsophageal ganglion is the centre for co-ordinating (1) the locomotive, and (2) the feeding movements, and (3) for a peculiar rhythmic swing of the limbs seen as soon as the supra-œsophageal ganglia are removed, and (4) is the source of a considerable amount of motor energy.

(*d*) That there is much less solidarity, a much less perfect *consensus*, among the nervous centres in the crayfish than in animals higher in the scale. The brainless frog, *e.g.*, is motionless, except when stimulated, and even then does nothing to suggest that its members have a life on their own account; whereas the limbs of a crayfish deprived of its first two ganglia, are almost incessantly preening, and when feeding movements are started, the chelate legs rob, and play at cross purposes with, each other as well as four distinct individuals could do.

(*e*) That some stimulus from other centres is more or less necessary to the activity of any given centre.

(*f*) The "natural" discharge of a ganglionic centre (not exhibiting "volition") appears to be of a rhythmic kind; the rhythmic movements becoming converted into varied movements by temporary augmentation or inhibition.

Zoological Society, March 4.—Prof. W. H. Flower, F.R.S., president, in the chair.—Mr. Sclater exhibited and made remarks on examples of two rare Fruit Pigeons, of the genus *Carpophaga*.—Mr. L. M. D'Albertis exhibited some new and rare birds, obtained during his recent expedition up the Fly River, New Guinea.—Prof. Newton exhibited, on behalf of Mr. J. Robinson, of Trinity Hall, Cambridge, a specimen of *Sylvia nisoria*, believed to have been killed at Cambridge many years ago.—A communication was read from Mr. L. Taczanowski containing a list of the birds collected by Messrs. Stolzmann and Jelski, in Northern Peru in 1878. Fifty-six species were enumerated, several of which were new to science.—Mr. R. Bowdler Sharpe, F.Z.S., read some notes on birds obtained on Kina-Balu Mountain, in North-Western Borneo, by the collectors of Mr. Treacher, amongst which were several species new to science.—Mr. F. Jeffrey Bell read the first portion of some observations on the characters of the Echinoidea. The present paper contained remarks on the species of the genus *Brissus* and on the allied forms *Meoma* and *Metalia*.—A communication was read from the late Mr. F. Smith, F.Z.S., containing the descriptions of new species of *Hymenoptera* from Central America.—A communication was read from Mr. W. A. Forbes, F.Z.S., containing a synopsis of the Meliphagine genus *Myzomela*, to which was also added the descriptions of two new species.—A communication was read from the Rev. O. P. Cambridge, containing descriptions of some new and little known species of Araneidea, principally belonging to the genus *Gasteracantha*.

Chemical Society, March 6.—Dr. Gladstone, president, in the chair.—The following papers were read:—On the quantitative blowpipe assay of mercury, by G. Attwood. The method consists in distilling the compounds either alone or mixed with litharge or with oxalate of potash and cyanide of potassium in ingeniously contrived retorts of glass or steel, the whole apparatus being three to four inches long, collecting the mercury in water, and weighing it when dry.—On some points in the analysis of combustible gases and in the construction of apparatus, by J. W. Thomas. The author has succeeded in exploding marsh-gas, &c., with almost theoretical quantities of oxygen, by using a diminished tension, about 160 mm. As less oxygen is thus required, the author has shortened the eudiometer tube to 500 mm. and thereby increased the delicacy of the apparatus; he has also reintroduced a steel tap, of, however, perfect tightness, and has in several points perfected and simplified the ordinary Frankland's and McLeod's apparatus. The steel face plates connecting the laboratory and measuring-tubes have also been abandoned.—On the action of isomorphous salts in exciting the crystallisation of super-saturated solutions of each other, and some experiments on super-saturated solutions of mixed salts, by J. M. Thomson. The author finds that a crystal to act as a nucleus must be not only isomorphous, but chemically similar, as to the water of crystallisation and to the substance in solution. Interesting results were obtained by introducing a nucleus into a super-saturated solution of two non-isomorphous bodies; under certain conditions a separation of these two bodies could be effected.—On the isomeric dinaphthyls, by Watson Smith. The author has determined the vapour densities of the dinaphthyls by means of V. Meyer's new apparatus, and gives in his paper the results of the reaction of carbon tetrachloride, chloroform, &c., on naphthalene.

Geological Society, February 21.—Henry Clifton Sorby, F.R.S., president, in the chair.—George Bond, Francis Gaskell, and George Henry Hollingworth, were elected Fellows of the Society.—The following communications were read:—A copy of a letter from the late Acting-Governor of the Falkland Islands, relating to the overflow of a peat-bog near Port Stanley, in East Falkland. Communicated by H.M. Secretary of State for the Colonies.—Note on *Poikilopleuron bucklandi*, of Eudes Deslongchamps (*père*), identifying it with *Megalosaurus bucklandi*, by J. W. Hulke, F.R.S.—Note on a femur and a humerus of a small mammal from the Stonesfield slate, by H. G. Seeley, F.L.S., F.G.S., Professor of Geography in King's College, London.—A review of the British carboniferous Fenistellidæ, by G. W. Shrubsole, F.G.S.

Anthropological Institute, February 25.—Mr. John Evans, D.C.L., F.R.S., vice-president, in the chair.—A paper by Mr. C. Staniland Wake, on the primitive human family, was read by the Director. The author endeavoured to combat some of the views usually associated with the name of Mr. McLennan.—Mr. E. W. Brabrook, F.S.A., read a paper entitled "Notes on the Colour of the Skin, Hair, and Eyes." The paper accompanied an exhibition of the "Echelle de Couleurs," published by the Société Sténochromique of Paris. The accurate determination of the colours of the skin, hair, and eyes, is a matter of great interest to anthropologists, and the author considered that though the object of the publication of this scale of colours was not exclusively anthropological, yet its value to anthropologists would be very great. Forty-two colours are specialised, of each of which there are about twenty shades.

Victoria (Philosophical) Institute, February 3.—Prof. McK. Hughes, F.R.S., delivered a lecture upon the antiquity of man, in which he analysed the nature of the evidence brought forward by others.

CAMBRIDGE

Philosophical Society, February 24.—Prof. Liveing, president, in the chair.—Prof. Cayley gave an account of an investigation which he had been led to, relating to what he calls the "Newton-Fourier Imaginary Problem." The Newtonian process of approximation to the root of a numerical equation $f(u) = 0$, consists in deriving from an assumed approximate root ξ, a new value $\xi_1 = \xi - \frac{f(\xi)}{f'(\xi)}$ which should be a closer approximation to the root sought for; taking the coefficients of $f(u)$ to be real, and also the root sought for, and the assumed value ξ, to be each of them real, Fourier investigated the conditions under which ξ_1 is in fact a closer approximation. But the question may be looked at in a more general manner; ξ may be any real or imaginary value, and we have to inquire in what cases the series of derived values $\xi_1 = \xi - \frac{f(\xi)}{f'(\xi)}$, $\xi_2 = \xi_1 - \frac{f(\xi_1)}{f'(\xi_1)}$, . . . converge to a root, real or imaginary, of the equation $f(u) = 0$. Representing as usual the imaginary value $\xi, = x + iy$, by means of the point whose co-ordinates are x, y, and in like manner $\xi_1, = x_1 + iy_1$, &c.; then we have a problem relating to an infinite plane; the roots of the equation are represented by points $A, B, C, \ldots$; the value ξ is represented by an arbitrary point P; and from this by a determinate geometrical construction we obtain the point P_1, and thence in like manner the points $P_2, P_3 \ldots$ which represent the values $\xi_1, \xi_2, \xi_3 \ldots$ respectively. And the problem is to divide the plane into regions, such that starting with a point P_1 anywhere

in one region, we arrive ultimately at the root *A*; anywhere in another region we arrive ultimately at the root *B*; and so on for the several roots of the equation. The division into regions is made without difficulty in the case of a quadric equation, but in the next succeeding case, that of a cubic equation, it is anything but obvious what the division is, and the author had not succeeded in finding it.

MANCHESTER

Literary and Philosophical Society, February 4.—E. W. Binney, vice-president, F.R.S., in the chair.—The area of the middle drifts as determined by their contents, by Alfred Bell, F.G.S. Communicated by R. D. Darbishire, F.G.S.

February 18.—J. P. Joule, F.R.S., &c., president, in the chair.—On a chemical investigation of Japanese lacquer, or "urushi," by Sadamu Ishimatsu. Communicated by Prof. Roscoe, LL.D., F.R.S.—On the bursting of the gun on board the *Thunderer*, by Prof. Osborne Reynolds, F.R.S., Professor of Engineering, Owens College, Manchester.

BOSTON, U.S.

Society of Natural History, October 2, 1878.—Notes on the physical geography and geology of Trinidad, by W. O. Crosby.

October 16.—The peculiarities in the growth of the Swamp Cypress (*Taxodium distichum*).

October 23.—Museum pests observed in the entomological collection at Cambridge, by Dr. H. A. Hagen.

November 6.—A century of orthoptera. Decade viii. Acridii (Melanoplus), by S. H. Scudder. Decade ix. Acridii (Pezotettix).

VIENNA

Imperial Academy of Sciences, January 23.—The following, among other papers, were read:—Measurements on simultaneous oscillation, by Prof. von Ettingshausen. This relates to a suspended and swinging coil of wire, the currents of which set a galvanometer needle swinging.—On curves of the fourth order with three double points, by Herr Ameseder.—On the diffusion of liquids (second part), by Herr Stephan.

February 6.—On some new and rare fish-species in the zoological museums of Vienna, Stuttgart, and Warsaw, by Dr. Steindachner.—On the occurrence of chlorophyll in the epidermis of leaves of phanerogams, by Herr Stöhr.—On the phenomena in the circulation after temporary closure of the aorta; contribution to the physiology of the spinal chord, by Prof. Mayer.—On the transformation of iodide of phenol into dioxybenzol, by Prof. Lippmann and Herr v. Schmidt.—Variations in structure and growth of the mesentery of the human intestine, by Prof. Toldt.—On organic ferricyanide compounds, by Herr Bernheimer.—Action of the fusing hydrate of soda on phenol, and synthesis of phloroglucin, by Herren Barth and Schreder.

February 13.—On muscular sounds of the eye, by Prof. Hering.—Covellin as a superficial pseudomorphism of a Celtic axe of bronze found at Salzburg, near Hallstatt, by Dr. v. Hochstetter.—Oxidation of resorcin to phloroglucin, by Professors Barth and Schreder.—Remarks on Dr. Wangen's memoir on the geographical distribution of fossil organisms in India, by Herr Wynne.

PARIS

Academy of Sciences, March 3.—M. Daubrée in the chair.—The following papers were read:—Reply to M. van Tieghem concerning the origin of *Amylobacter*, by M. Trécul.—Researches on the fœtal envelopes of the armadillo with nine bands, by M. Milne-Edwards. Exceptionally to the rule with mammalia, the four fœtuses of this armadillo are all lodged in a common chorion.—The waters of the Cheliff; some observations regarding the interior sea of Algeria, by M. Balland. The Cheliff is the principal water-course of Algeria, and flows to the Mediterranean. At present it carries down over three million tons of earthy matters (chiefly silica and clay) in twenty-four hours; this would give a layer 1 metre thick, over 300 hectares. The numerous Saharan rivers lost in the Chotts also convey large quantities of earth and sand; then there is the sand displaced by winds. These considerations are urged against the interior sea.—Discovery of a small planet at the Observatory of Marseilles, by M. Stephan.—Extract from a letter from P. Ferrari, relating to the intra-Mercurial planet. This calls attention to an important observation by P. De Vico in 1837 (which seems to have escaped Leverrier's notice), on a planet-like body which then passed over the sun's disk.—Formulæ relative to the theory of planetary perturbations, by M. de Gasparis.—On the multiplication of elliptic functions, by M. Halphen.—Resolution of a class of congruences, by M. Pellet.—On the emissive power of coloured flames, by M. Gouy.—On the spectra of absorption of didymium, and of some other substances extracted from samarskite, by M. Soret. The facts indicate the existence, in the didymium from samarskite, of a substance at least different from didymium. The same substance seems to be in less quantity in terbine, and in least in the didymium from cerite.—Action of sulpho-cyanate of ammonium on monochlorised acetone, by MM. Norton and Tcherniak.—On amidised acids derived from butyric and isovaleric acids, by M. Duvillier.—Researches on digestion in cephalopod molluscs, by M. Jousset de Bellesme. The posterior salivary glands of the poulpe facilitate the digestion of albuminoids, laying bare the muscular fibre for action of the chief digestive juice. The upper salivaries are merely connected with mastication and swallowing. The animal has a digestive aptitude only for albuminoid and connective matters; and this is the more remarkable because some of its organs, *e.g.*, the liver, contain a large amount of fatty matters.—Researches on *Peronospora gangliiformis* of lettuce, by MM. Bergeret and Moreau. Water slightly acidulated with nitric acid is a good remedy; it poisons the Peronospora and is a manure for the soil.—Influence of oxygen on alcoholic fermentation by beer yeast, by M. Bechamp. In a first series of experiments pure oxygen was conducted into the fermenting mixture continuously. In a second series the electrodes from a battery of six or eight Bunsen elements were put in the mixture; the gases of fermentation were collected, and the oxygen proved to be mostly absorbed. Oxygen acts as an excitant, stimulating the life of the yeast and the mutations of its matter. In the first period of fermentation (with the current) the absolute quantity of alcohol formed was greatest; it diminished to the end. The acetic acid increased from the beginning. The sugar-water absorbs part of the oxygen. Pure yeast in water under weak electrolytic action, may absorb all the oxygen.—On a method of conservation of infusoria, by M. Certes. He employs a solution (2 per cent.) of osmic acid. The important point is to make the reagent act promptly and with a certain force. One way, suitable for most cases, is to expose infusoria on a glass plate to the vapours of osmic acid for ten to thirty minutes. For very contractile infusoria, a drop of the reagent is deposited on the cover before covering the drop of water which contains them. Excess of liquid is removed with Joseph paper; two opposite sides of the cover glass are luted with paraffin or Canada-balsam; and for coloration a mixture of glycerine, picrocarminate, and water (equal parts) is used.—On the unity of forces in geology (continued), by M. Hermite. He offers objections to the hypothesis of igneous fluidity. The present form of the earth is attributed to the presence of its seas. Volcanic phenomena do not agree with the existence of central fire, or even a sea of lava of small extent.—On the hurricane which traversed Switzerland on February 20, 1879, by M. Forel. He finds in the data strong proof of a gyratory motion of the atmosphere.—Theory of glazed frost; reclamation of priority, by M. Nouel.—On some former examples of glazed frost similar to that of January last, by M. Vogt. He gives an account of one instance of the phenomenon observed at Geneva in 1856.

CONTENTS

THURSDAY, MARCH 20, 1879

ROYAL AGRICULTURAL COLLEGE, CIRENCESTER

DURING the last few years the question of Agricultural Education has been very fully and fruitfully discussed. The experiment of an examination in the principles of agriculture, under the Science and Art Department, had an unexpected success; and showed that there was throughout the kingdom a demand for instruction in agricultural matters. At the present moment efforts are being made to satisfy this demand more completely by means of local organisation for developing and extending the facilities already offered by the Science and Art Department.

There is at Cirencester a college founded specially for the advancement of agricultural education. It has one—or more—Royal Charters; it has the power of granting diplomas; it is under Royal patronage, and has the advantage of being managed by numerous Earls and M.P.s. This institution ought to (and might) have been the centre of the movement to which allusion has been made; but, unfortunately, its own troubles seem to be enough to occupy the whole attention of the Committee of Management; and, for the second time in the history of the college, threaten to bring about its extinction. For the past few weeks the agricultural press has been teeming with letters and articles headed "Professor Church and the Royal Agricultural College." The facts, as to which there seems to be no dispute, are briefly these:—Prof. Church is about to be married. Other professors, his colleagues and juniors, had done the same, and non-residence in their cases was not found incompatible with the proper performance of their several duties; as a matter of fact each of Prof. Church's predecessors was non-resident. Yet the Principal intimated to Prof. Church that without residence he could "no longer discharge the duties of Professor of Chemistry in this college." It appeared that this decision on the part of the Principal was not authorised under the bye-laws: such a point could be determined only by the Committee of Management; and the case was referred to them. The result was, however, unaltered. While "fully sensible of the services rendered by Prof. Church during his sixteen years' residence in the College," the Committee "regret that they cannot accede to his recent proposal of non-residence." The consequence of this was two resignations. Prof. Lloyd Tanner regarding the decision "as showing that neither long and zealous performance of duty, nor special ability for work are duly recognised," has resigned the Chair of Mathematics and Physics; and Prof. Fream, "as the only protest it is in his power to make against the treatment his colleague has received," similarly vacates the Chair of Natural History.

Such are the circumstances under which the three senior resident professors at Cirencester College are leaving. Other matters have rendered the affair even more painful than it need have been, but we believe the simple, undisputed facts of the case are amply sufficient to enable our readers to form a just opinion of the mode of managing Cirencester College. Those who intend to become candidates for the vacant chair have had an opportunity of judging how one bye-law can be and is used against a man such as Prof. Church; we will only advise them to study the other bye-laws and guess how they may be used against younger and less known men. Having seen these bye-laws ourselves we are curious to know who will be induced to replace the vacancies just announced.

PROF. HUXLEY'S HUME

Hume. By Prof. Huxley. (London: Macmillan and Co., 1879.)

PROF. HUXLEY has given a clear and succinct account of the philosophy of Hume, in a style at once fresh and pointed. We should be thankful to him that, following the example of Locke and Hume himself, he discusses philosophical questions in genuine and idiomatic English, and consistently avoids the use of a lumbering phraseology, imported from abroad, amid which the thinking evaporates, for the most part, in pure verbalism. The volume before us is limited to a brief account of Hume's life and his philosophical opinions. It hardly touches what has been said on the other side in criticism or in correction of Hume's views. Here and there Prof. Huxley offers a criticism; but, though generally acute, it is seldom on anything but a point of detail. Indeed, the volume may be described as rather too much of a bare statement of Hume's principles and conclusions.

As Prof. Huxley may fairly be regarded as dogmatically accepting Hume's principles and boldly carrying them out to their results, while Hume may with probability be regarded as having only hypothetically held the principles, we might have expected a fuller vindication of them than is at all attempted in the volume. On all the metaphysical questions of greatest moment Prof. Huxley's position is a negative one; and if, as it seems, he accepts Hume's principles absolutely, it is one of complete negation.

In the opening chapter on the Philosophy (Chap. II.) Prof. Huxley has done good service in clearly stating the terms of the question. He very properly points out that the question regarding the limits of knowledge, or "What we can know," is not a primary but a secondary question. He is emphatic in showing that it implies the previous questions as to what we mean by knowledge, and how we come by the thing we call knowledge. And he very well points out that these latter questions are psychological, and that psychology, accordingly, is the only proper basis of assertions about knowledge, whether these refer to its nature, conditions, or limits. This clear and vigorous statement is not inopportune, for there is somewhat of a tendency at present, very inconsistently indeed, to ignore psychology. We have professions of "deducing" the conditions of "experience." It seems strange that it does not occur to the advocates of such a method that its basis is necessarily an accurate examination of what experience or consciousness in its fullest extent is; what, in a word, is the thing spoken of, whose conditions it is proposed to evolve. This implies a full and scientific psychology—the only safeguard against fantastic system-making, otherwise the so-called "deduction" becomes a method of *if* and *must*

—hypothesis and hypothetical inference; having no bearing on our experience.

What Hume really sought in philosophy was the ultimate element, out of which all valid knowledge might be shown to flow. This element was to be at once the source and the test of every conception of the human consciousness. This he supposed he found in the "simple impression" or "simple impression of sensation." The essence of Hume's method is to reduce all so-called knowledge of objects to this test; his constant demand is—show me the "impression" from which your alleged conception or idea is derived, and then, but then only, shall I admit the reality and validity of your knowledge. If our conception be meaningless, the object of it is unreal. It is easy to see how on such a method, whether adopted hypothetically or dogmatically, self-existence, self-identity, personality, and Deity must be given up.

But the question at once arises :—*What* precisely is this so-called "*impression of sensation*," or "*singular sensation?*" The psychological method has been admitted. And we must apply this method to find whether there is such a thing as an *impression per se*. It is at least a consciousness, or state of consciousness. If it be said that *impression* is not the full fact, but a mere abstract part of the complex fact which we call consciousness—this is a position which is quite as vindicable on Hume's psychological method as his statement of the fact is. We do not require to have recourse here to any "transcendental deduction," or to Prof. Huxley's "pure metaphysician." We only ask whether the psychological method is fairly applied to the fact. Here we do not think that Prof. Huxley has done any justice to those who say and seek to show that *impression per se* is a mere abstraction—possibly even a simple unintelligibility.

No doubt Prof. Huxley tells us that Hume omitted an entirely irresolvable element of consciousness, viz., relation, as of succession, co-existence, &c. But one does not see that Prof. Huxley apprehends the true force of his own admission. The *relation* of succession is still as much an abstraction as *impression* is, in fact, an unintelligibility, unless on the supposition of some one conscious being,—subsisting through varying times. An appeal to memory is of no use here. Memory itself is but a phrase for the act of one and the same conscious being subsisting and recognising impressions in successive times. The unity of the conscious being is the ground of memory; not memory the ground of it; as this unity is equally the ground of the possibility of a known relation of succession, or successive impressions. Prof. Huxley does not recognise this in its proper place; he even in the end gives in his adhesion to Hume's denial of a *self* or unity in consciousness at all. But by this he cuts away all ground of right to acknowledge relation in knowledge; all ground in fact to affirm or deny anything.

Hume at once naturally takes up the question as to the kinds of impressions conveyed, as he phrases it, through the senses. His answer to this question may be said to be that all we know through the senses is of the same kind, whatever be our natural belief to the contrary. Figure, bulk, motion, colours, tastes, smells, sounds, heat and cold—pains and pleasures, from application of objects to our bodies—are all simply impressions or conscious states—each class has but the same "interrupted and dependent being." They are "nothing but perceptions arising from the particular configurations, and motions of the parts of bodies." In that sentence lies the main inconsistency of Hume; and it is a key to the constant shifting of ground, which, with all deference to the admirers of the consistency and cogency of his reasoning, nullifies large portions at once of the "Treatise of Human Nature," and the "Inquiry Concerning Human Understanding." For if the senses can in no way give us more than a conscious impression, they are absolutely impotent to tell us of a body which is not itself merely a conscious impression. And to say, therefore, that bodily motions are the antecedents or causes of conscious impression is simply to say that conscious impression is the antecedent or cause of conscious impression. If Hume assumes that the senses do more than this, and distinctly inform us of objects called body and bodily motion, then he contradicts his own doctrine regarding the reach and sphere of the senses. And if he holds that body is the cause of impressions, he must admit a clear knowledge both of body and of what it can do.

But Hume is represented as stating and refuting with effect "the arguments commonly brought against the possibility of a causal connection between the modes of motion and the cerebral substance and states of consciousness" (p. 76). Hume's argument is as follows: Cause is simply constant conjunction; *à priori*, anything may produce anything; no reason is discoverable why any object may or may not be the cause of any other, however great or little the resemblance between them. Thought *may* therefore be the effect of motion; we may perceive a constant conjunction of motion and thought. Nay, it is certain we have this perception, "since the different dispositions of the body change the thoughts and sentiments." Hence "motion may be, and actually is, the cause of thought and perception."

In this so-called proof Hume evidently felt in a dim way the force of the objection, that, on his doctrine, thought and motion are really identical, that in fact he was only surreptitiously begging for motion, a character which his system denied it—the vulgar realistic view—in order to prove that thought as a distinct thing from motion was yet produced by it. Accordingly we find a clause, as is Hume's manner, quietly inserted to blunt this criticism by the way. "We find," he says incidentally, "by the comparing the ideas that thought and motion are different from each other." Possibly enough that is so; but the difference, whatever it may be, cannot, on Hume's doctrine at least, be allowed to extend beyond the common genus of conscious impressions; and it is, therefore, wholly irrelevant to his argument.

Prof. Huxley must know that all psychologists of note, and of the most different schools, from Hartley to Hamilton, have admitted the fact of "constant conjunction," of bodily organic impressions with conscious sensations and perceptions. But after all that Prof. Huxley has said, as to the place which this organic impression has in the production of the sensation, the questions remain whether it is the cause, or a concause, or merely a condition, on which a higher power comes into play. Prof. Huxley has surely read of the fact of mental absorption—that state of mind in which, when it is occupied by strong emotion, or by intense thought, all the organic impressions may take place,

and yet no sensation follow. When a person is writing, the clock may strike in the room, the impressions on ear, nerve, and brain being complete, and yet the next moment he may have not the slightest memory of the sound; certainly, at least, not the consciousness or memory which in ordinary circumstances he would have had. These organic impressions have thus to meet as it were with something other than themselves—something we call consciousness or mind—ere even sensation becomes actual, or a mental fact. This truly reduces them to the place of a simple concause, and shows that there is another factor which they do not necessarily command, and which must concur in the realisation of the very lowest form of mental life. Then these physical antecedents relate to but the lower phenomena of mind. Even if it can be shown that imagination and intellect use portions of the brain, it must at least be admitted that they are there to use them. Can it be said that the apprehension of relations, or the act of generalisation, or volition, is properly spoken of as a conscious *impression?* Does Prof. Huxley imagine for a moment that any careful psychological analyst would place such operations on a level with the consequent of a series of organic movements

Again, what is the real meaning of the phrase that "the operations of the mind are functions of the brain, and the materials of consciousness are products of cerebral activity?" (p. 80). Prof. Huxley quite sees and admits that this is what is called "materialism," and indeed it is nothing else. One ought to thank him for his candour. But I should like very much to know the precise meaning of the statement so characterised. When analysed, it means this: that the nervous current generated by the brain out of food and blood is transmuted into mind; that as a certain molecular motion is transmuted into heat, so a certain nervous motion is transmuted into consciousness or mind. Now it seems to me, on the other hand, that not even sensation, to say nothing of intellect or the apprehension of relations of succession, coexistence, similarity, has been shown to be the transmutation of nervous force. We observe that physical forces are transmuted into each other; we can even quantitatively determine equivalents in this case. But the method fails us the moment we seek to show that or how a state of consciousness is a transmutation of the unconscious. For now we are no longer dealing with forces of the same kind—forces equally objects of consciousness itself—and known to be, to a certain extent, numerically determinable; we are dealing with the unconscious and the conscious; we are trying to bridge a gulf, on the further side of which we have no basis. We have no measure or rule for showing how the unconscious and the conscious are convertible, or that they have any conceivable relation whatever. Besides, even if we get sensation out of nervous force, what of the relations of difference, resemblance, succession, and coexistence among those sensations? Mr. Huxley calls these *impressions of impressions.* This is a very inaccurate expression. An impression of an impression must at least be picturable in the imagination. It is not so here. These relations are discerned by the intelligence; they suppose impressions; their material or nerve-antecedent is not observable, and they can in no way conceivably be referred to physical movements. Further, a physical or brain-force, though it give one definite sensation, or even a series, cannot provide for the pervading unity of self-consciousness. Physical forces, which are perpetually changing, successive and different, cannot be made convertible with the sense of unity which pervades all our consciousness. And further, the consciousness of a series of impressions, even of two impressions, the recognition of this fact or relation, its being in our consciousness at all, implies a standing unity of consciousness, a self or being, one and identical, which may be awakened into conscious life in or through those impressions, but which is in no way made by them—rather, is necessary to their being made or known.

But is Prof. Huxley's conclusion at all consistent with the law of physical energy? According to the law of the transformation of energy, the energy represented by motion or molecular change in matter passes into a consequent, which is also a movement or molecular change. The antecedent and the consequent states are still only forms of molecular change; and the amount or quantity of the antecedent is represented by the amount or quantity of the consequent. There is transformation of energy; but there is no change in the kind of the consequent. Now according to Prof. Huxley, a state of consciousness called sensation, or emotion, or idea, is as much the result of "the molecular changes which take place in that nervous matter which is the organ of consciousness, as the nerve-vibrations are the result of the impact of the light-waves on the retina." At the same time Prof. Huxley holds that the state of consciousness is distinct in kind or quality from the physical movement. It is psychical, or a form of *psychosis* as opposed to *neurosis.* And indeed he must admit a distinction in quality in the two cases. For the physical movement is possible—nay, is actually carried on apart from consciousness; whereas the sensation, the very lowest form of consciousness, is possible, is actual only in consciousness itself. There is all the difference between the fact which depends on observation by eye-sight and the feeling which is self-guaranteeing while it lasts, between the unconscious observed and the conscious felt. But be this as it may, he admits the distinction, as in fact impassable in thought. How is it then consistent to say that the state of consciousness is the effect of the physical movement? Either the law of physical energy is observed, and then we have only a physical movement as the determined result; or it is not, and then we have a state of consciousness, something distinct in quality from a physical movement; that is, we have as the result of the given physical force that which was not contained in the force as a simple quantum of physical energy.

But Mr. Huxley, following, as he thinks, Hume, tells us somewhat singularly that this materialistic doctrine of the origin of mind "contains nothing inconsistent with the purest idealism" (p. 80). In other words, what we call matter turns out in the end to be a purely hypothetical entity, assumed as a cause of certain states of consciousness. The very conception of such an entity is inconsistent with the basis here given; for if our sense-knowledge, indeed all our knowledge, be restricted to states of consciousness called feelings, we are precluded from forming an idea even of matter as an

object transcending consciousness, or of anything but states of consciousness, their compounds, and relations among themselves. To speak of "matter" as a cause of our feelings is, on such a theory, meaningless; and such a cause as an inference is impossible. Matter and motion, then, are simply convertible with states of consciousness, in fact, with feelings. And when we are told that these phenomena precede and cause the states of consciousness we call sensations, emotions, thoughts, we say merely that one set of states of consciousness is antecedent and cause of certain others. We have, therefore, wholly given up the dualistic scheme and the aim with which we started, viz., that of explaining the feelings by material phenomena. We now really profess to explain the whole of our conscious states—or mind—by one set of its states or phenomena, viz., those we call matter and motion. But does Prof. Huxley not see the *petitio principii* involved in such an argument? When I am cognisant of the phenomena, matter and motion, have I not assumed consciousness and its states to account for consciousness and its states; or rather, which is worse, have I not assumed certain very elementary states of consciousness—to account for, in fact, to generate the whole contents of mind—in all their complexity and reach—intellect, emotion, desire, volition, and moral sense? This is cutting the knot coarsely with a hatchet. It is not even solving the problem as to how from rudimentary states of consciousness itself, mind can rise to its recognised fulness and complexity—rise, in a word, to that which we call matured consciousness.

J. VEITCH

SACHS'S VENEZUELA

Aus den Llanos. Schilderung einer naturwissenschaftlichen Reise nach Venezuela. Von Carl Sachs. (Leipzig: Veit, 1879.)

NO one who has a liking for natural history should omit to read Dr. Sachs' account of his adventures in the Llanos of Venezuela. German books of travel, though possessing a large amount of solid information, are often rather dry and heavy. But Dr. Sachs' volume is certainly an exception to the rule, and may, we think, be placed, as regards the interest of its narrative, nearly, if not quite, on a par with the well-known works of Bates and Wallace.

The late Dr. Carl Sachs, who was formerly assistant to the great physiologist of Berlin, Emil du Bois-Reymond, and lost his life in an unfortunate accident on the glaciers of Monte Cevedale in August, 1878, went out to Venezuela, not with the ordinary objects of the travelling naturalist, although no opportunity was lost of collecting specimens, but for the especial design of obtaining a better knowledge of that most wonderful of fishes commonly called the electric eel (*Gymnotus electricus*). No more appropriate use could certainly have been made of the "Humboldt-Fund," collected in order to preserve in memory that great naturalist, than the devotion of it to such a purpose. Humboldt's account of the electric eels and the mode of their capture, is among the best known portions of his travels. Nearly eighty years had passed without any naturalist having trodden in Humboldt's footsteps, or having attempted on the spot the further elucidation of the extraordinary properties of this fish, aided by the enormous development which the science of physiology had made since that period.

With this object, therefore, Dr. Sachs left Europe in October, 1876, determined to visit the home of the electric eels in the same streams that Humboldt had found them in the year 1800. To arrive at this destination is not in these days a matter of great difficulty. From Hamburg a swift ocean-steamer bore our naturalist to La Guayra, and a day's ride over the coast chain of the Andes brought him to Caracas, the capital of Venezuela. After a few days' spent in rest in this lovely city and in excursions in the neighbourhood, Dr. Sachs turned his face due southwards, and, accompanied by servants and baggage-mules, rode over the grassy plains, or Llanos, which cover the southern part of the republic. Ten days' travel brought him to the little village of El Rastro, situated on one of the small confluents of the Rio Sisnado, a branch of the Orinoco, the very spot where Humboldt had captured *Gymnoti* seventy-six years before.

Humboldt's account of the mode in which this operation was effected in his days is well known. The Indians "fished with horses." About thirty wild horses and mules from the Llanos were collected and driven into the river. The stamping of the beasts drove the eels out of their hiding-places in the mud into the middle of the stream, where they got under the bellies of the horses and attacked them with repeated discharges of their electric organs. The unhappy quadrupeds rushed out to the banks, but were driven back into the water by the shouts and sticks of the surrounding Indians, until many of them, exhausted by the repeated shocks of the *Gymnoti*, sank to rise no more. The eels thus lightened of their superabundant stock of electricity were easily captured by the Indians.

Such is Humboldt's well-known story. But strange to say the Venezuelans of the present day simply laughed when Dr. Sachs proposed to put a similar plan in operation, and said they had never heard of such a thing. Indeed Dr. Sachs after various inquiries on the subject, was at last driven to the conclusion that fishing for electric eels with horses, as described by his illustrious countryman, must have been quite an exceptional occurrence, and could never have been a recognised custom.

In fact, Dr. Sachs was altogether unsuccessful in inducing the people of El Rastro to procure him electric eels in any way, and, after some rather disheartening attempts, shifted his quarters to the neighbouring town of Calabozo, where he hoped to find better quarters and a more intelligent set of assistants. Here, also, although his offers for electric eels were raised to ten pesos (about 30*s.*) a head, the fishes did not "come in," and poor Dr. Sachs was almost beginning to despair, when he fortunately heard of a certain "Llanero"—General Guancho Rodriguez—the very man for the occasion. How under Don Guancho's generalship these redoubtable eels were at length captured and brought home to the doctor's laboratory at Calabozo, how the necessary experiments were conducted to the wonderment of the good Calabocenos, and how Christmas is passed in that city, is all well told in some entertaining chapters, which will be much appreciated by those who read Dr. Sachs's narrative. It musts suffice for us to say that during Dr. Sachs's stay at Calabozo, which lasted until March, 1877, the main objects of the expedition were fully attained, and a number of important researches

carried out, the results of which, owing to Dr. Sachs's untimely decease, will, we fear, never be given to the public.

Leaving Calabozo, Dr. Sachs continued southwards to San Fernando de Apure, and thence down the Apure and Orinoco to Ciudad Bolivar, the capital of this part of Venezuela. From Ciudad Bolivar steam quickly carried him to Trinidad, and thence back to Europe. Of this section of his journey, as of the former part, Dr. Sachs's narrative is full of interest—nor will any one who reads it fail to regret that so promising a life should have come to such an early conclusion.

OUR BOOK SHELF

Education as a Science. By Alexander Bain, LL.D. (C. Kegan Paul and Co., 1879.)

THIS work, belonging as it does, to *The International Scientific Series*, naturally calls for some notice in our columns. We must confine our remarks, however, to the portions which deal with Mathematical, Physical, or Natural Science. The author, though Professor of Logic and English Literature, has already appeared before the public as one of the Editors of *Arnott's Physics*, and has laid down the law in a somewhat peremptory way about Elementary Geometry. We expect to find, therefore, accurate science, and above all, clear and definite composition, in his work.

The first of the following extracts supplied a hint which enables us to make the book review itself. We have taken the liberty of italicising a few words, in other respects we quote *verbatim.*

"Definite descriptions of definite failures, without note "or comment, are a power to punish. When there "are *aggravations, such as downright carelessness*, a "damaging commentary may be added; but in using "terms of reprobation, still more strict regard has to "be paid to discrimination and justice. The degrees of "badness are sometimes numerical . . . . this very "definiteness *literally stated* is more cutting than "epithets."

"The phrase 'cæteris paribus' (other things remaining "the same) is a mathematical coinage, for guarding "against the error of supposing that a course (*sic*) will "produce its effect under all circumstances indiscrimi-"nately."

"The advantages above set forth are such as Mathe-"matics is peculiarly fitted to give, and *without which* "*they* are scarcely ever attained at all. In so far as the "physical sciences unfold similar advantages the way is "paved for them by Mathematics. To this short sketch "of what Mathematics does, we should, for the sake of "clearness, append what it does not do, *and must be left* "*undone*, if we stop with it."

"The *earlier* parts of such subjects as Geometry and "Algebra need the *longest* iteration: the progress should "be at an *accelerating* rate. The higher Mathematics "should not be commenced with immature or incapable "minds."

"How to embody the actual problems in mathematical "language,—for example, the problems of motion in the "scheme of differential coefficients,—is a standing em-"barrassment, not to be met by any of the arts of "ordinary tuition."

"Try a child to lift a heavy weight first by the direct "pull, ..."

"Many trials must be allowed to *get a child into a new* "*shade of vowel*, as, for example, when Scotch (*sic*) "children have to learn the English sound of 'all.'"

"A high wrangler is a man professionally fitted for some "special post involving Mathematics; but, if he turns to "one of the other professions—Law, Medicine, the "Church, the Public Service, *he has incurred* an irreme-"diable waste of human strength."

Having attentively perused these extracts, the reader will probably be prepared to consider the following statement as more than plausible:—

"A purely psychological or metaphysical education "*might* be the worst case of any . . . ." P. G. T.

Life in Asiatic Turkey. A Journal of Travel in Cilicia, Isauria, and parts of Lycaonia and Cappadocia. By the Rev. E. J. Davis, M.A. Map and Illustrations. (London: Stanford, 1879.)

MR. DAVIS resides as Chaplain at Alexandria, and the present thick volume is the result of a tour in Northern Syria, in the summer of 1875. It is surprising that a region so full of interest should have been so little visited, and therefore Mr. Davis's account of what he saw is specially welcome. That there is much to interest in these parts is evident from all that Mr. Davis tells us, and his quiet and painstaking narrative will well reward a careful perusal. The pictures, coloured, from drawings by Mr. Davis, are unusually good, and add greatly to the interest of the volume, which is likely to take its place as a standard reference-work on the region with which it deals.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Tempel's Comet

WE find in NATURE, vol. xix. p. 347, some detailed particulars about the return of the comet 1867 II. (Tempel) in the course of the present year. In this article Sandberg's calculations are also taken into account. The real value of his studies about this comet seems to be but little known, as in other instances as well, new calculations are based upon them. In the *Astron. Nach.* Sandberg has only given a very short account of the results of his researches, and it is therefore but natural that their real value must be more or less concealed. Sandberg has published the details of the first determination of the orbit in a special treatise (Specimen Inaugurale de Orbita Cometæ II., 1867, Zwollae, 1869), which seems to be not very much known, and it is not necessary to read much of this work to make sure of the negative value of the calculations.

I now take the liberty to give here some short notices, which are, however, quite proof enough to keep astronomers eventually from unnecessary calculations. Sandberg makes use of all observations, and forms the normal places by the deviations of the different observations from the provisional ephemeris. In doing this he rejects, "prompted by the example of others," (*sic*) all observations which deviate more than 1·5s. in R.A., and 20″ in D. from the *ephemeris*, and not from the average. If, for instance, as with a normal place the mean deviation comes to + 14″ in D., he rejects observations which deviate 6″ from this average, whereas he accepts others to their full value, the deviation of which from the average is nearly 30″ (− 14″ from the ephemeris).

According to this proceeding is the accuracy of his calculations. An observation of Pulkowa deviates, according to his calculation, from the normal difference in D. (− 2″·2) 21″ (18″·9). If he had taken the parallax with correct sign, the observation would deviate 4″ from the average. In the same manner the above-mentioned great deviation from the average is wrong, and does in reality only come to about 3″. These facts are sufficient. It would be easy to mention many others of similar nature, but I do not think it necessary to take up more room in your esteemed periodical.

W. VALENTINER

Mannheim Observatory, March 10

Experiment with a Vacuum Tube

A TUBE like a radiometer tube contained a concave metal disk within the bulb; this disk could be connected with the pole of an induction-coil, and about a quarter of an inch above it was a small wire which could be connected with the other pole. The bulb was exhausted to such a point that a 5-inch spark could not pass from the wire to the metal disk. The wire and disk were connected for several minutes with an induction-coil giving 4½ to 5-inch sparks; although no spark passed between them, the glass on the side of the bulb, which was *just in the focus of the metal concave disk, was melted*, and the pressure of the external air forced the melted glass inward, and a minute hole was formed, of course destroying the vacuum. The diameter of the hole was about that of the finest sewing-needle; it was in the centre of a depression in the side of the bulb about a tenth of an inch in diameter. H. ALFRED CUNNINGTON

Devizes, February 28

[Having succeeded in melting platinum by the heat of molecular impact (*Proc.* Roy. Soc., No. 191, p. 110, and NATURE, vol. xix. p. 137), it is not surprising that the heat is sufficient to melt glass when the focus falls on it. In a paper communicated to the Royal Society in November last, now being printed in the *Philosophical Transactions*, I mentioned that by drawing the focus on to the side of the glass tube by means of a magnet, the glass became heated to redness. In December I wrote to Prof. Stokes that I had melted up a piece of Gorman glass in the focus of the rays, and at the same time I sent a piece of the melted glass to my friend Mr. Sorby, of Sheffield, for microscopic examination, as the fusion *in vacuo* had produced an unusual appearance on the surface of the glass.—WILLIAM CROOKES.]

Tides in the Bay of Fundy

HAVING resided for some years in the neighbourhood of this bay, I am able to give a little information respecting its tides. The bay splits into two at its inner end. One of these branches leads through a narrow channel into the broad basin of Minas. The other, called Chegnecto Bay, is not interrupted by any such contraction, and is therefore more favourable for the formation of very high tides. This bay itself divides at its upper end into two, and one of these, called Chepody Bay, contracts very gradually for some thirty miles inland, forming the estuary of the Petitcodiac River. This is the place where the highest tides occur, and as far as I have been able to learn, their maximum height is 70 feet. A powerful "bore" is formed by the incoming waters. The captain of the steamer *Emperor*, which plied between St. John, N.B., and Windsor, N.S., informed me that the highest tide in any part of Minas Basin was about 55 feet. This would probably be at the head of Cobequid Bay, near Truro. Noel Bay, which is mentioned in Dr. Haughton's letter (NATURE, vol. xix. p. 432), is in Minas Basin, rather more than half way from its narrow mouth to the head of Cobequid Bay. If the range here at ordinary spring tides is 50·5 feet, any one looking at the map and knowing the effect of funnel-shaped estuaries, would be prepared to learn that there is a range of from 60 to 70 feet in Chepody Bay and the estuary of the Peticodiac, at strong springs. J. D. EVERETT

Malone Road, Belfast, March 14

End-on Gas-Vacuum Tubes in Spectroscopy

WHILE nothing will give me greater pleasure and confidence in my own worked-out views than to learn, as you intimate in the editorial note (NATURE, vol. xix. p. 400), that so able a working scientist as Dr. Van Monckhoven had preceded me in pointing out the value of *end-on* gas-vacuum tubes, and had sent specimens similar to mine to several observers in England, allow me to inquire where I can find any published account in this country of his tubes, the parties to whom they were sent, and the work accomplished with them? And why, also, if the said tubes were found by those gentlemen as intensely superior for spectroscopic results as mine are proving themselves—they have not yet been described in any of the latest London books I have been able to look into on spectroscopy, natural philosophy, electricity, and instrument-makers' price lists, though the old, pale, imperfectly-lighted, transverse-vision tubes are referred to in all?

Your obliging answer to these questions will evidently be of interest to Dr. Van Monckhoven, as well as myself, while it will also have a far wider and more important bearing for many persons in Scotland. For they, conscientiously striving by all recognised public methods of study to keep up with progress in the south, and not having heard of *end-on* gas-vacuum tubes for the spectroscope before my recent paper on them, would very much like to have thereby and therein a practical demonstration of what a thing, and a good thing too, being, as you say of this, "already well known in England," really consists in; and to what extent, therefore, every member of the community here ought to have similarly known it on the 10th inst., and myself nearer to the same date in 1878, when M. Salleron made the first examples for me, on my then supposed new idea. PIAZZI SMYTH

Edinburgh, February 28

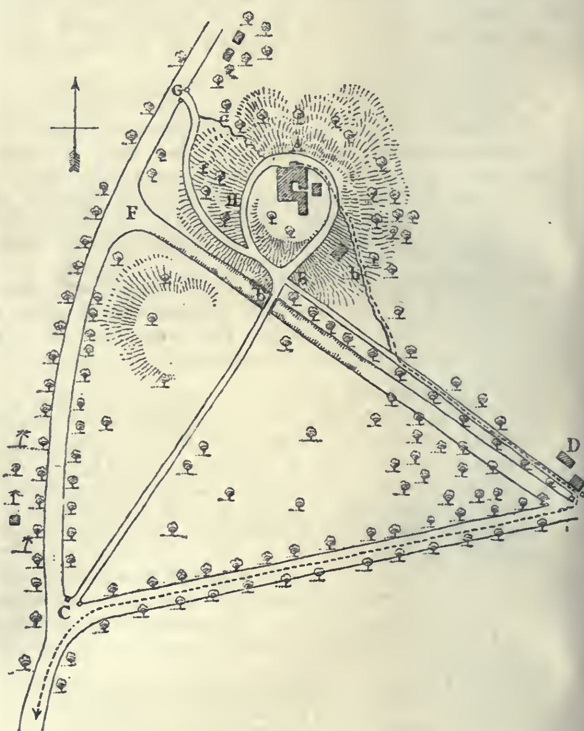
[Dr. Van Monckhoven writes that his new tubes were described to the Belgian Academy of Sciences in 1877, in a note, a copy of which he sends us. He states that he sent some of these tubes to Mr. Dallmeyer, who gave them to various English men of science. They give, he states, about 100 times more light than the ordinary spectrum tubes.—ED.]

Intellect in Brutes

DR. RAE has so fairly disposed of Mr. Henslow's examples of so-called "practical" and "abstract reasoning" that further comment is unnecessary. As, however, the subject of intellect in brutes is on the *tapis*, I will give an instance of sagacity in a dog that finally set at rest any doubts I ever entertained that the difference between human and animal intelligence is one of degree only.

If you have space for it, the accompanying plan will be of great value in describing the circumstances.

Mr. J. W. Cherry, of the Madras Forest Service, was owner of the dog in question, a bull terrier, called "Bully." We



lived in the bungalow (A), the compound of which was bounded south and west by public roads (D C) and (G F C) both leading to the cantonment of Mangalore in the direction C. There were three gates into the compound at (C) (D) and (G), the main approach to the Bungalow leading over a bridge (B), that spanned a branch public road (F D). The compound was filled with trees and shrubs, and bordered by dense lantana hedges, so that with the exception of a portion of the western road at F, neither of the cantonment roads were visible from the bridge, nor could the foot-paths (*a*) and (*b*) be seen thence.

Now Bully had a lady friend (canine) living in the canton-

ment, and at times she was so attractive that absences without leave on the part of the dog were frequent. After one of these excursions Bully had been brought back, and chained up for the night. Next morning, while his master and I were sitting at early breakfast, it was decided that he should be released, and to effectually stop further delinquency, a peon was sent down to the bridge with orders to intercept him if he started for the cantonment.

Bully was brought in and unchained; he had that unmistakable air of detected guilt deservedly punished, and spent some time in begging for scraps from the table in a most deprecating manner. Shortly, however, he strolled into the verandah, and then down the front steps on to the gravel walk. After wandering about aimlessly for a few minutes, he quietly started off down the approach (A H B). We followed, keeping out of his sight. At the turn of the road Bully met the unexpected apparition of the peon standing on the bridge. In a moment, though not a word was spoken by the man, the dog turned and came straight back to the room, whither we had in the meantime slipped back unobserved, and re-entered it wagging his tail violently and looking exceedingly sheepish. He now lay down and closed his eyes. The cocked ears showed that sleep was mere pretence, and he soon rose again, went out into the front garden, and hunted for buried bones, purely imaginary ones, I believe. His search gradually led him down the hill by the foot-path (*a*),—we keeping him in sight, as before—and he finally reached the road at the bottom. There all disguise was dropped, and he started off for the cantonment. As he neared the spot (F) the peon espied him, and shouted out his name. He turned at once, climbed the hill, and came into the bungalow, where the same farce of repentance was gone through.

Bully now seemed to have made up his mind that escape was impossible; he lay down on a mat in the verandah, and remained there for a long time. But for the persistent cock of the ears we should have imagined the animal really asleep. Mr. Cherry eventually went to his office-room, and I remained in the verandah reading the morning paper, and occasionally glancing at Bully. He lay very still, but once or twice I detected him opening his eyes and raising his head to look round him. Each time he caught my eye he wagged his tail vehemently for a moment or two, and then resorted to his sham sleep.

It may have been for half-an-hour, or thereabouts, that this state of things continued. I then became interested in an article in the paper, and when I next looked up Bully was gone. I called Mr. Cherry, and the house was searched. No Bully. The peon was sent for and interrogated; he had not seen the dog. As a last resource inquiry was made of the horsekeepers down at the stables (D). The reply was—"Yes, the dog had passed through the gate (D) some time before." Taking advantage of my occupation and the absence of his master, Bully had left the house, and taken his way to the cantonment by the only path by which he could have escaped unnoticed by the peon—that shown by the dotted line.

In this necessarily short account I have hardly done justice to Bully's diplomatic powers, but most of your readers will appreciate the intelligence that led the dog to successfully elude the watch set over him.

E. H. PRINGLE

A SMALL English terrier belonging to a friend has been taught to ring for the servant. To test if the dog knew *why* it rang the bell he was told to do so whilst the girl was in the room.

The little fellow looked up in the most intelligent manner at the person giving the order (his master or mistress, I forget which), then at the servant, and refused to obey, although the order was repeated more than once.

The servant left the room, and a few minutes afterwards the dog rang the bell immediately on being told to do so.

Royal Institution, March 14 JOHN RAE

OBSERVING the remarks of Mr. G. Henslow (NATURE, vol. xix. p. 433) in reference to "abstract reasoning" as not to be observed in the lower animals, it has occurred to me that the following facts may have a useful bearing on this subject:—My sister, who lives just opposite to my own house, possesses a cat (now about thirteen years old) whose intelligence is very remarkable. He has the habit of making use of the knocker of a side door, which is just within his reach as he stands on his hind legs, whenever he desires admission. A single knock is tried in the first instance, but if this is not answered promptly it is followed by what is known as a "postman's knock;" if this is not successful, trial is then made of a scientific "rat-tat" that would not disgrace a west-end footman. I should say that "Minnie" holds the knocker in his paw as we should hold it in our fingers, and not by simply tipping it up. How far this practice involves "abstract reasoning" I will not say, but something like an approach to it is suggested, for he was never taught to knock at the door, and adopted the habit some three years ago, evidently to gain admittance, very often to the annoyance of my sister's family, who have occasionally been disturbed in this way at unseemly hours. I should be sorry in thus referring to the sagacity of poor pussy (who is now also somewhat feeble) to reflect upon him by noticing some other of his peculiarities, one of which is his fondness for a little brandy and water and other alcoholic stimulants; but I think what I have referred to may be interesting to Mr. Henslow or some other of your correspondents, and it is within my own knowledge and observation. G. M.

March 15

MR. HENSLOW asks for "cases of purely abstract mental reflection in animals," and in reply I mention a case in Somersetshire of a kitten about half grown, at a house where I was stopping, having mental reflection of some sort.

I was sitting in one of the rooms, the first evening there, and hearing a loud knock at the front door, was told not to heed it, as it was only this kitten asking admittance. Not believing it, I watched for myself, and very soon saw this kitten jump on to the door, hang on by one leg, and put the other fore-paw right through the knocker and rap twice.

The knocker was an ordinary-shaped one fixed in the centre of the door, half way up; the top part of the door was glazed. I saw this performance dozens of times afterwards, and often used to put the kitten outside to see it done. It was never known to knock when any one stood in the garden, but if one went in-doors and shut it outside, in a few minutes came the usual knock.

A sister kitten to this one was never known to knock, but sat on the doorstep and entered when the door was opened, and in nine cases out of ten the knocks were successful.

This kitten was never taught in any way; it would knock at both front and back doors. I should like to know if Mr. Henslow considers this practical or abstract reflection; the result was *practical*. MAURICE BELSHAM

Simla Cottage, Barnes

THE explanations by Mr. Nicols (NATURE, vol. xix. p. 433) fail to convince me that the rats cut the pipes to get at the water. I have seen the edge of joists cut or gnawed about eight inches above the ground, where the rat would have to stand on its hind legs to do it—What was that for? Again, why does our cat scratch the legs of the kitchen table? It seems to me that rats are often like children, they must be doing something to work off the energy within them, and fill up the time, and they often do things without any definite reason. Lastly, if the water is at high pressure especially do they stop to drink the water at all? It also runs in my mind that the rats cut the lead pipes where there was plenty of clean water without doing so.

Glasgow, March 17 W. P. BUCHAN

I BEG to thank Mr. Nicols for his courtesy in supplying the missing links of evidence in the rat cases (NATURE, vol. xix. p. 433), cases which may, I think, be applied with reference to Mr. Henslow's difficulty concerning "abstract mental reflection;" for it seems to me now that the most probable supposition is that the rat-community had learned through experience (likely got accidentally in cutting pipes which obstructed their operations) that such-like pipes at times contain water, and by exercise of reason came to the conclusion that it was worth while to make the exploration in the instances given.

I give the following as told me by my wife—now dead—who personally witnessed the transaction on various occasions:—At her sister's house in Kent a donkey which, when not employed by the children, grazed in a field with some cows, was in the regular habit of acting as follows:—At the usual hour for the cows to come home to be milked the donkey lifted the latch of the field gate, opened and held back the gate (which would otherwise have swung close again) till all the cows passed out, then allowed the gate to shut, and went home with the cows. Of

course no one taught the donkey to do this; but the quadruped gave the biped a practical lesson, from which I am not aware that they drew the abstract verbally formulated conclusion that reason may be exercised without rhetoric.

March 14 HENRY MUIRHEAD

I BELIEVE that instances of rats gnawing through water-pipes are frequent. Two have come to my knowledge during the past fortnight. The one instance occurred at the house of a gentleman near West Hartlepool; in the other case a large hole, 3¼ inches long, and varying from 7/8ths of an inch to 1⅛th inch in breadth, was gnawed in the fresh-water pipe of the screw-steamer *Mary Coverdale*. A portion of this pipe, containing the hole, was cut off, and is preserved by me; it is a stout leaden pipe, a quarter of an inch thick, and with a diameter of 2⅝ inches. It is very doubtful whether there was any flaw before the hole was begun.

R. MORTON MIDDLETON

West Hartlepool

Distribution of the Black Rat

PERHAPS some of the readers of NATURE may be able to throw some light on the present geographical range of the Black Rat (*Mus rattus*, L.). In the early part of 1877 some individuals of this species came on board the steamship *Lady Frances* either at Bombay or at Rangoon, but, as the captain believes, at the latter port. The animals multiplied on board the vessel, and in August last I had the pleasure of receiving from the ship a living specimen, which was at once forwarded to the Zoological Gardens in Regent's Park, where, I believe, it may still be seen. In a "Catalogue of the Mammals of the Sahara," by my friend, Canon Tristram, F.R.S. (*vide* "The Great Sahara," p. 385), the author states that the "Far el Kla," as the black rat is called by the Arabs, "still maintains its position" in the Algerian Sahara. And I was yesterday presented by Mr. F. Donald Thompson, of Seaton-Carew, with a skin of *Mus rattus* from New Zealand. This example, like those from Burmah, was brought over by a vessel (the *Trevelyan*) which loaded grain at Lyttelton, in the province of Canterbury, New Zealand, where the rats embarked. In August, 1878, Dr. Sclater, F.R.S., was good enough to inform me that "*Mus rattus* has rather an extensive range over Europe and Western Asia," and added, "I fear it would not be possible to state it very exactly." But it is evident that the range of the species is much wider, as it is known to occur in North Africa, British India, and New Zealand; and it is also said, by Prof. Bell and Mr. Macgillivray, to have been carried to America and the South Sea Islands by ships. I should be glad to have further evidence as to its occurrence in Burmah, and it would be also desirable to know if it is found in the Malay Archipelago, China, Japan, or Australia. Dr. Peters, of the Zoological Museum at Berlin, assured me, in June last, that the species was extremely rare, if not actually extinct, in Germany, and showed me the only specimen in the fine collection under his care—an old and faded skin from Hanover. The animal lingers in one old building at Stockton-on-Tees, and there is clearly a possibility of its being reintroduced in many seaport towns through the agency of ships.

West Hartlepool, March 11 R. MORTON MIDDLETON

The United States Fisheries

IN your review of the report of the United States Commission of Fish and Fisheries, you say you are of opinion there is almost no difference between *Salmo salar* and *Salmo quinnat*. My friend Prof. Baird sent me his report some time since, and also forwarded several thousand eggs of *Salmo quinnat* for experiment in the hatching tanks of the Southport Aquarium. The eggs hatched out remarkably well, a very small percentage only being lost, and have proved much more hardy and tenacious of life than any *Salmo salar* I ever had to do with, and very much easier to feed. *Salmo salar* have never done well except when fed on the minute red worms found on the mud in the beds of some rivers and streams (our supply was obtained from the Thames). *Salmo quinnat*, however, live well, and grow faster on the roe of fish (refuse from the fish market), such as whiting, than *S. salar* will on anything. From what I have seen of them I quite agree with Prof. Baird in his admiration of this member of the salmon family, and I share his surprise that it has attracted so little attention among English fish-culturists. It would certainly be a most valuable addition to our food-fishes, stronger, and apparently of more rapid growth than our native species. On the continent, and in New Zealand and other countries, it is most greedily sought after, and each season for several years past an agent has carried from America to France, Germany, and other countries, large consignments of the ova. In England, so far, it appears to have been quite neglected.

Hill Fold, Bolton, March 15 CHAS. L. JACKSON

Plovers in the Sandwich Islands

I CAN vouch for the truth of the visit of golden plovers to the Sandwich Islands mentioned by Prof. A. Newton in NATURE, vol. xix. p. 433. They are very numerous during the winter from November until March. I do not know the scientific name, but I have shot a great many on Oahu and Hawaii.

If it will help Mr. Newton in the solution of the very interesting question he raises I may mention that M. Baillière, Consul-General for France at Honolulu, is in the habit of sending specimens of birds to (I think) the Jardin des Plantes, Paris, where doubtless a specimen might be found.

S. LONG

Hertford, March 15

Unscientific Art

IN the *Graphic* for December 28 there appeared a sketch of a man taking a reading on a marine barometer, on board the *Sarmatian*, during the voyage of the Marquis of Lorne to Canada. To see the scale better by the light of his lantern, the observer is represented as sloping the barometer at an angle of about 30° from the vertical.

JOHN W. BUCK

New Kingswood, Bath

ON THE POSSIBILITY OF EXPLAINING THE CONTINUANCE OF LIFE IN THE UNIVERSE CONSISTENT WITH THE TENDENCY TO TEMPERATURE-EQUILIBRIUM

THE idea of the ultimate final cessation of all physical change and life in the universe[1] has been contemplated by many physicists with some dissatisfaction, and with the desire if possible to find some explanation or physical means by which so apparently purposeless an end is averted, and of avoiding the necessity for assuming in past time a violation of physical principles at present recognised to exist.[2] Several attempts have been made to surmount the difficulty,[3] but apparently with no generally satisfactory result. Having given much time to physical problems having a relation more or less to this question, and having always kept the question itself in view, I should like to submit the following conclusion to the readers of NATURE as an attempt to solve the difficulty, though what I have to bring forward is probably not entirely new, as considerations partially tending towards the same final result have already been published by Mr. James Croll, *Phil. Mag.*, May, 1868, "On Geological Time;"[4] and Mr. Johnstone Stoney, "On the Physical Constitution of the Sun and Stars," *Proc.* of the Royal Society, 1868-69. The groundwork of what I have to suggest may be described in a few words.[5]

Taking a general view of the universe, we may consider it as so much matter, which contains a certain quantity of energy. Let us suppose for illustration the energy of

[1] Thomson, "On the Universal Tendency in Nature to the Dissipation of Mechanical Energy," *Phil. Mag.*, October, 1852; Clausius, Ninth Memoir, *Pogg. Ann.*, July, 1865; see also Tait, "Recent Advances in Physical Science," second edition, p. 22.

[2] The allied idea of the whole universe tending to agglomerate into one mass under the action of gravity, the notion of instability thus involved, all this has something incongruous and unnatural about it that appears to be scarcely in harmony with the orderly working of physical phenomena, and would seem to point to the necessity for some additional explanation.

[3] Grove, "Corr. of Physical Forces," p. 67; Rankine, "On the Reconcentration of the Mechanical Energy of the Universe," *Phil. Mag.*, November, 1852, &c., &c.

[4] Also *Quarterly Journal of Science*, July, 1877.

[5] The same problem was considered by the writer in special reference to Le Sage's theory of gravitation in the *Quarterly Journal of Science* for July last, but my present object is to deal with the question entirely independently of any special theories, and solely on the basis of generally accepted facts, or facts which if not known would be in harmony with or deducible from those which are known.

this matter to be raised to such a degree that the whole forms a gas, consisting of separate or dissociated molecules, filling space uniformly. This would evidently be the result of applying sufficient heat, just as for example a gas consisting of compound molecules breaks up into its elementary molecules when sufficient heat is applied, the molecules being unable to aggregate into groups on account of the expansive action of the high temperature. When the temperature of the gas is lowered the molecules (as is known) commence again to aggregate into groups, *i.e.*, to cluster about common centres in chemical union. So in the case of the universe, in the imaginary instance of an (adequately) extremely high temperature, we should have the entire universe consisting of separate molecules or forming a very rarefied gas, the molecules being unable to aggregate into distinct groups under the action of gravity, owing to the enormous velocities of the molecules. The molecules would simply rebound from each other in straight lines, according to the principles of the kinetic theory of gases.[1] Let us suppose, now, the excessive temperature to fall, or in other words the total energy to diminish. Then the molecules would commence to cluster into groups (forming masses) under the action of gravity, the mean size of such aggregated groups of molecules becoming greater as the temperature is less, and the number of such groups diminishing in the same proportion. At first, by a slight fall of temperature we should have a large number of small groups (or clusters) of molecules; by a further fall of temperature a further clustering of molecules under the action of gravity would occur, *i.e.*, the size of the separate masses would increase and their number diminish. The case is, in broad principle, exactly parallel to that of a compound gas when subjected to extreme variations of temperature, indeed as far as the purely mechanical considerations are concerned, it is only a question of scale.[2] We know that when a compound gas whose molecules possess a high complexity has been heated up to the temperature of dissociation, and the temperature is gradually lowered, then at first only a clustering of elementary molecules takes place; but as the temperature is further lowered, these compound molecules may cluster together to form compound molecules of a secondary order or higher degree of complexity (*i.e.*, molecular clusters of a larger mass). Thus the mean mass of the clusters of molecules in the gas increases as the temperature is lowered, and the number of such clusters (or centres of aggregation) diminishes correspondingly.

We will therefore suppose that the universe has attained a final state analogous to this, *i.e.*, such that the mean mass of a cluster of molecules (a stellar mass) and the number of such clusters (stellar masses) is such as exactly to represent that which must exist by the actual mean temperature of the universe. But it may be said, as far as we are able to appreciate and judge of the universe, it certainly appears as if the entire universe were losing its heat in the ether of space, and that this final state of things (equilibrium) were not yet attained. But it may be urged, in reply, we are judging of the entire universe from the point of view of a single stellar sun to which we belong. It is as if we were to judge of the temperature equilibrium of a gas from the point of view of a single molecule (or of a few others surrounding it), in which case it is certain we should be unable to form an idea of the state of temperature equilibrium of the gas. It is known to be a demonstrated consequence of the kinetic theory that the utmost diversity exists among the velocities of the molecules of a gas, or the temperature from molecule to molecule. In order to have a true idea of the state of temperature of the gas, we must investigate the conditions of a region containing some thousands of millions of molecules (any appreciable region or space actually containing this number). So in order to have an adequate idea of the state of temperature equilibrium of the universe, we should require the mean temperature (state of energy) of a region containing some thousands of millions of stellar masses, not the narrow view we have from one of these, and the velocities of the few we have measured—not to speak of the countless dark suns that may exist in space, and about whose velocities we know nothing. Mr. Croll has pointed out[1] how it is probable that such dark suns may possess exceptionally high velocities, as the bright (visible) suns would naturally have lost in the collisions which developed their heat part of their normal velocity of translation, the translatory motion having been partly lost by conversion into heat. In the parallel case of a gas, it is a known fact that even if the mean temperature of the gas be low (less than normal temperature), some molecules in certain parts must acquire in the accidents of collision enormous velocities, and are thrown into very forcible vibration at the encounters, such that they would become luminous if we were able to visualise single molecules. In other words, if *all* the molecules of the gas possessed the velocities of these single molecules (relatively few in number), the entire gas would appear like a flame. So in like manner, though single stars in the universe may be luminous, it (by analogy) by no means follows that this at all approximately represents the mean condition of the entire universe. This luminous state might be quite exceptional, and the mean temperature of the universe might be exceedingly low for aught we may know. We may happen to be in a part where the mean temperature of the component matter is exceptionally high, as, of course, from the fact of our being in existence, we must be in a part which is suited to the conditions of life. What is there, then, to oppose the inference that the mean temperature of the universe may be such that each stellar mass (or detached portion of matter, glowing or not) on an average receives as much heat from others as it emits itself, in analogy to the molecules of a gas in equilibrium of temperature; and this does not prevent single stellar masses (in analogy single molecules of the gas) from acquiring exceedingly high temperatures, indeed, they would naturally acquire this from the encounters in certain instances, according to the accepted principles of the kinetic theory.

As regards the state of aggregation of the matter of the universe as dependent on the energy, it would clearly in the same way be misleading if we were to attempt to judge of the mean state of aggregation from the point of view of the few masses in our immediate neighbourhood (or the narrow range of the universe overlooked by us). Thus, to recur to the smaller scale illustration of a gas. In the case of the molecules of a compound gas in a state of temperature-equilibrium, it is known that some of these compound molecules (representing a cluster of molecules aggregated about a common centre) must acquire now and then, in the accidents of collision, velocities corresponding to dissociation temperature. The compound molecule is thus broken up into its components at the collision, these components clustering together again in some other part of the gas, the *mean* state of aggregation remaining unchanged. Thus it would evidently be misleading to judge of the state of aggregation of the molecules of a compound gas from the point of view of an inappreciable region, containing a few hundred thousand molecules, which might in the accidents of collision have become exceptionally heated. In order to judge of the state of aggregation of the gas, we must investigate that of an appreciable region, containing some thousands of millions of molecules. So in the case of the universe, it

[1] The deflection from a straight line owing to the feeble action of gravity in the case of single molecules, would evidently be inappreciable.

[2] There is, of course, this detail of difference, viz., that while the aggregation of molecules about a centre in chemical action is limited, the aggregation in the case of gravity is unlimited. We merely apply in principle the same general considerations to molecules aggregated into clusters (lumps) under chemical action, as to molecules aggregated into lumps under gravific action (stellar masses).

[1] *Quarterly Journal of Science*, July, 1877.

would obviously be fallacious if we were to form an estimate of the general state of aggregation from that of the few masses we can judge of in our immediate vicinity; but we should require to know the condition of a region of an extent that we have no chance of overlooking, and under the principles of the kinetic theory, the local variations of the states of aggregation (themselves depending on local variations in the velocities of the masses) would fluctuate within wide limits. In order to have an idea of the actual (mean) state of aggregation, a being would be required that could (on comparative scale) sweep over the universe with the same facility as we sweep through or examine regions in a gas representing a multiple countless millions of times that of the mean distance of the detached portions of matter composing the gas.[1]

We are led to apply the principles of the kinetic theory to the case of the universe not so much as a speculation, but rather as a necessary deduction following from the known principle that detached masses moving freely in space (as the stellar masses are observed to do) and at such distances apart that gravity between the several masses is incompetent to deflect the path of the masses appreciably, must move in straight lines, and have their motions regulated under the mutual encounters in accordance with the principles of the kinetic theory. Only in the relatively near approach of the masses to one another does gravity come sensibly into play and deflect the path, causing under certain conditions rotation about a common centre (double stars), or, perhaps, by almost direct impact, nebulæ with but feeble rotation, &c.[2] To carry the analogy again to the smaller scale-case of a gas, it is there known that the molecules are in some cases feebly impelled towards each other at a near approach, the path of the molecule being thus deflected at its termination, whereby the conditions are given for causing a temporary rotation of the pair of molecules about a common centre, in an analogous way. The relatively vast distances of the stellar masses, compared with their dimensions, would involve, as a rule, an extremely long mean path before the encounters, corresponding to a proportionally long epoch of time adapted to the conditions of life. The apparent extreme simplicity of the means to the end by the application of the kinetic theory to the case would at least seem not to be out of harmony with its truth.

Thus the final conclusion to which these considerations lead would be that the universe has attained its final state of temperature equilibrium (if we set no fixed limit to its past existence), in the sense that if we were able to measure the temperature (or contained energy), of a sufficient number of masses through a sufficiently extensive region, we should find that in every such *equal* region throughout the universe the temperature (or contained energy) would be the same; just as (on a smaller scale) in the case of a gas, if we could measure the temperature of some thousands of millions of molecules in a given region, we should find that though the temperature differed to a practically unlimited extent from molecule to molecule, yet the temperature of every such equal region was the same.

[1] Just as in the case of a compound gas, the *uniformity* of temperature, of states of aggregation, &c., does not apply to the individual unit lumps of matter (molecules) forming the gas [which may be in vastly different states from one to the other]—but to unit *volumes* containing vast numbers of such units; so in the case of the universe, the uniformity of temperature, state of aggregation, &c., would not apply to the unit lumps of matter (stellar masses) but to unit volumes. In fact the universe may be regarded as a larger scale gas, with the difference that the central force producing the aggregated lumps of matter that move as wholes is not chemical action but gravific action. If we imagine (merely for illustration) a being on relative scale situated on a single compound molecule of a gas in a state of normal temperature equilibrium; this minute being would observe vast differences of temperature and of states of aggregation around him (some molecules in scattered parts glowing in a state of dissociation, &c.), and he would form a perfectly wrong judgment of the state of the gas from such a narrow point of view. So the observer connected with one unit lump of matter of the universe (stellar mass) can form no idea of the state of the rest from his narrow point of view.

[2] The occasional flashing out of stars, as if due to some sudden convulsion that might be referred to collision as a suitable cause, is a notorious fact in astronomy; though, from the extremely limited view of the universe that we possess, it would be unreasonable to expect such phenomena to be of frequent occurrence.

So in an analogous way as regards the *state of aggregation* of the matter of the universe, since this depends on the temperature, it would follow, assuming an indefinite past time, that the mean state of aggregation of the matter, like the mean temperature (mean energy), is the same throughout, *i.e.*, the average size of the separate masses, or the number in unit of volume (taking sufficiently large units of volume for comparison) would be equal throughout, though indefinite fluctuations of dimensions would occur from one mass to another, in analogy with the fluctuations of velocity from one mass to another.

It would further follow from the known principle that molecules of different densities (molecular weights) tend forcibly to become uniformly diffused, that by an indefinite past duration of the universe all the matter must be uniformly diffused if (as in the case of uniform velocity and uniform state of aggregation) regions of sufficient extent could be taken for relative comparison. This again resembles in principle the smaller scale case of a gaseous mixture, where it is known that the small detached portions of matter (molecules) are uniformly mixed, only when appreciable regions containing vast multitudes of molecules are examined, but that there is room for considerable local fluctuations of mixture (such as if only a few hundred thousand molecules were examined).

Thus it appears that the kinetic theory, applied to the universe, would have the peculiar characteristics of allowing almost indefinite local fluctuations of temperature, of states of aggregation, and of composition, of the matter forming the universe within regions very extensive, absolutely speaking, but infinitesimal, relatively speaking (*i.e.*, in comparison with the boundless universe), these regions being amply extensive enough to allow an amount of activity and variability of energy adapted to the conditions of life; while at the same time the principles of the theory, from their very nature, involve perpetually recurring and yet indefinitely variable changes within certain localised limits, the constitution of the vast whole (looked at broadly) remaining uniform throughout.

S. TOLVER PRESTON

FRITZ MÜLLER ON A FROG HAVING EGGS ON ITS BACK—ON THE ABORTION OF THE HAIRS ON THE LEGS OF CERTAIN CADDIS-FLIES, &c.

SEVERAL of the facts given in the following letter from Fritz Müller, especially those in the third paragraph, appear to me very interesting. Many persons have felt much perplexed about the steps or means by which structures rendered useless under changed conditions of life, at first become reduced, and finally quite disappear. A more striking case of such disappearance has never been published than that here given by Fritz Müller. Several years ago some valuable letters on this subject by Mr. Romanes (together with one by me) were inserted in the columns of NATURE. Since then various facts have often led me to speculate on the existence of some inherent tendency in every part of every organism to be gradually reduced and to disappear, unless in some manner prevented. But beyond this vague speculation I could never clearly see my way. As far, therefore, as I can judge, the explanation suggested by Fritz Müller well deserves the careful consideration of all those who are interested on such points, and may prove of widely extended application. Hardly anyone who has considered such cases as those of the stripes which occasionally appear on the legs and even bodies of horses and apes—or of the development of certain muscles in man which are not proper to him, but are common in the Quadrumana—or again, of some peloric flowers—will doubt that characters lost for an almost endless number of generations, may suddenly reappear. In the case of

natural species we are so much accustomed to apply the term reversion or atavism to the reappearance of a lost part that we are liable to forget that its disappearance may be equally due to this same cause.

As every modification, whether or not due to reversion, may be considered as a case of variation, the important law or conclusion arrived at by the mathematician Delbœuf, may be here applied; and I will quote Mr. Murphy's condensed statement ("Habit and Intelligence," 1879, p. 241) with respect to it: "If in any species a number of individuals, bearing a ratio not infinitely small to the entire number of births, are in every generation born with any particular variation which is neither beneficial nor injurious to its possessors, and if the effect of the variation is not counteracted by reversion, the proportion of the new variety to the original form will constantly increase until it approaches indefinitely near to equality." Now in the case advanced by Fritz Müller the cause of the variation is supposed to be atavism to a very remote progenitor, and this may have wholly prevailed over any tendency to atavism to more recent progenitors; and of such prevalence analogous instances could be given.

CHARLES DARWIN

Blumenau, St. Catharina, Brazil,
January 21, 1879

MY DEAR SIR,

If I remember well, I have already told you of the curious fauna which is to be met with between the leaves of our Bromeliæ. Lately I found, in a large Bromelia, a little frog (*Hylodes ?*), bearing its eggs on the back. The eggs were very large, so that nine of them covered the whole back from the shoulders to the hind end, as you will see on the photograph accompanying this letter, Fig. 1 (the little animal was so restless that only after many fruitless trials a tolerable photograph could be obtained). The tadpoles, on emerging from the eggs, were already provided with hind-legs; and one of them lived with me about a fortnight, when the fore-legs also had made their appearance. During this time I saw no external branchiæ, nor did I find any opening which might lead to internal branchiæ.

FIG. 1.

There is here another locality in which a peculiar fauna lives, viz., the rocks of waterfalls, which are of very frequent occurrence in almost all our mountain rivulets. On these rocks, along which the water is slowly trickling down, or which are continually wetted by the spray of the waterfall, there live various beetles not to be met with anywhere else, larvæ of diptera and caddis-flies, and a tadpole remarkable for its unusually long tail.

The pupæ of caddis-flies living on the rocks of waterfalls (I examined three species belonging to the *Hydropsychidæ*, *Hydroptilidæ*, and *Sericostomatidæ* [*Helicopsyche*]), as well as those living in the Bromeliæ (a species belonging to the *Leptoceridæ*), are distinguished by a very interesting feature. In other caddis-flies the feet of the second pair of legs (and in some species those of the first pair also) are fringed in the pupæ with long hairs, which serve the pupa, after leaving its case, to swim to the surface of the water for its final transformation. Now neither on the surface of bare or moss-covered rocks, nor in the narrow space between the leaves of Bromeliæ, the pupæ have any necessity, nor would even be able, to swim, and in the four species living on such localities which I examined, and which belong to as many different families, the feet of the pupæ are quite hairless, or nearly so, while in allied species of the same families or even genera (*Helicopsyche*) the fringes of the legs, used for swimming, are well developed.

FIG. 2.

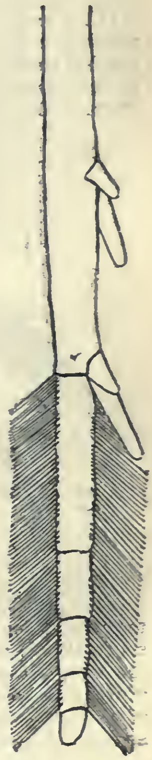


FIG. 3.

FIG. 2.—Tibia and tarsus of the two pairs of legs of the pupa of a species of Leptoceridæ, inhabiting Bromeliæ. FIG. 3.—The same of a nearly allied species inhabiting rivulets.

This abortion of the useless fringes in the caddis-flies inhabiting the Bromeliæ and waterfalls appears to me to be of considerable interest, because it cannot be considered, as in many other cases, as a direct consequence of disuse; for at the time when the pupæ leave their cases and when the fringes of their feet are proving either useful or useless, these fringes as well as the whole skin of the pupa, ready to be shed, have no connection whatever with the body of the insect; it is therefore impossible that the circumstance of the fringes being used or not for swimming, should have any influence on their being developed or not developed in the descendants of these insects. As far as I can see, the fringes, though useless, would do no harm to the species, in which they have disappeared, and the material saved by their not being developed appears to be quite insignificant, so that natural selection can hardly have come into play in this case. The fringes might disappear casually in some individuals; but, without selection, this casual variation would have no chance to prevail. There must be some constant cause leading to this rapid abortion of the fringes on the feet of the pupæ in all those species in which they have become useless, and I think this may be atavism. For caddis-flies, no doubt, are descended from ancestors which did not live in the water, and the pupæ of which had no fringes on their feet. Thus there may even now exist in all caddis-flies an ancestral tendency to the production of hairless feet in the pupæ, which tendency in the common species is victoriously counteracted by natural selection, for any pupa, unable to swim, would be mercilessly drowned. But as soon as swimming is not required and the fringes consequently become useless, this ancestral tendency, not counterbalanced by natural selection, will prevail, and lead to the abortion of the fringes.

I do not remember having seen, in any list of cleistogamic plants, the Podostemaceæ. These curious little aquatic plants, which Lindley placed near the Piperaceæ, Kunth between the Juncagineæ and Alismaceæ, and which Sachs considers as being of quite dubious affinity, cover densely the stones in the rapids of our rivers; on the branches which come above the surface of the water, there are pedunculated, open, fertile flowers; but there are numerous sessile flower-buds also on the branches,

which probably remain submerged for ever; I have not yet ascertained whether these submerged flowers are fertile; if they are so, they can hardly fail to be cleistogamic.

FRITZ MÜLLER

A STUDY IN LOCOMOTION[1]

II.

III. *The Paces of the Horse.*—Every one can recognise whether a horse is walking, trotting, or galloping, and yet few would be able to point out the rhythm and order of succession of the movements of the limbs in different paces. These movements, in fact, succeed each other

FIG. 8.—Registering apparatus for horse's paces.

too rapidly for the eye to follow them, and their rhythmic succession is more readily perceived by the ear than by the eye. It is indeed ordinarily by the ear that we become aware of a horse's pace. When at each return of the step (*revolution du pas*) we hear two distinct strokes of the hoofs, we call it an amble, or a trot; three

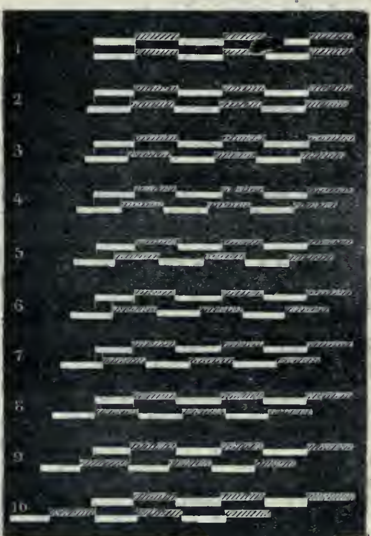


FIG. 9.—Synoptic table of the different paces of the horse, after the classic authors: 1, amble; 5, foot-pace; 8, trot, &c.

strokes unequally separated denote a gallop; lastly, four strokes indicate a foot pace. But these paces may be more or less irregular, variable, or crippled; besides that, when an animal passes in a very short space of time from one pace to another, how shall we decide upon the manner in which the transition is effected? To clear up these points great efforts have been made by horse-

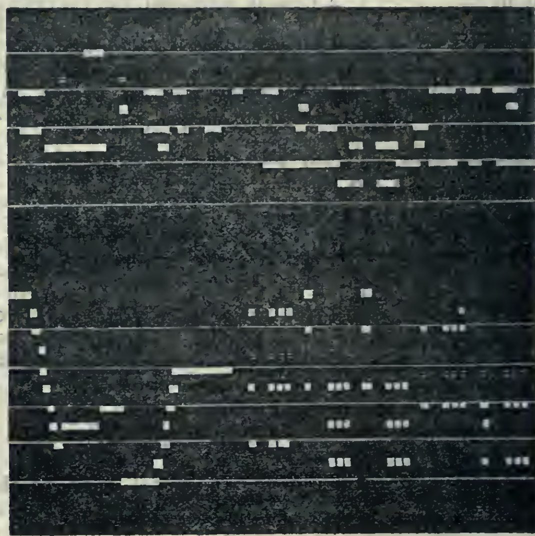
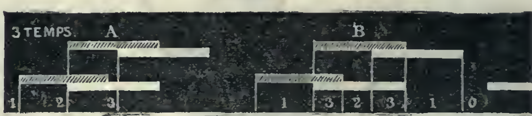


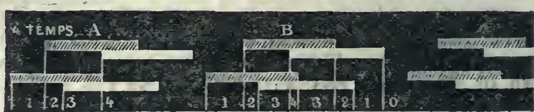
FIG. 10.—Notations of two airs, A and B, executed upon the keyboard of a harmonium.

trainers and veterinary surgeons, to whom the questions involved are of considerable importance.

Now, as I have just said, the ear judges better than the eye as to the rhythm of successive movements, but in order to demonstrate the production of these rhythmic strokes in twos, and threes, and fours, it is essential to know to which foot each separate sound is to be attributed. Ingenious experimenters have attached to the four feet of the horse bells of different tones, but in perfect harmony with each other. Varied melodies or harmonies are thus produced, according to the succession or synchronism of the strokes. But such an arrangement would certainly not give the length of time each foot remained upon the ground, therefore the question of the paces of the horse has not been entirely resolved even by this method. Turn to any special treatises on the subject, and you will see that beyond the amble, the downright



Gallop of 3 steps. A, indications of the three steps; B, indications of the number of feet which form the support of the body at each instant of the gallop of 3 steps.



Gallop of 4 steps.

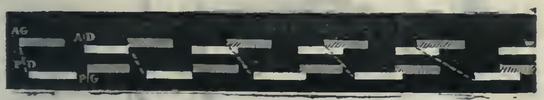
FIG. 11.—Notations of the gallop of 3 and 4 steps.

trot, and the three-step gallop, there is, perhaps, not a single pace respecting which contradictory theories are not held. In face of the difficulties of this problem, you will doubtless foresee what will be my conclusion; it will be necessary to have recourse to the graphic method which will solve the question in the simplest manner possible.

[1] "Moteurs animés; Expériences de Physiologie graphique." Lecture by Prof. Marey at the Paris meeting of the French Association, August 29, 1878.

Let us take up the question at the point to which it has been brought by the experimenters to whom I have just alluded. The succession of the movements of the horse's legs, since it is rhythmical, and since also we produce

Transition from the trot to the foot-pace.



Transition from the trot to the gallop of 3 steps.

Transition from the gallop of 3 steps to the trot.

FIG. 12.—Transitions between different paces.

from each foot a different tone, forms a sort of music. Now this music is very simple, because it is only composed of four tones. The following arrangement will

FIG. 13.—Diagram of the tracks of horses in different paces.

permit us to obtain the notation of this music, written by the horse himself. Under each of the horse's shoes we place a bag (*ampoulle*) full of air, which, by means of a tube is connected with another similar small bag, which, by its changes in bulk, acts upon a pencil. When the horse plants its foot upon the ground, it raises a pencil, which remains raised as long as the foot is kept down. Four pencils are placed in position, connected with the four under the horse's feet, and these pencils placed in a right line parallel with the axis of the cylinder, spontaneously trace the succession and duration of each setting down of a foot.

Fig. 8 shows the arrangement of the apparatus. The four limbs of the horse bear four india-rubber tubes, which converge to the hand of the jockey, and so to the inscribing apparatus on the cylinder which he holds.

The results thus obtained by the different paces are shown side by side to the number of ten in Fig. 9. Each foot, as in musical notation, has its characteristic sign, by the height of which the symbol of its stay on the ground is traced. We will agree that the fore feet shall mark upon the lines above, the rear feet upon the lines below. Now the horse can, with his four feet, execute the most rapid movements, and yet nothing will be wanting from the inscription. And in the first place, in order to give you confidence in the employment of this method, let us show you how it solves a problem still more difficult. When a clever pianist passes his fingers over the keyboard, who could describe the movements that he executes, say what note has been touched first, and for how long, then what notes followed, together or separately, with their rhythms and tones? But let us write down these movements, and when they are fixed upon paper we can analyse them with ease.

In the apparatus which I use, the keys act by pressure upon the bags of air connected by tubes with other bags, which cause pencils to move. See with what ease these little pencils reproduce all the movements of the fingers passing over the mute keyboard; you will presently see the instrument in operation. I present to you, first of all, what it has just written (Fig. 10). Observe these notes drawn up in gamuts and arpeggios, these varied accords, these changes of tone where sharps and flats reveal themselves by strokes drawn longitudinally. This instrument, which is now in action for the first time, has been constructed by our colleague, M. Tatin, whose skill has already excited the admiration of all who know him.

And now that you no longer doubt, I trust, the faithfulness of the method, let us analyse the tracings on the diagram I have just now shown you (Fig. 9). In order to understand it fully, let us borrow the ingenious idea of Dugès, and compare the horse or any other quadruped to two bipeds walking one behind the other. If the two walkers execute the same actions at the same time, that is to say, if they both simultaneously raise and set down the right foot and then the left, it is the *amble* (No. 1) which is produced. The ear only detects two sounds at each combined step, because two feet touch the ground together. This is an example of *lateral bipedal* movement. If the rearmost walker has half finished the resting of one of his feet when the foremost walker plants the same foot on the ground, it represents the *foot-pace.* Here the four footsteps are separate, and the ear distinguishes four equidistant sounds; the order of succession would be (commencing to count by the right foot), *foremost right*, *rearmost left*, *foremost left*, *rearmost right.* Let us imagine that the rearmost walker makes movements absolutely the reverse of those of the walker in front, that is to say, that one of the right feet strikes the ground when the other right foot rises from it, we shall then have the trot. Two feet will be always associated and will give but one sound, and these feet will exemplify *diagonal bipedal* motion.

Such, then, is our knowledge of the rhythm of paces, or at least the points upon which all are agreed. But if we desire to gather from divers authors the definition of more complicated paces, intermediary to those we have

just described, we shall only find, as I have before stated, a number of contradictions between writers holding different views. In the most rapid pace, the gallop, that of three steps,

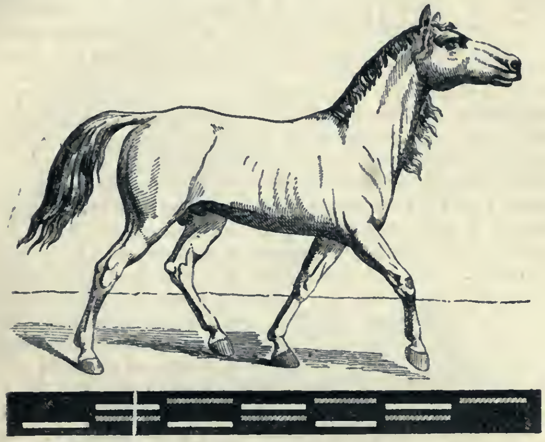


FIG. 14.—Trotting horse represented at the moment of diagonal support.

for example (Fig. 11), the first step is with the hind-foot, upon which the horse falls back after it has raised itself

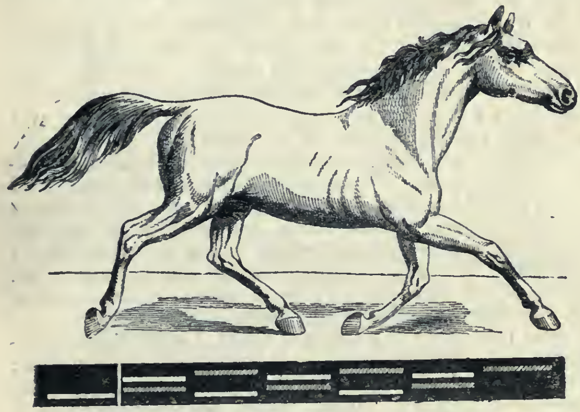


FIG. 15.—Trotting horse at the moment of suspension.

from the ground; then the other hind-foot and fore-foot which is associated with it diagonally fall together, and

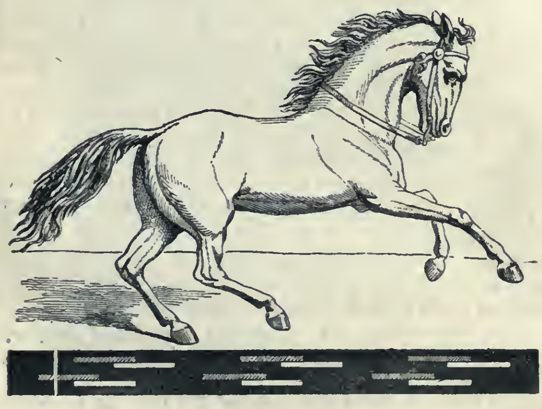


FIG. 16.—Horse at a gallop of three steps. Moment of the first step.

form the second step. Lastly, one hears the other fore-foot fall, and this is the third step.

The gallop of three steps may be distinguished in two forms; the right-hand gallop, in which the right-hand

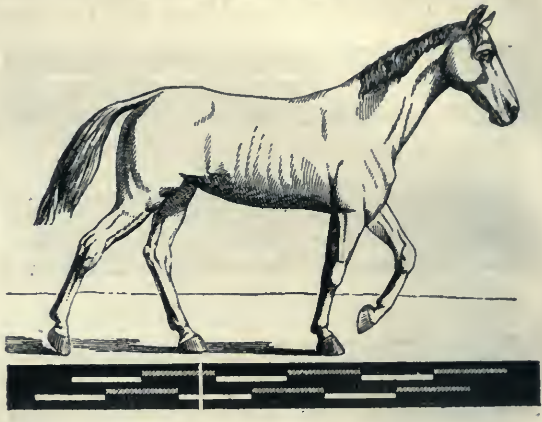


FIG. 17.—Foot pace, with effort of traction.

hind foot reaches the ground last; the left-hand gallop, in which the left fore-foot is the last to reach the ground.

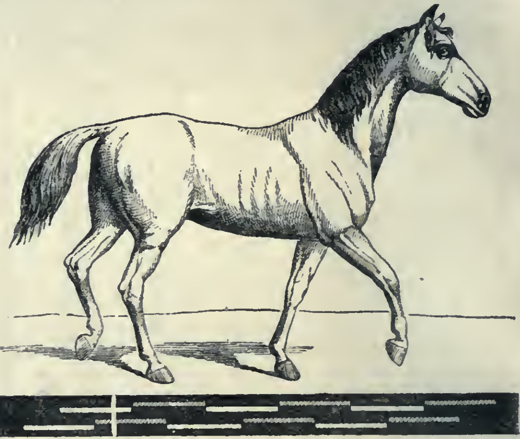


FIG. 18.—Foot pace, moment of diagonal support.

Do we desire to know upon how many feet a horse is supported at different moments of the gallop? Fig. 11

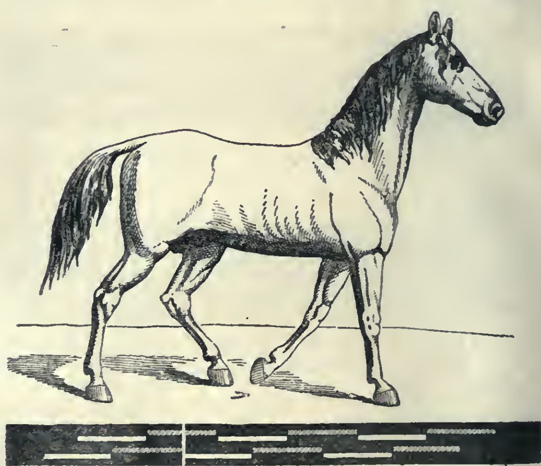


FIG. 19.—Foot pace, moment of lateral support.

responds to this question also. Just as the notation of a piece of music shows how many fingers rest at once upon

the notes on the keyboard, in the same way Fig. 11 shows that the horse, at the moment when he falls back upon the earth, is supported only by one foot; then, when the two diagonal feet strike the ground together in their turn, the horse has at this moment a triple support. Without the notation of paces we had certainly failed to distinguish this series of supports.

The gallop of the racecourse used generally to be considered as a pace of two steps, in which the horse struck the ground alternately with the two fore-feet and the two hind-feet. This gallop shows itself in the notation as a pace of four steps; the tracing dissociates the two fore-feet and the two hind-feet, although they follow each other at a very short interval.

The transition from one pace to another, impossible to determine by direct observation, is clearly inscribed in Fig. 12.

IV. *Artistic Representation of the Horse and other Animals.*—The artistic representation of animals requires a special and varied acquaintance with their peculiarities. Nothing can replace the patient study by which the painter or the sculptor acquires an anatomical knowledge of the limbs of animals and the aspects which they assume in different positions. But if the painter or the sculptor wishes to animate his work, if he wishes to show the horse putting forth its efforts in powerful traction, or to represent it urged rapidly forward in the race, it is necessary to have an exact acquaintance with different paces.

That which is true of the horse is equally so of other animals; but all present among themselves such great analogies in this respect, that if we are acquainted with the paces of the horse, we can represent those of any other animal.

The summary analysis we have just now made of the rhythms of steps in each pace is not yet sufficient to express the attitudes which represent them; we have as yet only examined in connection with these movements one of the two essential ideas. We are acquainted with the relations of time; it is necessary also to become acquainted with the relations of space, that is to say, to know at each moment in what place to find each of the members raised or planted upon the ground. Obliged to abridge this already long discourse, I will not tell you how one determines graphically the phases of the movement of a foot that is raised, but I will show you summarily how we determine the place where each foot is brought down. This indication is furnished by the imprint which the horse leaves upon the ground. M. de Curnieu, Capt. Raabe, and M. Lenoble du Teil have studied with particular care these imprints or tracks of the horse at different paces. The smooth sand of the sea-shore presents a surface admirably adapted for this study. Persons who have acquired the habit, easily decipher such imprints. But in order to render them easily read by every one, we have conceived the idea of giving a different form to the shoes of the fore-feet from those of the hind-feet by furnishing the latter with clamps. The principal paces represented by their tracks have been thrown together in Fig. 13, which I have borrowed from M. Lenoble du Teil. In combining with the idea of the rhythms, that of the place where each foot would be planted, the errors of attitude which disfigure so many *chefs d'œuvres*, would be avoided. You will, perhaps, say that few persons are capable of recognising faults of this kind. On this point one might repeat what Baron Dupin said with respect to perspective in its relation to the art of painting. "In proportion as exact knowledge becomes more widely diffused, many faults which to-day only shock a small number, will shock the general public, and artists will no longer be able to perpetrate them with impunity."

Those artists who at the present time make such praiseworthy efforts for the correct representation of the horse, would find great assistance from making use of the notation of paces. Let us see in what way. Take, for example, the notation of *the trots*. We all grant, in the first place, the possibility of dividing each step into a series of successive instants, ten or twenty, for example. At each of these instants the horse will have a different attitude, but throughout the time the limbs diagonal to each other will be executing the same motions at the same time.

Let us take one of these instants at random and mark it by a vertical line (Fig. 14). The notation shows us that at this instant the right hind-foot and the left fore-foot are planted upon the ground, but that the right fore-foot and the left hind-foot are still raised, and are about to be set down. That is exactly as represented in the figure.

Upon another notation (Fig. 15) we have chosen another instant, that in which the horse is suspended in the air, and when the hind-feet have already quitted the ground, whilst the fore-feet do not yet touch it.

Let us pass on to the pace of the gallop (Fig. 16). The moment chosen is that in which the horse falling back upon the hind-foot has just made his first step. Two limbs in diagonal relationship are about to strike the ground at the same time, namely, the right hind-foot and the left fore-foot, represented as already being lowered towards the earth. As to the right fore-foot, that will strike the ground last, accordingly it is shown as still the furthest removed from the earth.

With regard to the foot pace, which is the most difficult to explain, three instants have been selected on the notation: 1. That where the hind-foot is about to be raised, and where the fore-foot is in the middle of its elevation (Fig. 17). At this moment there are three feet at rest, which only takes place with horses when making an effort of traction. 2. The moment in which the two diagonal feet are raised, the one having just quitted the ground and the other being about to be placed upon it (Fig. 18). 3. The moment when the animal, supported by two feet on the same side, is raising one of its fore-feet and is about to set down one of the hind-feet on the same side (Fig. 19).

These pictures have no other pretensions than to be correct as regards the position of the members; it would be the artist's duty to add elegance of form. But is it not something to have a simple and sure means of representing a horse in any pace and in any phase of the steps in that pace? The employment of the graphic notation would give to the artist the double advantage of representing the paces with truthfulness and of varying them to an extent almost illimitable. Now, imperfection in art is not displayed solely by errors that may be committed, for too often an artist who is thoroughly acquainted with a correct attitude repeats it with regrettable monotony.

(*To be continued.*)

GEOLOGY OF NATAL AND ZULULAND

SOME years have now passed since Mr. Griesbach gave to the Geological Society his paper and map illustrating the geology of Natal and the borders of Zululand. Passing events now give to his investigations the greatest interest, not simply due to the possible light that may be thrown on unsolved, or partly known, problems by the sojourn in that area of the contingent that has lately left our shores, but from the fact that the safety and success of our forces in great measure depends on the surface contour and physical character of the country to be traversed, which are necessarily directly dependent on its geological structure.

Few can have studied a geological map, without noticing the close connection between long lines of escarpment and belts of level plains with particular rocks, or noticing the marked uniformity in direction of strike of geological formations over large portions of the earth's surface; rock series after rock series plunging beneath its neigh-

bour, and disappearing from view, only to be succeeded by higher and higher strata above them. No better example of this could well be found than the map of Natal of Mr. Griesbach; the rocks all run parallel to the sea-coast, striking into the Zulu territory, and dipping steadily the one after the other into high ground, forming the watershed between the Indian and Atlantic oceans. The eastern or seaward belt about fifty miles in width, consists of ancient mica-schists, resting on granite and gneiss seen at the bottom of the deeper valleys, the whole surmounted by the "Table Mountain Sandstone," of carboniferous age, forming extensive plateaux, lying perfectly flat on a horizontal surface of clay-slate, and broken by lines of fault, into a series of steps, plateaux rising above plateaux, with precipitous sides to a height of 2,300 feet between the sea and Pietermaritzburg, where the country again descends to 2,080. These table-lands are covered with extremely poor soil, supporting a dense grass vegetation, on which feed numerous herds of cattle; not a shrub occurs to enliven the endless uniformity of the scene, broken only by the ravines formed by the rivers cutting down through the sandstones to the granite and old rocks beneath, often forming precipitous cliffs several thousand feet high, the vertical drop, from the Krantzkop Mountain to the River Tugela being nearly 3,800 feet. The top of this mountain is composed of melaphyre; these melaphyre greenstones contain copper ores and strike south-westwards to the Ingeli range in Kaffirland.

At Pietermaritzburg, the next belt of country commences, the town being built on the basement beds of the Karoo formation, belonging to the *Dicynodon* beds of South Africa, of triassic age, the name being given from the "karoos," or immense plains of the interior, forming the largest part of South Africa, including the elevated tract of "Kalahari, the Free States, and the Transvaal, as well as the country to the north, as far as Limpopo." They are present in the Zambezi, and rise to a height of 12,000 feet in Mont-aux-Sources, in the Drakenberg Mountains. The base of the Karoo series rests unconformably on the carboniferous table mountain sandstone, and consists of large angular blocks of transported granite, greenstone, and gneiss, in a matrix of clay and grit. They occupy a large area, and pass under plant-bearing shales. These boulder beds have been ascribed a glacial origin by Dr. Sutherland, Surveyor-general of Natal. Mr. Griesbach points out the overlying plant-beds correspond to the plant-beds of Southern India, associated with *Dicynodon* remains, and also resting on a boulder bed (Talchir group).

The great Karoo plains, Dr. Grey is inclined to regard as the bed of an inland sea; salts of soda predominate largely in the salines of the soil, and assist in forming the "background" of this region (sandy soil, with salt, carbonate of soda, and some salts of magnesia and alumina). Its surface forms the sweet-grass country of the Dutch "Zout-Veldt," yielding the valuable Karoo plant (*Adenchœra parviflora*). In this tract the climate is most salubrious, and the higher the country ascends the more fruitful is the ground. The yellow wood flourishes, wheat and European fruits flourish, and the cold of the winter, though not so severe as that of northern Europe, braces the European settler, and agrees with his constitution.

Fringing the Natal shore, there is a narrow belt of the Karoo formation, resting unconformably on the table mountain sandstones and older rocks, so that the latter form an exceedingly low and flat-topped anticlinal arch, throwing off the Karoo beds on either side. Landward these rise to the Drakenberg, seaward they have for the most part been denuded away, though their presence in Southern India points to the former extension over what is now the Indian Ocean of a series of lakes fringed by lands covered with plant growths, extending over Southern India, and parts of South Africa. The investigations of Mr. Blanford in Southern India support the views of Prof. Huxley and Mr. Sclater as to the existence of an extending submerged mesozoic continent, "Lemuria," which was shadowed forth in Mr. Darwin's researches on coral reefs. Mr. Blanford comments strongly on the great relation between the plants of the Indian and Australian (New South Wales) coalfields, many of the species being identical, the two localities being no less than 5,550 miles apart. In India these plant-beds rest on the supposed glacial (Permian?) beds of the Talchir group, the included scratched blocks being often forty-two feet in circumference.

The Karoo boulder bed is described by Dr. Sutherland as containing well-scratched blocks, inclosed in a material which has since been metamorphosed, and resting on scratched old silurian sandstones. The characters of the various members of the Karoo series is well capitulated by Prof. Rupert Jones in Mr. Ralph Tate's paper on South African jurassic marine mollusca, the sequence being: Stormberg beds (Huxley); Beaufort Beds; Koonap beds, and Ecca beds. The Beaufort beds most closely correspond to the Dicynodon beds of India, the boulder beds in both countries, according to Mr. Blanford, being pre-triassic, and he carries back his Indo-oceanic continent to Permian times, and extending up to a late jurassic epoch—South Africa, India, and Australia being connected at the early part of the period, Africa and India, up to the end of the miocene.

In 1824, some caves called *Izinhluzabalungu* (white men's houses) were discovered by Mr. Fynn to be fossiliferous; the name, given by the natives, was due to shipwrecked sailors having taken up their abode in them. In 1851 Capt. Garden had his attention called to these fossils, especially some gigantic *Inocerami*, two feet by one foot, by his servant, named Thomas Souton, a Private in the 45th Regiment, after whom one of the fossils obtained was named by Mr. Baily, who examined them at the request of the late Prof. Forbes. The deposit occupies a small tract on the south end of the colony, and, as Mr. Baily pointed out, may be correlated with the lower cretaceous of Southern India, one species *Pecten quinquecostatus* being common to the English greensand. The investigations of Mr. Griesbach have largely added to the number of the species, and supported Mr. Baily's conclusions, twenty-two of the species occurring in India, thirteen being peculiar. Another patch of cretaceous rocks occurs at St. Lucia Bay, in Zululand, resting unconformably on the Karoo strata.

At the close of the jurassic period, the Indo-oceanic continent was submerged beneath a shallow cretaceous sea, surrounded by coasts, covered with vegetation, extending from India to Natal. At the close of this epoch elevation commenced, and is probably still going on, as raised beaches, coral reefs, and oyster banks may be seen twelve feet above the sea. Through this action the Port of Durban must inevitably be silted up, which will be the fate of most of the ports on this coast, except the large port of Delagoa Bay, which is naturally clean swept by the north and south Mozambique current, which has gradually hollowed out the Bay.

CHAS. E. DE RANCE

OUR ASTRONOMICAL COLUMN

PRIZES OF THE PARIS ACADEMY.—At the annual public sitting of the Academy of Sciences at Paris, last week, the medal on the foundation of Lalande was awarded to M. Stanislas Meunier for his researches on the constitution of meteorites, which, in the opinion of the Commission appointed for the consideration of claims, have led to results that occasion surprise, but at the same time appear justified by M. Meunier's investigations. Astronomers had followed with interest the labours of M. Daubrée, who has contributed so much to establish

a connection, little expected, between these bodies falling from the heavens, and the lower strata of our globe, and this circumstance has caused an increased amount of attention to the researches of his pupil and follower, M. Meunier, who finds by his recent work that the analogy alluded to is not confined alone to mineralogical constitution, but that it is extended to the relation which these cosmical materials, disseminated in space, present when compared amongst themselves, as is done for the constituent rocks of our globe. The Commission considered that M. Meunier had reason to conclude, from his experiences, that all these masses once belonged to a considerable globe, like the earth, of true geological epochs, and that later it was decomposed into separate fragments, under the action of causes difficult to define exactly, but which we have more than once seen in operation in the heaven itself. Such a conclusion, it is remarked, adds greatly to the interest attaching to these "minute stars:" the astronomer, once occupied only with their motions and their probable distribution in space, finds himself confronted with a sidereal geology, as he already was under the necessity of having regard to celestial physics, celestial chemistry, and celestial mineralogy. The medal is awarded with the view to encourage M. Meunier to follow up his studies, so interesting in regard to the constitution of the solar system.

The Valz prize was adjudged to Dr. Julius Schmidt, for his great chart of the moon, and the immense labour which its production has involved during a period of thirty-four years. The report of the commission for this prize contains a brief *résumé* of earlier work in this direction, concluding with a remark, the truth of which will be sufficiently obvious, that Dr. Schmidt's work, "aujourd'hui déjà si précieux, servira dans l'avenir de base à de nombreuses investigations, et nous pensons que le temps ne fera qu'en accroître la valeur."

The Damoiseau prize, first proposed in 1869 for a revision of the theory of Jupiter's satellites, discussion of the observations, and redetermination of the constants involved, with the formation of tables of the satellites, has been renewed without effect in 1872, 1876, and 1877, and is further remitted to 1879. The value of this prize is 5,000 francs.

FAYE'S COMET.—Dr. Axel Möller, continuing his elaborate investigations on the motion of Faye's comet, which he has conducted with so much success during the last twenty years, has communicated to the Stockholm Academy elements and an ephemeris for the next appearance, which it now appears will not take place under such favourable circumstances for observation as has been stated elsewhere. From November, 1874, to April, 1876, the distance of the comet from Jupiter was less than twice the mean distance of the earth from the sun, and in June and July, 1875, was not more than 1·5; the effect of this has been to retard the next perihelion passage by more than thirty-eight days, or to delay it till January 22, 1881, under which conditions the theoretical intensity of light can at no time be half as great as at the date of discovery by M. Faye in 1843. At the last return only four observations appear to have been secured, owing to the comet's excessive faintness, three by M. Stephan, at Marseilles, on September 3, November 28 and 30, and one by Dr. C. H. F. Peters, at Clinton, U.S., on December 23; so admirably had the calculations of the perturbations during the preceding revolution been effected by Dr. Axel Möller, that M. Stephan's first observation gave the comet's position only *four seconds of arc* from the predicted place. The chief disturber of the motion of this comet is, of course, the planet Jupiter, but Dr. Möller takes into account also the effect of the attraction of Venus, the earth, Mars, Saturn, and Uranus. The amount of perturbation during the actual revolution is greater than in any other since the comet's discovery. The next perihelion passage takes place 1881, January 22·665, G.M.T., the comet at this epoch moving in an ellipse with a period 56·526 days longer than at the previous perihelion passage in July, 1873. Dr. Axel Möller's ephemeris extends from 1880, July 1, to 1881, January 1; the comet will be nearest to the earth on October 3, distance = 1·09, and situate at this time some ten degrees south of α Pegasi.

BIOLOGICAL NOTES

FOSSILS OF THE AMAZONIAN DEVONIAN.—MR. R. Rathbun, late of the Geological Survey of Brazil, has published a list and description of the Brachiopods of the three Amazonian-Devonian localities, showing that of the twenty-one species recorded from the Mæcurú, thirteen were also found on the Caruá, including all the commoner species of the former. There is not so close a relationship between the Ereré fauna and the Mæcurú. Several of the commonest Mæcurú species do not occur at Ereré, and *vice versâ*. At Ereré there are five species of Lingula, four of Chonetes, four of Spirifera; at Mæcurú there are no species of Lingula, four of Chonetes, and six of Spirifera. Several of the Amazonian shells are identical with those of the North American Devonian; three in the Mæcurú, and Caruá, viz., *Spirifera duodenaria*, *Amphigenia elongata*, and *Strophodonta perplana*. Two forms of these are only known in the Corniferous limestone and Schoharie grit of North America. The Ereré beds are more closely related by their fossils to the Hamilton group than to any other North American group. In Pará, on the whole, there is the same general succession of species as in the Corniferous and Hamilton groups of North America, and a similar intermingling of forms. The lamellibranchs are not published yet, but it appears probable that many species are identical with New York State forms. Among the Trilobites are species of Homalonotus, Phacops, and other genera. (*Proc.* Boston Society of Nat. Hist., 1878.)

AUSTRALIAN FOSSIL CORALS.—The subject of Australian fossil corals has occupied much attention among palæontologists of late years. The investigations of the forms found in the deep sea has brought the tertiary forms into prominent notice. Following in the line of the researches of Prof. Duncan, the Rev. J. E. T. Woods has recently published (*Journal* and *Proceedings* of the Royal Society of New South Wales, vol. xi., 1878) a paper on some Australian tertiary corals, in which he describes some new species from Muddy Creek, near Hamilton, in Western Victoria. Some of the species are very interesting, and the author concludes his paper by asserting:—1. That there is no species of the genus Caryophyllia living in the Australian seas, or to be found fossil in its rocks. 2. That there are three well-marked and peculiar forms of Deltocyathus. 3. That of the two species known of Sphenotrochus in Australia, one is still living (*S. variolaris*, n.s.) at a depth of seventy fathoms. 4. That there are two fossil analogues of the living *Conocyathus sulcatus*, which itself is supposed to be identical with a European miocene form. 5. That there is a fossil form in the miocene rocks of Australia, of the cretaceous genus Smilotrochus. The Rev. W. Woods mentions that he is preparing a monograph of the recent species of Australian corals.

HERRING CULTURE.—Dr. H. A. Meyer has published an interesting contribution to the natural history of this important fish, as part 1 of a series of short papers to be issued by the Commission for the Scientific Investigation of the German Sea (Berlin, 1878). In this he supplements his previous researches into the influence of the temperature on the development of the spring herrings' eggs. It may be remembered that in the large report published by the Commission it was found that the escape of the herring from the egg, in the case of the autumn herring, could be very considerably delayed by keeping

the eggs in very cold water; and now experiments made with the same object in view prove that in this respect there is very little, if any, difference in the behaviour of the autumn and spring spawnings. As in the previous experiments eggs artificially fecundated were those operated with, and while some of the eggs were exposed to the salt water at its ordinary temperature at Kiel, others were placed in a wooden refrigerator, into which the same sea-water, but cooled down to the desired degree, was admitted. A most necessary precaution was keeping the eggs from being heaped together, as they then almost invariably became mouldy. Another series of experiments was made to test whether the eggs exposed to the very salt waters of the North Sea would ripen quicker or slower than those exposed to the less salt water of the Baltic, but the time of the development, the temperature of the waters being the same, was found to be very slightly, indeed hardly perceptibly different. A third series of experiments were of a very interesting nature, supplementing those already made, as to the rearing of the herring from artificially fecundated eggs. So far as is known, no one has yet succeeded in rearing the young herring, and even Dr. Meyer's repeated attempts broke down, owing to the impossibility of stopping the formation of the hyphæ of some fungus, and also in some measure to the difficulty of obtaining suitable food. Very soon after the yolk was altogether consumed they would die, so that most of the experiments on their growth were made on specimens freshly caught from time to time. Once he succeeded, in the spring of 1878, in rearing a few until they attained the size of 72 mm. However, as the result of these experiments, a great deal of insight has been obtained into the food—at first of almost microscopical dimensions—which the young herring consumes, and as to the enormous voracity of the little fish.

MADAGASCAR FORMS IN AFRICA.—At a recent meeting of the Society of Naturalists of Berlin Herr Eichler exhibited specimens of a new species of *Ouvirandra* lately discovered by Herr Hildebrandt in Eastern Africa. The remarkable form of water-plant known as the Lattice-leaf Plant (*Ouvirandra fenestralis*) with two other species of the same genus have been hitherto regarded as amongst the wonders of the peculiar flora of Madagascar, so that the discovery of a member of the same group in continental Africa is a fact of much importance in botanical distribution. The new *Ouvirandra*, although agreeing with the Madagascar species in all essential points of structure, does not present the singular holes in the leaves that distinguish the *Ouvirandra fenestralis*, but one of the other Madagascar species is likewise abnormal in this respect.

THE "DIGGER" MOLLUSC AND ITS PARASITES.—The little digger, *Donax fossor*, represents a countless mass of life off Cape May, New Jersey, large areas looking like barley grains lying on a malting floor when the tide retires. It gets uncovered by the breaking surf and instantly reburies itself with its powerful foot when the waves retire. The siphons are long and active, looking like so many wriggling worms. Although the prey of shore birds and fishes, and beset with parasites, they lie so thick as even to interfere with one another in burying themselves. The liver of these bivalves is always found beset by flukes, from half a dozen to several dozen, and a bell-shaped trichodina crowds the branchial cavity.

ACTION OF THE HEART OF THE CRAYFISH.—M. Felix Plateau, of Ghent, has succeeded in applying the graphic method to the study of the heart's action in the crayfish. A curve is obtained, of which the ascending portions correspond to diastole, and the descending to systole, contrary to what obtains in the vertebrate heart. It is strikingly like the trace of the contraction of a muscle; a rapid, almost sudden ascent, with a short flat summit, then a gradual descent, at first quicker, then slower. This, however, does not represent the whole truth; it is possible, also, to demonstrate a wave affecting the muscular wall of the heart, and travelling from behind forwards, thus demonstrating that this condensed heart is a true dorsal vessel. On the stimulus of the entrance of renovated blood, it is only the hinder half or two-thirds of the heart that contracts immediately. This forces blood into the forward half, which contracts only when the posterior division is again dilating. When the temperature is increased, as a general rule the diastolic phase is abbreviated, the number of pulsations rising at the same time. M. Plateau has also succeeded in making experiments on the action of the cardiac nerve of Lemoine, an unpaired branch of the stomatogastric ganglion. It is proved that excitation of this nerve quickens the pulsations of the heart, and augments their energy, while section of it slows the heart. Excitation of the thoracic ganglia always retards the heart, the converse of the cardiac nerve. Acetic acid applied to the heart substance arouses its contractions even when they have ceased, and maintains them for several hours. The action of a number of other substances is equally noteworthy, and M. Plateau's full communications to the Académie Royale of Belgium will be awaited with interest by physiologists.

GEOGRAPHICAL NOTES

THE various geographical journals to hand contain several papers of importance. In the January *Bulletin* of the Paris Society M. Maunier gives a full and intelligent sketch of geographical work during 1878, while Dr. Harmand gives a brief statement of the results of his recent journeys in Anam. The Paris Society seems to have followed the example set by the London Society, and has introduced a new feature, "Nouvelles," containing notes of geographical work beyond the limits of its own papers. The *Zeitschrift* of the Berlin Society contains two instructive papers, on the Andamans, by Ad. de Roepstarff, and an account of a journey in south-west Persia, by Dr. A. H. Schindler. In the two numbers of the *Verhandlungen* of the same society, the last for 1878 and first for 1879, the papers of most interest are those on the Mining Industry of Russia, by C. Skalkovsky; on the latest researches on the Aurora Borealis, by Herr Förster; and on the people of East Africa, by Herr Hartmann. In the *Mittheilungen* of the Vienna Society, No. 2 of this year, Herr Franz Heger gives some hints as to a solution of various geological questions,—glaciation, climate, coal-deposits, &c.—apparently seeking to account for many of the great geological problems by a change in the earth's axis. The March number of Petermann's *Mittheilungen* contains several papers of interest. From the journal of a Bremen merchant a narrative is given of a journey up the Jenissei, from its mouth to Jenisseisk, in the summer of 1878; and M. N. Latkin gives a detailed account of our knowledge of the Lena and its basin. Exact news of Nordenskjöld's position is given from the San Francisco whaling captain, who was the first to hear of him, and a statement as to the course to be followed by the steamer *Nordenskjöld*, now building at Malmö, and which will start in May, first to succour the Swedish expedition, and then to proceed to the mouth of the Lena. If it cannot return through Behring's Strait, the staff will spend the winter in collecting all possible data in various departments of science. Nos. 3 and 4 of the *Bulletin* of the American Geographical Society contain, the former a paper by Rear-Admiral Ammer, on the Inter-oceanic ship canal across the American Isthmus, and the latter an interesting sketch of the life and work of Mercator, by Mr. E. F. Hall.

NEWS of two African expeditions are to hand, in one case telling of misfortune, and in the other of success. The Belgian expedition, unfortunate from the beginning,

has met with another disaster in the death, from dysentery, of M. Wautier, at a place called Kekongen (? Ukonongo). On the other hand, Major Pinto, the leader of the Portuguese African expedition, telegraphs to Lisbon from Pretoria, that he has virtually crossed Africa from the west coast, after struggling with hunger, thirst, beasts, natives, floods, drought. His route must have to some extent almost coincided with that of Livingstone, and he tells us he has saved all his papers, twenty geographical charts, many topographical maps, several vols. of notes, drawings, meteorological data, a diary of the exploration of the Zambesi's seventy-two cataracts and rapids. He says he has discovered the secret of the Cubango, by which he seems to mean the river which, under various names, was for a time taken by some to be the upper course of the Congo. He lost many followers, and his expedition seems in a small way to have been modelled on that of Stanley's.

THE *Times* Roman correspondent writes that Menotti Garibaldi and Achille Fazzari intend, if England does not object, to sail in summer or autumn with 3,000 Italians for the south coast of New Guinea, to establish a colony there, and found a new city under the name of Italia. The arrangements, it is said, are almost completed, the 30,000,000 francs required ready, and that applications to join the party are more than can be granted. Part of the equipment will be a telegraph cable, to place the colony at once in communication with North Australia. Men of all ranks and callings (except lawyers) are included in the party, and among them several men of science. The proposed colonists express the greatest good will towards England, and it seems to us the trial would be worth making. The Italians make better colonists than the French, and Italians have done so much for the exploration of New Guinea that it seems only fair that they should be allowed to reap some benefit from the labours of such men as D'Albertis and Beccari.

AT the last meeting of the Société Commerciale de Géographie at Paris Dr. Raffray gave some particulars respecting his recent explorations in New Guinea, and called attention to the fact that that country offered a vast field for discovery and study to the traveller, especially from an ornithological and entomological point of view. A report on the subject of a railway across the Desert of Sahara was afterwards read, being the result of the labours of a committee, of which M. Gazeau de Vautibault is president. M. Deloncle also made a communication respecting the Volta region in West Africa, which has been explored by M. Bonnat, and he announced that two Lyons merchants had already determined to establish business houses there.

MGR. LAVIGERIE, Archbishop of Algiers, has forwarded to *Les Missions Catholiques* the commencement of the journal of the Algerian missionaries, recording the incidents of their march towards the Nyanzas and Lake Tanganyika. This portion of their journal stops at Mukuduku in Ugogo on August 20, and the first instalment of this is now published. It had been intended to accompany it by a map of Equatorial Africa, sent home by Père Charmetant some time back, but it has been thought better to delay the publication, in order that the itinerary of the missionaries and the additional geographical information contained in their journal may be included in it.

IT is stated in an Italian newspaper that the Duke of Genoa will go on an exploring expedition, and will sail from Venice in the *Vittore Pisani* at the end of this month. The programme of the route is to be Port Said, Suez, Aden, Ceylon, and Singapore, where a longer sojourn will be made. Afterwards the traveller will proceed to the Chinese and Japanese coasts; in 1880 he will visit Australia and direct his special attention to the exploration of New Guinea. On the return journey the *Pisani* will cruise in the Persian Gulf. Capt. Sebastian and Count Antonela have started on an exploring tour through Africa.

A POSTCARD was received at Berlin on February 15 from Dr. Gerhard Rohlffs, dated January 27 and posted at Sokna, some 250 miles south of Tripolis, at the foot of the Black Mountains, stating that he was in perfect health. The postcard bears the stamp of Dr. Rohlffs's desert post, and a prettily-drawn postage-stamp with African palm-leaves.

TO accompany the map of Zululand, noticed last week, Mr. Stanford has published a few useful notes on the physical features and population of the country.

THE *Jeannette* is fitting up in San Francisco harbour, and will leave for polar exploration in the month of June. Mr. Bennett, who is now in Europe, has been making inquiry at Paris as to the best means of constructing and inflating balloons in the Arctic regions. It is thus likely that aërial navigation will play a part in this new effort to solve the mystery of the north.

EDISON'S TELEPHONE

OUR readers may remember a few months ago we stated, in an article on the Carbon Telephone (NATURE, vol. xix. p. 56), that Mr. Edison had devised an entirely new form of receiver, for use with his telephone, which delivered the voice as loudly as if the words were spoken at the distant end. This receiver has now arrived in England in charge of Mr. Edison's nephew, and to judge from its performances last Friday, it is likely to accomplish all that Edison has stated concerning it.

The principle of this new receiver is that of the *electro-motograph*, and to those of our readers who may not be acquainted with this instrument the following extract from a recently published lecture, on Edison's inventions, by Prof. Barrett will explain what the electro-motograph is.[1]

"Mere ingenuity in contriving machines does not add to the sum of human knowledge, and if Mr. Edison were merely a clever inventor and nothing more, I should feel less interest in the man. It is, however, a noticeable feature of Mr. Edison's inventions that they, in general, contain some new principle, some original observation in experimental science, which entitles him to the rank of a discoverer. Such is the character of the next invention we must consider, the so-called *electro-motograph*. This is an entirely new method of receiving telegraphic messages, discovered by Edison in 1874. As every one is aware, the ordinary system of telegraphy depends upon the production of magnetism by means of an electric current, the current either attracting and releasing a movable piece of iron, or deflecting a magnetic needle to the right or to the left. By the to-and-fro movements of the iron or the needle the conventional signals are produced which are employed in telegraphy. Now Mr. Edison made the curious and important discovery that messages could be received by the well-known Morse recorder without the use of any magnet. This, to a telegraphist, would be like attempting to perform the play of "Hamlet," while omitting the part of *Hamlet* himself. In fact, all that is necessary in this simple telegraphic instrument is a band of moistened paper drawn beneath a metal style. The accident of holding his finger against the style of a Morse instrument led Mr. Edison to notice that when an electric current passed from the paper to the point resting upon it the friction of the moving paper was lessened. Hence, if the paper were drawn with a uniform force it would slip more easily beneath the point the moment the current passed. The slipping of the

[1] "Science Lectures for the People," No. 5, Tenth Series. (Manchester, Heywood.)

paper is converted into a to-and-fro motion of a lever to which the point is attached, and which is made to actuate a bell, or "sounder," and give rise to audible signals in the usual way. It is necessary to moisten the paper with a solution of certain chemicals. Potash was at first used, but a solution of sulphate of soda or of common salt and pyrogallic acid is found to be best.[1]

"The advantage of this instrument, which Mr. Edison calls an *electro-motograph*, is said to be its extreme sensitiveness, it having been worked over a circuit of two hundred miles with only two cells, so that with weak currents, unable to affect ordinary instruments, the electro-motograph can receive messages. More than this, the speed of its working is greater than with the ordinary instruments. Using it as a relay, that is, an instrument for translating weak currents into strong ones, no less than 1,200 words per minute have been transmitted by its means, or five times as fast as it is possible for any person to read the message which comes through the instrument. So prompt and delicate is the motion of this machine that Edison has applied it to the purposes of the receiving instrument for the Reiss telephone, a musical telephone that was made many years ago. The slipping of the paper causes a slight sound. If, then, we sing a certain note into the Reiss transmitting instrument, which vibrates in unison with that note, we obtain the same number of electric currents produced per second as we had of sonorous vibrations in the moving diaphragm. Thus, if we sounded the middle C we should get 264 vibrations, and there would be 264 electric currents, and 264 slips of the paper, thus producing a note of the same pitch in a distant room. The cause of the curious slipping has not been fully ascertained. It may possibly be due to that peculiar repulsive effect to which Mr. Crookes has lately drawn attention, and which produces the dark region around the negative electrode during the continuance of an electric discharge in a vacuum tube, or it may simply be due to electrolytic action."

It is, then, this principle which Edison has made use of in his new receiver, which is of the simplest construction. A diaphragm, preferably of mica, some four inches in diameter, held in a suitable framework, has attached to its centre a spring, or "pawl," the free end of which rests on a little cylinder of chalk, capable of rotation by the hand or other means. The chalk cylinder replaces the paper in the electro-motograph, and is necessarily impregnated with sulphate of soda, or other suitable solution. As the cylinder is rotated, the friction of the spring on the chalk causes the diaphragm to be pulled in or pushed outwards, according to the direction of the rotation. So far the operation is purely mechanical; as soon, however, as the current passes, either owing to electrolytic action or the friction, it is lessened, and the diaphragm tends to spring back to its normal position; on the cessation of the current the friction is restored, to be lessened on the recurrence of another electric wave. Thus, a series of tremors are given to the diaphragm corresponding to the swiftly changing character of the electric waves, and these again faithfully express the motion of the diaphragm at the transmitting end. It will thus be evident that the incoming current has simply to do the work of liberating the already strained diaphragm. As everyone knows, in Bell's telephone the voice has to do the work of creating the current at the transmitting end, and the feeble magneto-electric currents thus generated throw into motion the diaphragm at the receiving end. In Edison's telephone this is not so. The voice at the transmitting end has simply to vary the *electric* resistance in the path of a current generated by an ordinary voltaic battery; stronger currents can thus be sent along the line, and these arriving at the receiving end, have merely to vary a *mechanical* resistance, and not to do the work of overcoming the inertia of the diaphragm. It is probable the rotating chalk cylinder acts on the diaphragm with its attached spring like a resined bow on a violin string; vibrations are set up, the extent, rate, and manner of which are modified by the varying friction due to the telephonic currents. Whether these new receivers will retain their present efficacy when in constant use remains to be seen. We should be inclined to think the soft surface of the chalk will eventually wear with the friction, and that a more permanent arrangement will have to be devised. No invention, however, reaches perfection at once, and the present receivers, excellent as is their performance, were, we understand, hastily made in a few days, in compliance with the urgent request of Mr. Edison's courteous representative in London, Col. Gouraud.

The instrument has the appearance of a small box attached to the wall, and from which there projects a single funnel. Sounds of singing, speaking, whistling, sent from the other end, quite a mile off, were heard in every part of a moderately sized room. Telephonic connections, now so common in America, have been established by Col. Gouraud between various business houses in the city; and we believe that shortly this method of communication must become quite common.

[1] Practical difficulties have, we believe, been found in the working of the motograph, so that it has not come into telegraphic use.

NOTES

Dr. Miclucho Maclay, the eminent Russian Naturalist and New Guinea explorer, has been trying to rouse the Linnean Society of New South Wales and the scientific public of Sydney to the necessity of founding a zoological station, similar to that at Naples. He tells of the great inconvenience he himself has suffered during his residence at Sydney from the want of such a station, even though the Hon. Mr. Macleay placed his museum at his disposal. But Dr. Maclay's scheme embraces much more than a station at Sydney. He has written to the German Eastern Asiatic Society at Japan and to Mr. August Godeffroy at Samoa, urging that similar stations be founded at these places, and he has reason to believe that his proposals will not be without result at both places. Thus should zoological stations be instituted at Sydney, in New Zealand (as Dr. Maclay also proposes), in Japan, and at Samoa, we might hope in a very few years to have a fairly complete knowledge of the fauna of the Pacific. Dr. Maclay's proposal deserves the heartiest encouragement, and we trust that ere long it will be fully carried out. We hope the people of Sydney, at any rate, will take Dr. Maclay's appeal to heart; he tells them, moreover, that he will judge of the intensity of the scientific life of Australia by the interval which elapses between the reading of his paper on the subject and the actual foundation of the station. He shows what valuable results have followed the foundation of the Naples station, and gives a few hints as to how such a station at Sydney should be organised. We shall be curious to see what will be the result of Dr. Maclay's fervent appeal.

We are requested to state that on and after April 1 any person may obtain by telegraph from the Meteorological Office the latest information as to the weather in any district of the United Kingdom by payment of a fee of 1*s.* in addition to 2*s.* the cost of the message to the Meteorological Office and the reply. The telegram containing the inquiry must not exceed twenty words in length, and must be addressed, "Meteorological Office, London." The Meteorological Office does not undertake to give any information which is not substantially included in the latest notice posted at its own doors, nor does it give forecasts of the weather on the Atlantic coasts of the British Isles; although it is ready to furnish any information it possesses as to the actual state of the weather on those coasts. The Meteorological Office is open for such inquiries between the hours of

11 A.M. and 8 P.M. on week days, and between 6 P.M. and 8 P.M. on Sundays.

THE Emperor of Germany has confirmed the election of Sir G. B. Airy as a foreign member of the Berlin Academy.

RUSSIAN astronomers seem determined to outstrip their *confrères* in other countries in the matter of telescopes; we are informed that funds have been subscribed for the construction, for Pulkowa Observatory, of a refractor of thirty-two inches aperture.

WE have received a circular from the Research Committee of the Institution of Mechanical Engineers, drawing attention to three subjects which they have selected for first investigation, and asking for any information, bearing on all or any of them, which any one interested may be willing to communicate: *e.g.*, records of unpublished experiments, references to authorities on the question, copies of books or papers in which it is treated, &c. The Committee would be glad to receive such information in full detail, and at the earliest convenient date; and it will be suitably acknowledged in their report. The subjects are:—Subject A. The hardening, tempering, and annealing of steel. Subject B. The best form of riveted joints to resist strain, in iron or steel, or in combination. Subject C. Friction at high velocities, specially with reference to friction of bearings and pivots, frictions of brakes, &c. The address of the Institution is 10, Victoria Chambers, Victoria Street, Westminster.

IN the Monthly Weather Report of the U.S. Signal Service for January, 1879, are many points of great interest to meteorologists. The particular report before us belongs to the division of "Telegrams and Reports for the Benefit of Commerce and Agriculture," and contains a multitude of well-arranged data received up to February 14. The charts accompanying the Report are of special interest. One of them shows the tracks of ocean-storms from November 23, 1878, to January 16, 1879, and exhibits the paths of seven storms. No. 6 of these is shown to have commenced in California on January 6, to have come down to the Gulf of Mexico by the 8th, to have gone north-east through the United States to Newfoundland, between the 8th and 11th, across the Atlantic between Scotland and Iceland, the storm expending itself off the coast of Norway on the 15th, one week after starting from California.

EXPERIMENTS in electric signalling and reconnoitring have been made at Mont Valérien on a large scale. Details are wanting, as the French Government think it prudent to keep secret almost all experiments relating to military matters.

WE have received an interesting sketch, with portrait, by Prof. Ehlers, of Göttingen, of the late Wilhelm Engelmann, so long the head of the well-known Leipzig scientific publishing house, and who did so much for the advancement of scientific knowledge in Germany.

The Science Index is the title of "A Monthly Guide to the Contents of the Scientific Periodicals," the first number of which we have just received, though dated January. This delay is apologised for, on account of the difficulty of getting together a first number. The aim of the journal is highly to be commended, and if carried out on a thoroughly well-considered plan, ought to prove of great service. We are not disposed to criticise this first number too severely, though we think there is considerable room for improvement. It is by no means exclusively devoted to science, including as it does Art, Architecture, Strikes, Baking and Confectionery, Bells, Commerce, and other miscellaneous topics. Indeed, on the principle which has been partially followed, we do not see where the line is to be drawn short of an index to everything. We hardly think the plan has been well considered, and we think that many of the subjects indexed might be omitted with advantage if it is meant to be really a Science Index. Judging from the number of misprints, this number seems to have been hastily got out. In geography alone we meet with such horrors as "Afkanistan," "Leybian Desert," "Oxies" for Oxus, &c. References to the Swedish Arctic Expedition occur under different headings, as if the compiler did not know that the items referred to the same thing. Still, the index is a step in the right direction, and we hope the editor will take competent council, and introduce such improvements as will make his *Science Index* what it might and ought to be.

MAJOR MAJENDIE, as the result of a series of experiments with dynamite, has come to the conclusions that frozen dynamite is considerably less sensitive to explosion by a blow than unfrozen dynamite; that cartridges of dynamite having small quantities of exuded nitro-glycerine within them are decidedly more sensitive to explosion by a blow than cartridges in which there is no such exudation; that frozen dynamite is much more susceptible to explosion by simple ignition than unfrozen dynamite; that frozen dynamite is much less sensitive to explosion by the impact of a bullet than unfrozen dynamite; that the danger attending the mere breaking in two of a frozen dynamite cartridge does not seem to be of the formidable character indicated by the Austrian regulations; and that frozen nitro-glycerine is not susceptible of detonation by detonators of the same strength as those with which the detonation of unfrozen nitro-glycerine may be readily and certainly effected.

THE *Bradfordian* is the title of a magazine "written and supported by the two Grammar Schools" of Bradford. It has a varied programme, in which, we are pleased to see, science finds a place.

THE *Times* Geneva correspondent writes that M. A. Borel, of Chaux-de-Fonds, has just had the good fortune to find in the Lake of Neuchatel, between Bazuge and Chatelard, a prehistoric canoe, probably the finest specimen of the sort that has yet come to light in Switzerland. Hollowed out of a single piece of oak, the vessel is 8 metres long, 90 centimetres wide, and 65 centimetres high. It is well finished, and in a perfect state of preservation. The stern carries a spur, and the prow is curved in the form of a hook, probably for the purpose of attaching it by a rope to a landing-place. The canoe is sufficiently large to carry twelve persons. There is no appearance of rowlocks, but the supports on which the thwarts formerly rested are still plainly to be seen. M. Borel proposes to present this interesting "find" to the Museum of Chaux-de-Fonds.

A PETROLEUM spring, one boring of which has yielded 2,000 kilos in twenty-four hours, has been discovered at Pohar, in Austrian Poland.

ACCORDING to the report of H.M.'s Consul for Hiogo and Osaka, the Japanese claim that petroleum has been known in Japan for over 1,200 years, and it would certainly be curious if the numerous springs which exist in certain localities should have escaped notice in their immediate neighbourhood. It is doubtful, however, whether it was ever utilised, and certainly no attempt was made to refine it before the arrival of foreigners. The first efforts in that direction were made near Niigata in 1875, but the petroleum then refined failed to stand a higher test than 75° F. Accordingly Prof. Lyman, who had previously performed a similar service for India, was sent for from America to conduct a professional survey of the region. His report, however, was unfavourable, chiefly on the ground of an insufficient supply. This opinion the Japanese are now about to test, for which purpose they have established a refinery near Hiogo. Its supplies of crude oil are to be drawn from the province of Potomi, distant about 100 miles to the north, the transport being conducted by sea.

AT the evening meeting on Monday, the 31st inst., at 8.30, at the Royal United Service Institution, D'Arlincourt's telegraph will be exhibited and worked, illustrative of a means of communicating orders in the field. The paper of the evening is on "Orders in the Field and the Means of Communicating them," and will be read by Major Webber, R.E.

A PAMPHLET has just been issued at Quebec, compiled under the auspices of the Boards of Trade of that town and Montreal, dealing with the subject of telegraphy with the coasts and islands of the Gulf and Lower River St. Lawrence, and the coasts of the maritime provinces, considered from the point of view of its relation to the shipping, to the fisheries, and to the signal service. The *brochure* is accompanied by a large-scale coast telegraph chart of the region named, delineated under the direction of the Hon. P. Fortin, on which are shown existing and projected telegraph lines and the ordinary tracks of vessels.

AT the last meeting of the Eastbourne Natural History Society an interesting paper was read by Dr. Murdie on sea-water, its adaptations to various purposes, and its management in reference to marine zoology.

MR. TEGETMEIER has published a facsimile reprint of Moore's "Columbarium" from the original edition of 1735. The work, the source from which all subsequent works have taken their rise, ought to interest both fanciers and naturalists; by its aid, as Mr. Tegetmeier says, the latter may trace the alterations produced in varieties of the same species, continued for 150 generations. Moore was a well-known doctor of the City of London, and was so eminent in his way as to merit a bantering satire from Pope.

PROF. EDWARD MORSE, of Tokio, announces that he has discovered undoubted evidences of the practice of cannibalism among the early inhabitants of Japan. These evidences he describes in a paper read at the Biological Society of the Tokio Dai Gaku.

THE *Colonies and India* has an interesting note on the subject of Vanilla, which appears to be mainly supplied by Mauritius, Brazil, and Mexico, but could probably be grown in many of our colonies. The parasitical plant which yields this aromatic bean will climb up any tree that gives sufficient shade; it attains a height of about a foot, and thrives for thirty or forty years, producing some fifty pods each year after the second. The beans take eight or nine months to mature, and are gathered between October and December; they are oiled occasionally to prevent excessive shrinking, and dried in the sun; when warm they are wrapped in woollen cloths to absorb the evaporation, and during the process attain their black silvery hue. Vanilla is the most costly, in proportion to weight, of all vegetable productions, and only a few hundredweights reach England annually.

MASSON, of Paris, has published in a collective form, under the title of "Revues scientifiques publiées par le journal *La République Française*," a large number of papers on subjects of scientific interest published at intervals in that paper, under the direction of Prof. Paul Bert.

THE comparative effects of pressure and hammering in changing the volume of soft masses, has been investigated by Herr Kick (*Ding. Pol. Jo.*). A carefully cast lead cylinder 100·3 mm. high, 70·2 mm. diameter, and 387·85 ccm. at 15°, was compressed in a Gollner's machine to 69 mm. and 50 mm. height respectively. The volume was found to be hardly altered at all (it became 387·814 ccm. in the second case). A lead cylinder 59·7 mm. high and 50 mm. diameter was now beat down by means of a steam hammer to 16·7 mm. height. The volume was reduced from 117·56 to 117·33 ccm. Thus, to condense metals, it is necessary to resort to beating, or to use extraordinary pressure on inclosed material.

SOME interesting experiments with regard to tension of carbonic acid in blood have recently been made by Herr Gaule in the physiological laboratory of Prof. Ludwig (Du Bois Reymond's *Archiv*). He notes the following differences between blood and serum in this connection:—1. The proportion of carbonic acid in serum is higher than that in blood. 2. The tension of carbonic acid in serum is less than that in blood. 3. If in serum the quantity of free carbonic acid diminish, there is decomposed only the quantity of bicarbonate of soda which corresponds to this diminution, but in blood more. 4. On addition of simple carbonate of soda, the tension of free carbonic acid diminishes in the serum, but in blood it does not. 5. In blood there exists a substance which is capable of dissolving the entire combination between carbonic acid and soda, but in serum not. This substance, which attracts the carbonate of soda, is very probably hæmoglobin, which then further decomposes the combined salt into soda and carbonic acid, and thus maintains with the bicarbonate salt and the free carbonic acid a series of complicated exchanges, which probably render possible and promote the giving out of carbonic acid and the removal of it from the tissues and the lymph.

CARL'S *Repertorium für Experimental Physik* (xv. Band, 2 Heft) contains an illustrated account of the new meteorological magnetic observatory for St. Petersburg at Pawlowsk. In the same number we note a useful simplification of the spectroscope, by Herr Hüfner.

THE measuring of sea-depths with the lead leaves much to be desired in point of accuracy. Dr. Rühlmann is of opinion (*Annalen der Physik*) that reliable results will be had, only when it is practicable to measure the weight of the water column. This weight, and therewith the height of the column, might be easily ascertained from pressure on a manometer. And the point is, to combine with the pressure-measuring instrument an arrangement whereby one may read off what the apparatus indicates at that point, the depth of which below the surface is to be determined. To construct such a manometer need not, he thinks, greatly puzzle the mechanician. The various forms of aneroid barometer are a good direction. Suppose, *e.g.*, a manometer made after this fashion, and available for very high pressure, and let there be adapted to it an electro-magnetic arrangement, wherewith, when a current is sent through it from the ship, the index is pressed so forcibly against the scale, that a mark is produced on this. In this way, closing the circuit at different times, it would be possible to determine the pressure in different parts of the sea. If, at the same time, there were sunk a junction of a thermo-element, the other junction of which was kept at constant temperature, and if the current of this thermo-element were conducted through a sensitive marine galvanometer on the ship, one could ascertain the temperature of the water at the points for which measurements of pressure were obtained. Very accurate results could only be expected, when not only the pressure at the sea-bottom, but also the temperature of the water at as many points of less depth as possible, were known. Dr. Rühlmann is not aware if such a method has been put into practice, but he gives an exposition of the theory of it.

THE additions to the Zoological Society's Gardens during the past week include an Indian Fruit Bat (*Pteropus medius*) from India, presented by Capt. F. P. Millett; a Mule Deer (*Cervus macrotis*) from Ottawa, Illinois, U.S.A., presented by Judge Caton; two Gaimard's Rat Kangaroos (*Hypsiprymnus gaimardi*) from Australia, presented by Mr. Ernest E. Harrold; a Spotted Ichneumon (*Herpestes auropunctatus*) from India, presented by Miss H. Boteler; a Brent Goose (*Bernicla brenta*), European, presented by Mr. H. A. Dombrain; a Black-faced Spider Monkey (*Ateles ater*), a Black-handed Spider Monkey (*Ateles melanochir*) from South America, received in exchange; a Black

necked Stilt Plover (*Himantopus nigricollis*) from South America, purchased; three Common Badgers (*Meles taxus*) born in the Gardens.

THE PARIS ACADEMY PRIZES

LAST week the Paris Academy held its annual public meeting, when the prizes for 1878 were awarded. According to old custom, M. Fizeau, the president of last year, was in the chair. He remarked on the unprecedentedly large number of prizes that were not awarded, either because there was no competition or because there were no competitors of sufficient merit. On this account several of the most important prizes have not been awarded this year, and it seems to be the common opinion that some of the problems proposed are much too difficult. M. Dumas read an *éloge* on M. Balard, the discoverer of bromine, and M. Bertrand did the same for Leverrier. M. Bertrand made no allusion to the part taken by Leverrier in the public affairs of his time, and made but slight allusion to his organisation of the Meteorological Service, and that almost as if it were not a thing quite worthy of encomium. M. Bertrand's address does not seem to have given universal satisfaction, and several of the audience on leaving the hall were heard to say: "Quant à l'éloge de Leverrier il est encore à faire." The following are the principal prizes awarded at the meeting:—The Extraordinary Prize of 6,000 francs for the greatest prgress in naval construction, to M. Perroy and Lieut. Baills; the Poncelet Prize in Mechanics to M. Maurice Lévy; the Montyon Prize of 1,000 francs, in Mechanics, to Mr. George H. Corliss, for his well-known engines; the Plumey Prize to Capt. Vallessie, for his differential counter to regulate the progress of steamships. In Astronomy the Lalande Prize was awarded to M. Stanislas Meunier, for his researches on meteorites; the Valz Prize to Dr. Julius Schmidt, for his lunar charts. In Physics the Bordin Prize was awarded to M. Reynard for his researches in connection with Ampère's law. In Chemistry the Jecker Prize was awarded to M. Reboul, specially for his memoir on the isomers in the propylene series. In Botany the Barbier Prize was given to M. Ch. Tauret, and encouragements of 500 francs each to M. Cauvet and M. E. Heckel; the Desmazières Prize to Dr. Bornet; the Shore Prize to Prof. Ardissone for his "Floridee Italiche;" in Anatomy and Zoology the Serres Prize was awarded to Prof. Alexander Agassiz, for his various embryological and other investigations; and the Montyon Prize in Physiology to M. Charles Rechet, for his researches on gastric juice. The Tremont Prize was given to M. Marcel Deprez for his application of electricity to the solution of various problems in mechanics; the Gegner Prize to M. Gaugain; the Delalande Guérineau Prize to M. Savorgnan de Brazza, for his exploration of the Ogové River; and the Prize founded by Mdme. de Laplace to be awarded to the pupil who leaves the Polytechnic School with the highest honours, to M. de Béchevel.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

M. FERRY, the French Minister of Public Instruction, has presented a project for the reorganisation of the Superior Council of Universities. According to the proposals of the minister, which are sure to be adopted by the Assembly, the bishops and other religious members are to be excluded, and the Council exclusively composed of persons belonging to the teaching profession. Moreover, it is proposed that all degrees be henceforth granted by the State, and only to those who have taken the curriculum of a recognised university.

THE examiners for the Burdett-Coutts' Scholarship (Oxford) have awarded it to Mr. Algernon Philips Thomas, B.A., Scholar of Balliol College; and they consider Mr. Henry Nicholas Bidley, B.A., of Exeter College, worthy of honourable mention.

SCIENTIFIC SERIALS

Bulletin de l'Académie Royale de Belgique, No. 12, 1878.—In this number is a paper by M. van Beneden, giving a historical sketch of whale-fishing and of the first Arctic expeditions.—A lecture by M. Houzeau, the president, has for its subject certain enigmatical phenomena of astronomy.—M. van Rysselberghe describes a parabolic regulator, rigorously isochronous, and the velocity of which can be varied at will. Regarding it, M. Folie reports that it has too many articulations and movable rings for common use, and it hardly realises ideal perfection for physical and astronomical apparatus.—M. Malaise announces the discovery of a mineral species new for Belgium, viz., arsenopyrite or mispickel, and M. Monier describes a hydrophane opal and hydrated transparent silica, obtained by action of oxalic acid on alkaline silicates.—There are also several mathematical papers and reports on prize competitions (subjects chiefly botanical).

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xii. fasc. ii.—We note the following papers in this number:—Considerations on a letter of Tyndall's regarding heterogeny, by Prof. Giovanni.—On the causes of asphyxia and the agglutination of the blood corpuscles in diphtheria, by S. Trevison.—The Sanitary Office of the German Empire, by Dr. Zucchi.—Studies on milk (continued), by Drs. Pirotta and Riboni.—On cortical psycho-sensory centres, by Professors Luciani and Tamburini.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, March 13.—C. W. Merrifield, F.R.S., president, in the chair.—Mr. J. D. H. Dickson was admitted, Mr. R. Hargreaves and Prof. W. E. Story were elected, and Mr. Donald McAlister was proposed for election into the Society.—Prof. Cayley, F.R.S., spoke briefly but in high praise of the late Prof. Clifford's work as a mathematician, instancing more particularly his papers "On the Canonical Form and Dissection of a Riemann's Surface," "On Mr. Spottiswoode's Contact-Problems," and "The Classification of Loci."—The chairman, the Rev. A. Freeman, and Dr. Hirst, F.R.S., added a few remarks on the loss the Society and the mathematical world generally had sustained, and expressed the hope that steps would be taken to secure the publication, if desirable, of any mathematical papers Prof. Clifford might have left.—Dr. Hirst made a statement respecting the "De Morgan Memorial" Medal to be presented to the Society to be awarded in such manner as the council shall hereafter determine; it appeared that the bust and die for the medal had been executed by Mr. Woolner, and that after all claims had been met there would still be a small sum required to make up the requisite total for the purpose contemplated. The late Prof. De Morgan was the first president of the Society and always took a warm interest in its advancement. It was resolved that a subscription list should be opened in order that old pupils and members of the Society might have an opportunity of aiding in the above design. Subscriptions for this special purpose may be sent to Mr. Tucker (Hon. Sec., University College School, W.C.), or to Mr. Alfred Wills, Q.C., 12, King's Bench Walk, E.C., the Hon. Sec. to the general fund. Copies of the medal were exhibited (Profile with dates of birth and death, on the reverse, Pascal's hexagram, surrounded by the "Zodiac of Syllogisms," and the title President of the London Mathematical Society).—The following communications were made:—On differential equations, total and partial, and on a new soluble class of the first and an exceptional case of the second, by Sir J. Cockle, F.R.S.—Discussion of two double series arising from the number of terms in determinants of certain forms, by Mr. J. D. H. Dickson.—Two geometrical notes relating to surfaces of the second order, by Prof. H. J. S. Smith, F.R.S.

Physical Society, March 8.—Prof. W. G. Adams in the chair.—Dr. Hurst and Mr. Jacob were elected Members.—Prof. Ayrton brought forward a new theory of terrestrial magnetism originated by himself and Prof. Perry of the Imperial Engineering College, Japan. It is well known that metal cages act as screens against induction in the case of static electricity or electricity at rest, and hence Clerk Maxwell, at the British Association meeting for 1876, suggested that no earth connection was necessary for lightning conductors, since a cage would be sufficient. But dynamic electricity is different from static in this respect, and Professors Ayrton and Perry found that even a thick block of copper will not screen a coil of wire from the induction of a *current* flowing in a neighbouring one. Some experiments of Dr. Muirhead, not yet published, would seem to favour the view that a current is a series of intermittent changes of potential, and that the inductive effect was due to a difference in the epochs of the currents in the two coils. It was found by Helmholtz that a quantity of static electricity in mechanical

motion performs work. Conversely Mr. Crookes finds that the stream of molecules from a − pole *in vacuo* is electrified, and may be deflected by a magnet. It is upon that fact that Professors Ayrton and Perry have based their theory, which is easily explained by supposing the earth to be an isolated sphere with a static charge residing on its surface. Then, since each electrified particle at the surface will be moving relatively to a point in the interior, it follows that the interior must be magnetic. The theory is independent of the substance of the interior; but in order to simplify the working the authors treated the case of a solid iron ball, and curiously enough arrived at the result expressed by Biot's law for the distribution of magnetism on the surface of the earth—

$$I^2 = M \sqrt{1 + 3\cos^2\theta},$$

and similarly they found that if the earth were electrified to the potential of 10^8 volts, relatively to interplanetary space, its magnetisation would be as it is. If the earth were alone in the universe, then, by this theory, it would have its own magnetic state by virtue of its electric charge and axial rotation. If other bodies in the universe, however, had their magnetic states too, these would influence the earth's, and hence we should have terrestrial tides and storms of magnetic force, such as are known to exist, as, for instance, when changes take place in the sun's atmosphere by approach of planets or other causes. Lastly, the iron in the interior of the earth may give it a certain amount of coercitive force, but the theory does not rest on this.—Dr. J. Hopkinson then read an account of some experiments made with the quadrant electrometer, which showed that Clerk Maxwell's formula for the sensibility of the electrometer—

$$(A - B)\left(C - \frac{A + B}{2}\right),$$

where A and B are the potentials of the two pairs of quadrants, and C the potential of the needle, only holds good when C (the charge of the jar or needle) is less than 200 Daniell elements. Above that a different law appears to hold. Dr. Hopkinson also remarked that any degree of low sensibility down to zero could be got from the electrometer by connecting a condenser to each pair of quadrants and adjusting their capacities.—Mr. F. D. Brown described his apparatus for maintaining constant temperatures and pressures. A constant temperature can be obtained if the pressure can be kept constant. The vessel in which the constant pressure is desired communicates with an air-pump by a pipe in which a movable tap or valve is placed. By opening or closing this tap the pressure is regulated. This is effected by an electric clutch arrangement. A mercury anemometer sends a positive or negative current from a battery through the clutch according as the pressure is too high or low, and this current actuates the clutch to close or open the valve. The clutch consists of an axle driven by a turbine to get power to work the valve, and the current, by means of electromagnetism, connects the tap to the axle, which then opens or closes it as the case may be. In this way a pressure varying no more than one-fifth millimetre each way can be obtained.

Linnean Society, March 6.—William Carruthers, F.R.S., vice-president, in the chair.—Mr. Thos. Christie exhibited and made remarks on a series of specimens illustrating the little-known and remarkable Australian Pituri plant; also the *os sepia* of a rare Australian cuttle-fish, obtained by Dr. Bancroft.—Mr. R. Irwin Lynch showed a growing example from Kew and dried leaves of *Xanthosoma appendiculatum*, bearing pouch-like excrescences from the midrib of the leaves.—The Vice-President announced from the chair an alteration in the Bye-laws, Chap. XIII., proposed by the Council.—A letter was read from a correspondent referring to the increased production of beet-root sugar by careful artificial selection of the beet. The saccharine produce of sugar-cane, on the contrary, remains stationary, if not retrograde, and its continual multiplication from stolons some regard as giving rise to various diseases. Crossing and selection are now suggested as worthy of a trial in the interest of commercial results.—A short paper on Entozoic Florideæ growing within living Bryozoa and Sponges, by Dr. P. F. Reinsch, was read, and Mr. A. W. Waters exhibited in connection therewith, under the microscope, specimens of Polyzoa containing parasitic algæ.—In a note on the fruiting of *Wistaria sinensis* in Europe, by Mr. W. T. Thiselton Dyer, the author avers from his own and others' observations that plants trained on a garden wall at Glyon, east end of the Lake of Geneva, yield abundance of brown tomentose pods annually. Near the town of Geneva, however, fruiting is of rarer occurrence, but again more frequent at Lyons and the Rhone valley. Fruiting, he suggests, may be a question of temperature and not of nutrition, dependent on presence or absence of support to the stem and branches. From the above and other data, Mr. Dyer fails to see the evidence of the antagonism of the vegetative and reproductive forces, as asserted to be the governing law, according to Mr. Thos. Meehan's experiments, and lately quoted by the Rev. G. Henslow. If such barrenness were the case with its scandent habit, then *Wistaria sinensis* would probably already be extinct.—The Secretary read a paper by Mr. Edward J. Miers, on the classification of the Maioid crustacea or Oxyrhyncha. The Maioid crabs have been placed by nearly all carcinologists at the head of the Brachyura, from the high degree evinced in their sensory organs and nervous system, and the group, moreover, is interesting on account of the variety of types. Though closely related to the Oxystomata, the Oxyrhyncha differ from them in their triangular buccal cavity and position of afferent branchial channel; but Mesorhœa approximates on the part of the Parthenopidæ to the Oxystomatous type. From the Cancroid crabs (Cyclometopa) typical Maiidæ are distinguished by longitudinal antennules and position of basal antennule joint, the Parthenopidæ occupying an intermediate place between the rest of the Oxyrhyncha and certain Cancroidea. The author reviews the various classifications, and then gives a new synoptical arrangement founded on certain anatomical configurations, &c., of their buccal cavity, situation of afferent and efferent canals, antennules, genital appendages, &c. He divides the group into 4 families, 12 sub-families, 106 genera, and 14 sub-genera, giving short diagnoses of each.—Prof. J. Reay Greene, Dr. P. H. Stokoe, Mr. R. Johnston (of Tasmania), Mr. B. S. Williams, and Prof. J. Wood Mason, were balloted for and elected Fellows of the Society.

Entomological Society, March 5.—J. W. Dunning, vice-president, in the chair.—The chairman referred to the great loss sustained by the Society in the death of Mr. F. Smith, of the British Museum.—Mr. C. Brogniart, of Paris, was elected a Foreign Member and Mr. J. T. Harris, of Burton-on-Trent, a Subscriber to the Society.—Sir Sydney Saunders exhibited a series of bees belonging to the genus *Halticus*, from Greece, containing several remarkable new forms. The following papers were communicated:—On some new species of British hymenoptera, by Mr. Peter Cameron, and descriptions of some new species of coleoptera from New Zealand, by Dr. Sharp.

CONTENTS

THURSDAY, MARCH 27, 1879

ORGANISATION AND INTELLIGENCE

Habit and Intelligence: a Series of Essays on the Laws of Life and Mind. By Joseph John Murphy. Second Edition, illustrated, thoroughly revised, and mostly rewritten. (London: Macmillan and Co., 1879.)

Life and Habit. By Samuel Butler. (London: Trübner and Co., 1878.)

THE first edition of Mr. Murphy's work was reviewed in NATURE, vol. i. pp. 288 and 315, a little more than nine years ago, and on reading the article we find little or nothing in the remarks and criticisms then made which require modification on account of subsequent discoveries. The present work is, however, very largely new, about one-third of the matter in the first edition, which treated of physical questions, being omitted, and replaced by a series of new chapters on biological subjects. It is to these new chapters that we shall mainly confine our present notice.

Chapter XI. gives a very good summary of the facts of variation from Darwin's "Domestication of Animals and Plants," and other works; and in Chapter XII. we have these facts discussed in regard to the sufficiency of natural selection for the origin of species. The greatest use is here made of the argument (said to be Prof. Tait's) in the *North British Review* (June, 1877), and which another writer has summed up as follows:—"The final establishment of the superior type is dependent at each step upon three accidents. First, the accident of an individual sort or variety better adapted to the surrounding conditions than the then prevailing type; secondly, the accident that this superior animal escapes destruction before it has time to transmit its qualities; and thirdly, the accident that it breeds with another specimen good enough not to neutralise the superior qualities of its mate." Put in this way, the difficulty staggers most persons who are not practical naturalists; yet it has always seemed to me to be really beside the question, and by no means of the importance which Mr. Darwin himself has given to it by acknowledging that the argument had not occurred to him. Even acute writers like Mr. Murphy do not see that individual variations or "sports" are of no importance whatever to the theory of natural selection, or he would never bring forward the argument at p. 380, that with an animal born of two parents "there is an almost overwhelming probability that the favourable variation is found only in one," and will therefore diminish in each succeeding generation till it disappears, unless the same favourable variation recurs again and again to counteract this tendency. In what may be termed normal variation, however (which Mr. Darwin has always considered the main agent in supplying materials for natural selection), none of these difficulties occur, and as it is very important to make this clear, I will give a few illustrations of it. There is no part, organ, or character of an animal or plant but what is sometimes more sometimes less developed in different individuals. The whole population of a species in any given year may therefore be divided into two equal portions, with regard to any such organ or character—the less developed and the more developed. Thus, for example, all the foxes of the species *Canis vulpes* are necessarily divisible into a lighter and a darker coloured group; into a longer and a shorter tailed group; into a fleeter and a less fleet group; into a group with more developed and less developed canine teeth; and so on with regard to every character, external and internal. This can only be denied by asserting that there are characters which in the species in question are *absolutely unvarying*, an assertion which I am not aware that any one has made or attempted to prove, while it is certainly contradicted by the observations of all who have ever studied nature.

But if so, what happens when changed conditions occur, rendering the increased development of some faculty or organ beneficial? Can any one doubt that the one or five, or twenty per cent. of individuals which annually survive will belong, wholly or almost wholly, to the moiety in which that organ or faculty is better developed rather than to that in which it is worse developed? It matters not at all whether the *most* perfect individual or the twenty most perfect individuals survive or not; but the survivors will certainly be found among the better adapted rather than among the worse adapted half, and most likely will include a majority of individuals in the better half of the better half. And this process will be repeated *every year* without fail. There is thus no waiting for favourable variations to occur; no series of coincident improbable accidents is required; but the process goes on continuously with ever increasing power owing to the influence of heredity, till the species is modified up to the requirements of the changed conditions. By this process, leading to a decided advance *every year*, we can quite understand how any dominant species (that is, one which occupies a wide area and has a large population) may be modified quite as rapidly as is required by all ordinary changes of conditions, although extraordinary changes may lead to the extinction even of dominant species. It is hardly possible to conceive any improvement or modification of a species which might not be brought about by so powerful a selection as this, acting on variations which seem to us very trivial; while, on the other hand, the effect of greater individual variations or "sports" is very uncertain, and may perhaps never be used in nature as a means of modifying species.

These considerations also show the true bearing of "Delbœuf's Law," to which Mr. Murphy attaches much importance. It is proved mathematically that if, in any species, several individuals are in every generation born with any particular variation which is neither beneficial nor injurious to its possessors, and if the effect of the variation is not counteracted by reversion, the proportion of the new variety to the original form will constantly increase until it approaches indefinitely near to equality. But as, in every species, there are not one only, but hundreds of distinct variations in every generation, all subject to change in amount and direction in each succeeding generation, and as each of these will by the above law tend to equality with all others, the result must be that every slight recurrent variation will maintain itself in the species on terms of approximate equality with all other variations; and this will evidently be useful, by keeping up a vast stock of slightly varied forms within the species, which will be ready at any moment to furnish the material

on which selection may work when variations of a particular kind are needed.

The next three new chapters, on "Fixation of Characters," on "Effect of Change of Conditions," and on "Mimicry, Colour, and Sexual Selection," contain much interesting matter, with a number of suggestions of difficulties mostly dependent upon our total ignorance of the peculiar conditions or laws under which certain characters first arose. Such difficulties are of little importance, because they are always liable to disappear with an increase of knowledge. What, for instance, is the value of such a criticism as this: "The abnormal position of the left carotid artery in some groups of parrots is as good an instance as can be mentioned of a character which is constant throughout an entire group, which *must* have arisen suddenly, and *cannot* have been fixed by natural selection, because it *cannot* be useful." The three words I have italicised mark three positive statements which cannot possibly be verified, and which may very probably all be wrong. The parrots form not a *family* only, but a very distinct *order* of birds, and, from the occurrence in the miocene of France of a parrot of a living genus, are probably of immense antiquity. Not only do we know nothing of their early history, but, owing to their so rarely breeding in this country we know nothing of their embryology, and can therefore have no grounds for assertions as to what could or could not have been in the remote past, when they were developing into the varied forms that now exist, under conditions of which we are perfectly ignorant.

Another new chapter, on "Metamorphoses and Metagenesis" gives an interesting outline of the metamorphosis of insects, crustacea, and hydroids, illustrated by a number of excellent figures, and remarks on the difficulty of explaining many of the facts by variation and natural selection, the conclusion being that "many of the transformations, especially among the crustacea and the hydrozoa, do not consist in adaptations to any new or special mode of life, and consequently cannot be accounted for by the Darwinian or any similar theory, but must be due to a formative impulse impressed on living matter at the beginning."

The next chapter, on "Structure in Anticipation of Function," is not so good as some of the others, and here again objections are brought forward whose whole weight depends on our ignorance of the conditions under which certain structures were modified. Thus, it is said to appear impossible to account for the transition from the fin of *Ceratodus* to the simple fin-ray of *Lepidosiren* by any means which Darwinism admits, because it seems impossible that the loss of the membranes of its fins can be beneficial to a fish. But in this case there seems to be a difference of habits which may show how the "impossible" occurred. The *Lepidosiren* of the Gambia burrows in the mud, where it remains during the dry season, and for this "burrowing" the cylindrical rays may be better adapted than the broad fins of *Ceratodus*.

In the chapter on the "Origin of Man" Mr. Murphy replies to my argument that the brain of savage man is an instrument beyond his needs, by pointing out that "the real superiority of man consists in the faculty of language, and that the mental power implied in this unique faculty is represented by the very great excess in the size of the human brain over that of the highest apes;" and he goes on to say: "If, then, the Darwinian theory is true of man, the difference between the highest ape and that of the lowest man is due to the exercise of the brain during the period while the power of language was in process of evolution, aided by the natural selection of the largest brains, in which, of course, this new power would be most highly developed." This appears to me a very forcible objection, and I must acknowledge that it is "a sufficient answer" to my argument, so far as regards the difference between the brain of savage man and apes. The question remains, however, of the latent powers in the brain of savages; and Mr. Murphy maintains that the languages of many savages—of the Kafirs, for example, are so much in advance of their needs that they could not have been evolved by natural selection. In most other respects he agrees with the arguments in my essay on "The Limits of Natural Selection as applied to Man."

This concludes the new matter in the physiological part of the book; but before passing on to the psychological portion, I must notice one passage embodying a very common source of confusion as regards the geographical distribution and mode of origin of species. Referring to the marine lizard of the Galapagos, *Amblyrhynchus*, Mr. Murphy remarks: "This singular species is found nowhere except in the Galapagos, and consequently has, most probably, been evolved there; but on Darwinian principles, how can so peculiar and aberrant a form have been evolved during the geologically short time that has passed since these islands first rose above the ocean?" The difficulty thus raised, with many analogous cases, I have endeavoured to explain in the February issue of the *Nineteenth Century*. The idea that this peculiar lizard has been "evolved," in the Galapagos really implies spontaneous generation; for what was it evolved out of? A remote ancestral form *must* have reached the islands from the main land, if there is to be any "evolution" in the case, and if a remote why not a near ancestor? It appears to me, not a mere probability but almost a certainty, that the generic type, if not the actual species, was "evolved" in America; that it was once an abundant, and, in fact, a dominant group; that it then spread to the Galapagos; that the entire group then died out on the main land, but was preserved in the islands, *owing to the absence of enemies and competitive forms*. On this principle almost all the supposed difficulties of geographical distribution may be rationally explained; and this mode of explanation is in accordance with palæontological evidence whenever it is procurable.

The remaining chapters form the psychological part of the work, in which the author develops his theory of the organising intelligence in animal forms. The only new chapter here is that on "Automatism," in which the various questions connected with the automatic motions of plants and animals, and with the instincts and the habits of animals, are set forth and commented on; and here it is very interesting to compare the conclusions arrived at with those of Mr. Butler in his very original and suggestive book on "Life and Habit."

Mr. Murphy says that the actions of a sea-anemone in seizing on its prey with its tentacles, or in closing itself

when left uncovered by the receding tide, are probably purely automatic, and completely independent of sensation, consciousness, or will; and further, that there is probably no difference whatever between these motions and those of the leaves of *Dionæa* and *Drosera* which crush insects to death and suck their juices. But though independent of consciousness they are not independent of intelligence: they are instinctive, and instinct is intelligence unconscious of itself. All such actions as these are classed as *primarily* automatic, having no relation to consciousness; but there are also actions which are *secondarily* automatic, which were once conscious actions but have become unconscious through habit. These habits may become hereditary, forming instincts, and can then in some cases not be distinguished from primary automatism. Elsewhere he speaks of "a principle of intelligence which guides all organic formation and all motor instincts, and finally attains to consciousness in the brains of the higher animals, and to self-consciousness in the brain of man."

We will now turn to Mr. Butler's work, and see how he deals with these and analogous facts. He first discusses acquired habits, showing that, as we do things more and more frequently we do them with less thought and effort, till at last, when we do them perfectly we also do them unconsciously. He then shows that the same law applies to knowledge and beliefs, which are only complete and unwavering, when we have ceased to doubt or to think of reasons or facts in support of them, when, in fact, they have become unconscious. We then come to habits acquired at or soon after birth, as walking, or eating, which, though they continue to be voluntary, are often performed quite unconsciously. Swallowing and breathing, though very complex acts, are acquired by the infant a few minutes after birth, and thence performed unconsciously, and we endeavour to explain this by the terms "hereditary instinct," and the "experience of the race." Mr. Butler concludes that these terms are unmeaning, and that, because we see that all actions when performed sufficiently often become automatic, we ought to conclude, whenever we see actions performed automatically, that there *has* been this repeated performance of them somehow or other. He thus sums up his facts on this phase of the question: "We are most conscious of, and have most control over, such habits as speech, the upright position, reading and writing, which are acquisitions peculiar to the human race, and always acquired after birth. We are less conscious of, and have less control over, eating and drinking, swallowing, breathing, seeing, and hearing, which were acquisitions of our prehuman ancestry, but which are still, geologically speaking, comparatively recent. We are most unconscious of, and have least control over, our digestion and circulation, which belonged even to our invertebrate ancestry, and which are habits, geologically speaking, of extreme antiquity." These principles are then applied to a great variety of facts in biology with extreme and, as some may think, perverted ingenuity, of which we can only give a single illustration: "We say of the chicken that it knows how to run about as soon as it is hatched. So it does; but had it no knowledge before it was hatched? What made it lay the foundations of those limbs which should enable it to run about? What made it grow a horny tip to its bill before it was hatched, so that it might pick all round the larger end of the egg-shell, and make a hole for itself to get out at? And is it in the least agreeable to our experience that such elaborate machinery should be made without endeavour, failure, perseverance, intelligent contrivance, and practice? In the presence of such considerations it seems impossible to refrain from thinking that there must be a closer continuity of identity, life, and memory between successive generations than we generally imagine." This is the "unconscious organising intelligence," says Mr. Murphy, ultimately becoming conscious in the complete animal. "It is the result of often repeated conscious acts," says Mr. Butler, "which are now performed unconsciously after countless repetitions."

At first sight we seem to have here only the "ancestral experience" which has already been objected to as unmeaning. But this difficulty is overcome by the strange assumption that "it is the same chicken which makes itself over and over again; for such unconscious action is not won, so far as our experience goes, by any other means than by frequent repetition of the same act on the part of one and the same individual." Let no reader throw the book aside on coming to this astounding sentence, till he has read the two succeeding chapters on "Personal Identity," which are full of curious facts and subtle reasoning, and which lead to the conclusion that life is the one great personality, of which all living things are but differentiated offshoots still retaining a latent memory of a long succession of ancestral habits and experiences. This idea is carried further in the next chapter, on "Our Subordinate Personalities," in which it is shown that the highest authorities maintain the distinct individuality of the countless cells or physiological units of which our bodies are composed, and Mr. Butler remarks: "With the units of our bodies it is as with the stars of heaven, there is neither speech nor language, but their voices are heard among them. Our will is the *fiat* of their collective wisdom as sanctioned in their parliament, the brain; it is they who make us do whatever we do. When the balance of power is well preserved among them, when they respect each other's rights, and work harmoniously together, then we thrive and are well; if we are ill, it is because they are quarrelling among themselves, or are gone on strike for this or that addition to their environment, and our doctor must pacify or chastise them as best he may."

Passing on to Chapter IX.—on the "Abeyance of Memory"—it is shown that we remember best two classes of phenomena, either very unfamiliar objects or combinations—as if we were once in our lives shipwrecked on an iceberg, or very familiar object or acts, which produce their effect by repetition. These last, however, are apt to become unconscious, or to be wholly lost sight of, except when the usual conditions call them up, an amusing illustration of which is given as follows:—"Men invariably put the same leg first into their trousers—this is the survival of memory in a residuum; but they cannot, till they actually put on a pair of trousers, remember which leg they *do* put in first; this is the rapid fading away of any small individual impression." It is on the same principle that every act of growth of cells

and organs is said to be unconsciously remembered, when the same or analogous conditions recalls it to the dormant memory.

In another very ingenious and suggestive chapter entitled "What we might expect," it is maintained that the preceding facts and principles lead up to and explain all the curious phenomena of growth, reproduction, variation, and heredity, as set forth in the works of Darwin, Spencer, and other writers; and the same principles are applied in succeeding chapters to the phenomena of instinct, and the theories of Lamarck, Darwin, Mivart, and others. The argument is then summed up, and the conclusion arrived at that "Life is that property of matter whereby it can remember. Matter which can remember is living; matter which cannot remember is dead. The life of a creature is the memory of a creature. We are all of the same stuff to start with, but we remember different things. As for the stuff itself of which we are made, we know nothing save only that it is 'such as dreams are made of.'"

Such a brief notice as this can give no adequate idea of the originality and the logical completeness of Mr. Butler's remarkable work, which is far less known than it deserves to be. It may be truly said of it that it is more amusing than most novels, while it contains more material for thought than is to be found in most books of double the size. It will be seen that there is a certain agreement with Mr. Murphy, but Mr. Butler goes much further, in tracing the former writer's vague and unlocalised "unconscious intelligence" to the physiological elements of all organisms; and, however wild and improbable the theory may seem, it receives, strange to say, considerable support from the views of Haeckel and other German physiologists of the most advanced school. If the reader will turn to NATURE, vol. xix. p. 115, he will find Haeckel maintaining that "in the *Infusoria* a single cell performs all the different functions of life, including the mental functions." . . . . "By the same right by which we ascribe an independent 'soul' to these unicellular *Infusoria*, we must ascribe one to all other cells, because their most important active substance, the protoplasm, shows everywhere the same psychic properties of sensitiveness (sensation) and movability (volition). The difference in the higher organisms is only that there the numerous single cells give up their individual independence, and like good state citizens, subordinate themselves to the 'state-soul,' which represents the unity of will and sensation in the cell-association."

We have here an extraordinary agreement with Mr. Butler, although, as we are informed, he was quite unacquainted with Haeckel's works when he wrote his book; and this fact should induce us to give a more careful consideration to the views of a writer who, although professedly ignorant of all science, yet possesses "scientific imagination" and logical consistency to a degree very rarely found among scientific men. The want of a practical acquaintance with natural history leads the author to take an erroneous view of the bearing of his own theories on those of Mr. Darwin. There is really nothing to prevent their harmonious combination, and they may even be said to be in great part complementary to each other. Mr. Butler's book is so full of strange fancies and witty conceits, as to have led some readers to look upon the whole as an elaborate jest. Beneath this sparkling surface there is, however, much solid matter, and though we can at present only consider the work as a most ingenious and paradoxical speculation, it may yet afford a clue to some of the deepest mysteries of the organic world.

ALFRED R. WALLACE

RODWELL'S ETNA

Etna: a History of the Mountain and of its Eruptions. By G. F. Rodwell, Science Master in Marlborough College. With Maps and Illustrations. Pp. 142. (London: C. Kegan Paul and Co., 1878.)

IN this little volume Mr. Rodwell has essayed to do for Etna that which the late Prof. Phillips accomplished so successfully in the case of Vesuvius, namely, to write a popular and at the same time accurate account of the past and present conditions of a mountain, which from the very earliest periods to which human history and tradition go back, has powerfully arrested the attention and excited the imagination of mankind. The scope and aim of these two works being so nearly the same it is hard to avoid drawing a comparison between them.

The first and fifth chapters of the work of Mr. Rodwell, which deal with the past history of the mountain and the record of its eruptions, indicate much learning and careful research on the part of the author, and indeed these portions of his volume may compare not unfavourably with the equivalent parts of Prof. Phillips' work; higher praise than this can scarcely be given to it. Almost equally praiseworthy are the second and fourth chapters, which give a general sketch of the physical features of Etna and an account of the origin, the past history, and the present condition of the numerous towns which are crowded about the flanks of the great volcano. The third chapter, giving details concerning the author's own ascent of the mountain, though sufficiently interesting in itself, is perhaps better fitted for the pages of a popular journal than of a work like the present, since ascents of Etna are now sufficiently common and every-day occurrences.

It is when we come to the more purely scientific portions of the volume that a comparison of the work of Mr. Rodwell with that of Prof. Phillips places the former in such a disadvantageous light. It is rather startling to find the more general and popular descriptions occupying five chapters, including 113 pages, while the account of the geology and mineralogy of the mountain is condensed into a single chapter of 29 pages, and when these pages are read we cannot help feeling that the questions treated of in them are handled in a somewhat imperfect and perfunctory manner. Any one turning to a treatise professing to deal with the geology and mineralogy of Etna might fairly expect to find a fuller and clearer account than Mr. Rodwell gives us of the exact relations of the volcanic masses to the stratified and highly fossiliferous deposits with which they are so intimately associated. Equally disappointing is it to find that the important question of the elevation hypothesis of von Buch is so summarily dealt with by Mr. Rodwell, especially when we remember that in the discussion on this subject which took place between Élie de Beaumont and Dufrenoy

on the one hand, and Scrope and Lyell on the other, Etna supplied so crucial a test. Nor can we regard the few notes of Mr. Rutley on the microscopic characters of several specimens brought to him by the author, excellent as they are in themselves, as affording anything like an adequate discussion of the nature of the Etnean lavas.

There are not, indeed, wanting indications in the work before us that the author has scarcely succeeded in so far mastering the scientific questions connected with his subject as to qualify himself for giving anything like authoritative opinions concerning them. Thus on page 111 we find him speaking of a crater as "composed of a prehistoric grey labradorite, and of doleritic lava." Again, so far as can be gathered from the work before us, the hypotheses of elevation craters and eruption craters are of about equal value. We are informed simply that "the opinion of geologists is divided as to the manner in which a volcano is formed;" and then follows a statement of the two rival hypotheses. Surely after the convincing reasoning of Scrope, and the patient observations of Lyell on Etna itself, as detailed in the celebrated memoir read before the Royal Society in 1858, it is strange to find such language used upon the subject, more especially when we recollect that no attempt was ever made by Lyell's opponents to discredit his observations or to reply to his deductions. We should almost as soon expect to read in a modern work on astronomy that the opinion of astronomers is divided as to whether the earth moves round the sun or the sun round the earth.

We find so much to praise in this little book, especially in the clear *résumé* of the history of the mountain and its eruptions, and the illustrations so carefully selected and reduced from those of larger works which are not easily accessible to general readers, that we regret we cannot express more unqualified approbation of that portion of the book which calls for especial notice in the pages of this journal. We can only hope that in a second edition the author may find an opportunity, which he will not neglect, of considerably lengthening and very greatly strengthening this scientific portion of his work; and in order to do so, without at the same time impairing its popular character, we can scarcely suggest a better example for him to follow than the work of Prof. Phillips, to which we have alluded at the commencement of this article.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Trans-Neptunian Planet

THE explanation given by Prof. Peters (*Astr. Nach.*, 2,240) of the observations made at Washington in 1850 of this supposed planet is put beyond doubt by the examination of Mr. Ferguson's observing-book. It is due, however, to Mr. Ferguson to say that his record is full and complete, and that his changes in the reductions were honestly made. The record is in pencil, and no figures were erased or rubbed out. They are crossed out, and the assumed figure is put by the side of the original one, while at the bottom of the page is a note with pen and ink, and in Mr. Ferguson's handwriting, stating the changes that were made. Prof. Peters's ingenious discovery of the truth was made without knowledge of the observing-book.

Such criticisms are instructive, showing how unsafe it is to build theories before we are sure of the facts. They may also be a means of avoiding a waste of labour. It is known to me that at least two American astronomers, armed with powerful telescopes, have been searching quite recently for a trans-Neptunian planet. These searches have been caused by the fact that Prof. Newcomb's tables of Uranus and Neptune already begin to differ from observation. In this connection the note of Mr. Dunkin on the errors of Leverrier's tables of Saturn is interesting. But are we to infer from these errors of the planetary tables the existence of a trans-Neptunian planet? It is possible that such a planet may exist, but the probability is, I think, that the differences are caused by errors in the theories of these planets. My observations of the satellites of Saturn are not yet discussed, but they indicate that Bessel's mass of Saturn is nearly correct. Now Leverrier has diminished this mass by about $\frac{1}{115}$th, and it seems probable that this diminution was caused by some error in his theories of Jupiter and Saturn.

A few years ago the remark was frequently made that the labours of astronomers on the solar system were finished, and that henceforth they could turn their whole attention to sidereal astronomy. To-day we have the lunar theory in a very discouraging condition, and the theories of Mercury, Jupiter, Saturn, Uranus, and Neptune, all in need of revision; unless, indeed, Leverrier's theories of the last two planets shall stand the test of observation. But after all, such a condition of things is only the natural result of long and accurate series of observations which make evident the small inequalities in the motions, and bring to light the errors of theory.

Washington, March 7 ASAPH HALL

Rats and Water-Casks

MR. NICOLS says, in NATURE, vol. xix. p. 433:—

"A ship's carpenter told me that, in the old days, before the use of iron tanks on board ship became general, the rats used to attack the water-casks, cutting the stave so thin that they could suck the water through the wood without actually making a hole in it. If any one could substantiate this it would have an important bearing on the question under consideration."

Capt. Wickham, when First Lieutenant on board H.M.S. *Beagle*, told me that when he was a midshipman it was his duty, on one of the king's ships to see that certain vessels on deck were always kept full of water, in order to prevent the rats gnawing holes through the water casks, and that through such holes nearly all the water in a cask would leak away.

CHARLES DARWIN

Tides at Chepstow

I OBSERVE two letters in NATURE lately upon this subject. Many years ago I took some pains to ascertain the greatest known rise of tide at Chepstow, for I doubted the accuracy of the common statement that it was seventy feet and upwards. At the time I made the inquiry the large railway bridge at Chepstow to carry the South Wales Railway across the River Wye was being constructed. I was acquainted with Mr. Oakden, one of the engineers on the work, and he, with great care, took levels of the marks which had been made from time to time recording the very high tides, some of them going back many years. He found the highest of them to be some decimal (of which I have no record) above fifty feet above ordnance datum. I think this may be relied upon. It is corroborated in a paper by the present Astronomer-Royal, on "Tides and Waves," in the "Encyclopædia Metropolitana," vol. v. p. 242, paragraph 7, first edition. He says: "Thus, at the entrance of the Bristol Channel the whole rise at spring-tides is about eighteen feet, at Swansea about thirty feet, and at Chepstow about fifty feet." W. B. CLEGRAM

Saul Lodge, Gloucestershire, March 18

Migration of Birds

PROF. NEWTON in his article on Migration of Birds (NATURE, vol. xix. p. 433) has omitted one, and a very important limit to the height at which birds of passage can perform their journeys. This is *temperature*. The following table of Daniell's will show

how little probability there is of migratory birds flying at great elevations, and that even in low latitudes, the temperature at altitudes exceeding four and a half miles would be prohibitory to the existence of the majority of migrants :—

Altitude.	Temp. F.	Temp. F.
′	°	°
0	+ 80	0
5,000	+ 64·4	− 18·5
10,000	+ 48·4	− 37·8
15,000	+ 31·4	− 58·8
20,000	+ 12·0	− 82·1
25,000	− 7·6	− 109·1
30,000	− 30·7	− 140·3

Calculations by Mr. Glaisher's rule for approximate temperature (decrease of 1° F. for every 300 feet elevation) give less startling results than the above, but even then, with the thermometer marking 80° at sea level, we find that a temperature of 40° of frost must exist at five miles in height.

The advocates of the "sight theory" have rather more in their favour than Prof. Newton has conceded to them. It is not necessary that birds should fly at such heights as to literally view the land they guide their course to or by. The "loom" of land, so well known to sailors, is visible when the land itself is below the horizon; and I do not think we are entitled to say that birds would not, equally with mariners, notice the indication. Then again the action of one flock of birds when in sight of land, might guide other more distant flocks, and these might influence birds still further off. We know how the circling downward swoop of a vulture on some discovered carrion will draw to the feast vultures from all parts of the sky. We know the power of our own vision, certainly inferior to that of many birds; and it is therefore well within the bounds of possibility that migrating birds, watchful because weary and hungry, may see and be influenced by the movements of flocks of their companions thirty to forty miles distant. A few flocks might thus bridge a wide expanse of barren ocean.

It is not necessary, however, to insist that sight alone is the guiding faculty in migration. The majority of, if not all, animals possess that marvellous "sense of direction" that has become so blunted in civilised man. Both savages and lower animals will find their way back in a "bee-line" through unknown country, to places whence they have been led by tortuous tracks. Why should not this "sense of direction" then guide birds over oceans without landmarks. The case of first migration of young birds (cuckoos and starlings) quoted by Prof. Newton, is, it must be confessed, a problem difficult to solve; but when the journey has been once made by an individual bird in a flock I cannot see more mystery in the arrival of that flock at their destination than there is in the perfect accord between the hand and the eye of a good shot or a good billiard player.

We must all concur with Col. Donnelly in desiring further observations, with facility for publication and discussion, and I venture to hope that we shall see many more papers from Prof. Newton's pen on the subject. E. H. PRINGLE

Scientific Club, Savile Row, March 18

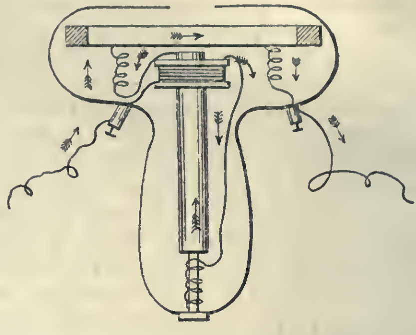
The Microtelephone

TOWARDS the end of last year I got constructed a telephonic apparatus which gives results much superior to those of the ordinary Bell telephone. Its construction is based on four principles, two of which have not yet been applied to telephones :—

1. The magneto-electric principles of the Bell telephone.
2. The microphonic principles of Hughes (different quantity of the points of contact).
3. The principle discovered by Beatson and De La Rive (1845), and which explains the experiments of Messrs. Blyth and Hughes with the speaking microphone (production of sounds by the passage alone of a discontinuous or undulating electric current).
4. The principle that the intensity of the sound depends on the density of the air in which it is produced.

All the principles are combined in so simple a manner that the microtelephone differs from the Bell telephone only in the three following points :—

1. The electric current engendered by the approach or withdrawal of the iron membrane, traversed not only the bobbin, but also the magnet and the membrane itself.
2. The communication of the current with the vibrating plate of iron is effected by means of two small springs, which are lightly pressed by the membrane, and as this pressure may be more or less strong during the action of the apparatus, the latter acts as a microphone of a relatively weak sensitiveness, but which permits the telephone to be spoken to at a distance of several centimetres, and of hearing the ticking of a watch, or the sounds of a musical box with the aid of a carbon microphone.



3. Three millimetres above the iron membrane is another membrane of caoutchouc (which should not be very fine), and both membranes inclose a layer of air, moderately compressed, which in this way must vibrate, together with the two membranes.

The microtelephone is regulated once for all, aud transmits the feeblest word with a truly perfect precision.

JULIAN OCHOVOWICZ

University, Lemberg, Galicia

Vacuum Tube Phenomena

HAS it been observed that the area of the exposed surface of the negative electrode in a highly exhausted vacuum tube exerts an important influence on the facility with which the discharge takes place?

I have recently been observing Crookes's molecular shadows with a tube constructed by Mr. J. Marr, of Liverpool, in which one electrode is a flat disk about 1 inch in diameter, and the other a piece of platinum wire about ⅜ inch long. When these electrodes are connected with the terminals of an induction coil capable of giving a 4½-inch spark in air, and the contact-breaker arranged so that the shadows can just be seen when the disk is in connection with that terminal which becomes negative when the current in the primary wire is broken, a reversal of the commutator causes the discharge to cease.

If, now, the coil power be increased by the proper manipulation of the contact-breaker, a condition of things is reached in which the dark shadows flash out intermittently, even though the disk is connected with what is called the positive terminal of the induction coil.

This is evidently caused by the passage of the inverse induction current; I mean that current which is produced when the circuit of the primary is completed. It thus appears that a condition of things can be obtained in which the effect of the greater electromotive force produced on the breaking of the primary circuit is counterbalanced by the influence of the relative size of the electrodes.

The above observation appears to be interesting, and as it may possibly be new, I venture to send you an account of it.

Nottingham, March 8 J. J. H. TEALL

Leibnitz's Mathematics

PROF. TAIT has recently given your readers one mathematician's opinion of Leibnitz as a discoverer. The following extract is serviceable in the same direction, while it has the further merit of attesting to the existence of a still later "vestige of presumption" than has yet been referred to. The extract is from a review by M. Bertrand of Dühring's "Kritische Geschichte der allgemeinen Principien der Mechanik" (Berlin, 1873). M. Bertrand says :—

Les sévérités de M. Dühring sont impartiales, et l'un des plus grands génies de l'Allemagne semble précisément le plus maltraité de tous. Les *Actes* de Leipzig, de 1684, donnèrent, est-il dit dans le texte, la première publicité à la théorie des fluxions de Newton, et en note, on ajoute: "Il n'a pas été possible d'opposer à Leibnitz des preuves complètes qui le

forçassent à avouer son emprunt ; mais la connaissance de son caractère donne à l'acte qu'on lui reproche une probabilité voisine de la certitude. Une lettre d'Huyghens à L'Hospital est sur ce point fort instructive.

"'M. Leibnitz,' dit Huyghens, 'est assurément très habile, mais il a avec cela une envie immodérée de paroître, comme cela se voit, lorsqu'il parle de son Analyse des infinis . . . , des lois harmoniques des mouvemens planétaires, où il a suivi l'invention de M. Newton, mais en y meslant ses pensées qui la gastent . . . Encore suis-je fort en doute, pour des raisons que je pourrois alléguer, s'il n'a pas tiré sa construction (de la chaînette) de celle de M. Bernoulli.'

"Dans la préface de son 'Calcul différentiel,' Euler n'attribue à Leibnitz que la réduction des principes de Newton en système. Lagrange, qui cherche chez Fermat l'origine du Calcul différentiel, ne manque pas, dans ses 'Leçons sur le Calcul des Fonctions,' de signaler les concordances de l'écrit de Leibnitz de 1684, avec la théorie antérieure de Fermat. Gauss pensait, comme on le voit dans l'écrit de Sartorius de Waltershausen, que Leibnitz, même de loin, ne doit pas être comparé à Newton."

It is satisfactory to quote Herr Dühring through his reviewer, because the introductory sentence of the extract makes evident to those not already aware of M. Bertrand's historical leanings that it is on Dr. Ingleby's side the eminent French mathematician would give his voice. THOMAS MUIR

High School of Glasgow, March 17

Blue Flame from Common Salt

SOME time ago the question was raised in NATURE (vol. xiii. p. 287) concerning the origin of the blue flame produced when common salt is thrown into a hot fire.

Among the suggestions that were advanced, no one offered the only explanation that is at all feasible, viz., that it is due simply to hydrochloric acid.

The blue flame is not produced by sodium chloride only, but by other chlorides as well. Those I have tried are $BaCl_2$, $SrCl_2$, KCl, AmCl, Hg_2Cl_2, and HCl, the last both in solution and as gas.

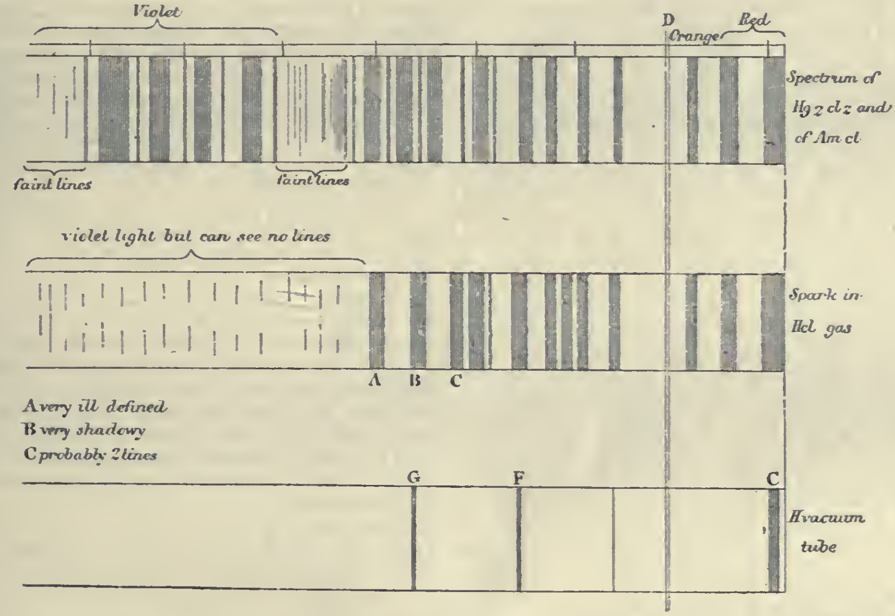
It would be waste of time and space to enumerate all the experiments I have made on this subject ; many of them were for the purpose of proving that neither carbon nor sulphur had any share in the reaction.

One of my methods of obtaining the flame was to burn pure hydrogen from a glass jet, and allow a mingled stream of HCl and NH_3 from two other jets to pass into it. The best source, however, is calomel, heated on wire gauze by a Bunsen burner ; the next best, AmCl.

The spectrum of the "chloride" flame is characterised by a series of double bands in the green, blue, and violet, the least refrangible of each pair being the broadest. The four pairs in the violet are especially prominent. There are two red bands or lines, and one orange. The least refrangible red line occupies the place of the hydrogen line C.

A spark between carbon points in a bottle of HCl gas yields a spectrum similar in appearance to that obtained from a chloride, but I was unable to see any violet bands, only a faint continuous spectrum.

I was able to ascertain that the red lines coincided exactly,



but I cannot affirm with the same positiveness that all the green lines and bands coincide. Some undoubtedly do, but the spectrum of the HCl was so faint, and the spectrum of the chloride so transient, that measurements were very difficult, and I do not pretend to any high degree of accuracy in my delineations of the lines, besides the different conditions under which the two were compared might account for considerable variations. For the HCl I used a coil (capable of giving a 2" spark) with Leyden jar, worked by six Smees. The carbon points were ½ inch apart. The spark was tried both focussed and unfocussed on the slit of a 2-prism spectroscope. When comparing the two spectra side by side it was difficult in some cases to be sure of coincidence, because the flame from the Hg_2Cl_2 would flash out brilliantly for a moment and quite overpower the more feeble lines of the HCl ; it would then disappear entirely, and more chloride would have to be placed on the gauze.

I have no doubt in my own mind that HCl is the cause of the blue flame. I have proved that Cl is a necessary constituent, and I have not been able to get it in the absence of hydrogen (the spectrum of pure Cl is very different), and besides the red H-line is present in both cases, and probably the other two as well. I do not think that the presence of aqueous vapour is sufficient to account for the red line.

I subjoin the spectra *as I mapped them*, but it must be borne in mind that I do not vouch that they are free from error. I intend to photograph them when I have sufficient leisure, and I hope the results will be more definite. I may be able to find violet lines in the spectrum of HCl.

A. PERCY SMITH

Temple Observatory, Rugby, March 15

Unscientific Art (?)

IN NATURE, vol. xix. p. 460, Mr. Buck complains of the drawing in the *Graphic* for December 28, wherein the observer is represented as "sloping the barometer at an angle of about 30° from the vertical," in order to take a reading on a marine barometer by means of the lantern for better illumination. May not the artist be correct, and Mr. Buck have discovered a mare's nest? The barometer may be placed entirely horizontal for reading the scale, *after* the vernier has once been set when the instrument was vertical.

CHAS. COPPOCK

Grosvenor Road, Highbury New Park, N., March 21

OUR ASTRONOMICAL COLUMN

THE DISTANT HERSCHELIAN COMPANION OF γ LEONIS.—In 1861 Prof. Winnecke, writing from Pulkowa, drew attention to a star of the ninth magnitude near the double-star γ Leonis, which M. Otto Struve had found to have an annual proper motion exceeding 0″·5. The star was observed with the Dorpat transit-instrument, on April 12, 1820, and once by Bessel in zone 502, on April 12, 1831, and from these observations compared with two at Pulkowa in April, 1861, and with micrometrical measures from γ Leonis by M. Otto Struve, Prof. Winnecke concluded that the proper motion of the small star with respect to the neighbouring binary, was very nearly 0″·85 in R.A. and 0″·10 in declination, annually. Sir W. Herschel observed a distant companion of γ Leonis, the mean of two angles giving 297°·5 for about 1782·9, with a distance of 111″·4, which he thought was "pretty accurate," though as we are now aware, many of these wider measures of Sir W. Herschel require material correction.

We refer to this star from having remarked that M. Flammarion, in his recently published "Étoiles Doubles et Multiples en Mouvement relatif certain," has made it the subject of a strangely confused article, which is calculated to mislead the reader who cannot refer to original authorities. The star had been measured by Secchi in 1856, and by Powell in 1861, and M. Flammarion states that "the enormous difference between the measure of 1782 and that of 1856" had induced him to search for other observations and to reobserve it himself, which he did, in 1877. He says he had found five observations by Flamsteed in 1691, ten by T. Mayer in 1755, and fifteen by C. Mayer in 1777; these, it is added, are not very precise, for they consist only of differences of right ascension, without taking account of the declination; nevertheless he considered they had their value, and comparing his own measures of 1877 with previous observations separately, he deduces "a very surprising result," viz., that the distant star is remarkable for its motion, which, if one may judge by the totality of observations, has a mean value of 1″·08, but which appears variable, as "at present it certainly has not that value."

The main cause of M. Flammarion's difficulty is his having confounded two quite distinct objects: we have not referred to the work of C. Mayer, but the star observed by Flamsteed, which he more than once calls "Comes γ," and that observed by Tobias Mayer, is really the bright neighbour of γ, or 40 Leonis; Flamsteed did observe the declination, as will be seen in his column "Distantiæ a vertice correctæ;" and Mayer also noted the declination on one occasion, though generally recording only the right ascension. M. Flammarion says he found five observations of Flamsteed in 1691, which is a greater number than we recognise in the "Historia Cœlestis," but there are observations in 1690 and 1692. The zenith distances of γ Leonis and *Comes* on April 6, 1691, and the names of the stars on January 23, 1692, are interchanged in the "Historia Cœlestis." Tobias Mayer's observations do not apply to the year 1755, when his observatory at Göttingen was not yet erected, but to 1756 and 1757, chiefly the former year. Bessel's observation applies to 1831, not 1825, as M. Flammarion assumes.

The star in question is No. 90, in Argelander's valuable treatise, "Untersuchungen über die Eigenbewegungen von 250 Sternen, &c.," where he deduces for the annual proper motion in arc of great circle, 0″·512 in the direction 270°, or the proper motion is entirely in R.A. He observed the star upon the meridian at Bonn, once in 1857 and four times in 1862-63. It was also meridionally observed at Greenwich in 1862. It is No. 234, Hour X., in Weisse's Bessel. Thus we have three stars situate within half a degree, with large proper motions, very divergent, however, in direction:—

	Secular P.M.	Direction.	Authority.
	″	°	
40 Leonis	32·2	229·0	Mädler.
W. B. X. 234	51·2	270·0	Argelander.
γ Leonis	32·3	118·6	Mädler.

A METEOR WITH SHORT PERIOD OF REVOLUTION.—In the very interesting report of the "Luminous Meteor Committee" of the British Association for 1877-78, we find a note by Capt. G. L. Tupman, referring to a fire-ball seen on November 27, 1877, which he considers to have been moving in a nearly circular orbit, with short periodic time. Capt. Tupman observed this meteor from a position about half a mile east of the Royal Observatory, Greenwich; it began as a first or second magnitude star, but suddenly increased in brilliancy and size to a fine bluish white fire-ball ten or twelve minutes in diameter, emitting a train, coloured blue, red, and green, many degrees long. It moved very slowly, so slowly, indeed, towards the end of its course, that it appeared to come almost to a standstill. The duration was considered to be fifteen or sixteen seconds. The meteor was observed by Mr. H. Corder, at Writtle, near Chelmsford, and by Mrs. Ware, at Clifton Down, Bristol, and the positions for beginning and ending, estimated at these stations, were found to be in remarkable agreement, the true path deduced from these satisfying them all, both azimuths and altitudes, within 1°.

It appears that the meteor first became visible at a real height of fifty-six miles vertically over a point off the mouth of the Thames in long. 1° 21′ E., lat. 51° 33′, and disappeared when it had descended to a height of thirteen miles vertically over a point, about twelve miles west of St. Omer, in France, in long. 2° 0′ E., lat. 50° 45′, the length of the entire path being about eighty miles.

Capt. Tupman thinks the radiant point was pretty accurately determined in R.A. 285°, Decl. + 64°, or in longitude 340°, and latitude + 83°. The elements of the real orbit, which, with the aid of the other corresponding data depending upon the earth's position in her orbit, are thence deduced, are as follows, taking the real duration at fifteen seconds:—

Perihelion distance	0·9858	Excentricity ...	0·1568
Longitude of perihelion ...	70° 6′	Inclination ...	15° 0′
,, ,, ascending node	245° 50′	Anomaly ...	−4° 16′
Semi-axis major	1·1691	Periodic time...	462 days

Motion—direct.

The precise Greenwich time of the occurrence of the meteor was 10h. 26m.

If the duration of visibility is diminished to 7½ seconds the elements are still very similar to the above; the semi-axis major becomes 1·3785 and the period 591 days. Capt. Tupman remarking that such favourable conditions for inferring the orbit of a meteor may rarely happen, adds, it is sufficient for the establishment of a short periodic time (such as 500 days) that "the meteor moved slowly from a fairly well-determined radiant distant about 90° from the point of the heavens towards which the earth's motion was directed."

We may mention that there is one singular circumstance not alluded to in Capt. Tupman's note: the elements defining the position of the orbit of the meteor

have a striking general resemblance to those of the orbit of Biela's comet, in the descending node of which body the earth was precisely situated at the time.

FOSSIL CALCAREOUS ALGÆ

THE very important memoir of M. Munier-Chalmas, "Sur les Algues calcaires appartenant au groupe des Dasycladées Harv. et confondues avec les Foraminifères," which was published in the *Comptes Rendus Hebdomadaires* of the French Academy of Science for October 29, 1877, opens up quite a new or almost a new field of research, which has been followed up by the same author in a note presented last month to the Geological Society of France, "On the genus Ovulites." Though regarded by some of the most eminent palæontologists as a monothalmic foraminifer related to Lagena, the genus Ovulites is herein clearly demonstrated to be neither more nor less than an articulation of a siphonaceous alga having very close affinities to Penicellus.

Ovulites margaritula is described by Messrs. Parker and Jones "as a common Foraminifer of the 'Calcaire grossier.' Shaped like an egg, and when full grown about the size of a mustard-seed, it is one of the most elegant of the fossil forms. The large terminal apertures, moreover, curiously impress upon the mind its resemblance to a 'blown' bird's egg. [Written in 1860; nowadays bird's eggs are not thus blown.] It is the largest of the monothalamous Foraminifera. As a species it appears to have been short-lived. Fully developed in the deposits of Hauteville and Grignon it breaks in at once in the Eocene period. It lingers as an attenuated form in the Miocene beds of San Domingo. A recent Ovulite has not been met with. Scarcely another Foraminifer presents us with a similarly brief history—an undescribed form allied to Dactylopora affording almost the only parallel (namely, *Acicularia pavantina*, d'Arch.)."

In passing it may be noted that without doubt this last-mentioned form is also only a portion of a calcareous alga.

The earlier memoir, of which the *Comptes Rendus* publishes only an abstract, reminds us that it is not so very long ago (1842) since Prof. Decaisne demonstrated that a number of marine forms known as zoophytes, Corallina, Cymopolia, Neomeris, Penicellus, Udotea, Halimeda, &c., were in reality veritable algæ. But we may remark that Prof. Schweigger, of Königsberg, had, from actual observation of living specimens of several species of these calcareous algæ at Villefranche, come to the same conclusion in 1818 ("Beobachtungen auf naturhistorischen Reisen. Anat.-phys. Untersuchungen über Corallen," Berlin, 1818). To go back to the pre-Linnean times, Ray (1690) described Corallina as "plantæ genus in aquis nascens," and Spallanzani, Carolini, and Olivi even maintained the same against the peculiar reasonings of Ellis, the authority of Linneus, and despite the conversion of Pallas; but so influenced by authority were, apparently, the botanists down to 1842, that a Professor of Botany in the Edinburgh University (Graham) once politely requested the zoologists to keep *their* cryptogamia to themselves, and a Professor of Botany in the Dublin University (Harvey), in the first edition of his "Manual of British Algæ" (1841), did not include any of the Corallines. Since the memoirs of Decaisne and Chauvin, all this has changed, and we imagine that there is now no difference of opinion existing among botanists as to the general affinities of the living forms of calcareous algæ.

M. Munier-Chalmas in his memoir demonstrates that there must be also added to this group a numerous series of fossil forms which the old authors placed among the polyps, and which most of the modern writers on the subject have ranked among the foraminifera. Bosc in 1806 described and figured (*Journal de Physique*, Juin 1806) some fossil organised bodies under the name of *Rétéporites ovoides*, for which bodies Lamarck in 1816 established the genus Dactylopora. "The most singular varieties of opinion have existed," writes Dr. Carpenter in his well-known "Introduction to the Study of the Foraminifera," "as to the true character of these fossil organisms. In separating them generally from Retepora Lamarck still associated them in the same group of supposed zoophytes; this position was also accepted for the genus by De Blainville and Defranc." [It is but justice to De Blainville to point out that he quotes without disapproval the statement of Schweigger, "que les dactylopores et les ovulites ne sont rien autre chose que des articulations d'une grande espèce de cellaire, analogue à la cellaire salicorne"]. "In 1852 Dactylopora was included among the Foraminifera by D'Orbigny, who showed, notwithstanding, by the place he assigned to it, a misapprehension of the real nature scarcely less complete than that under which his predecessors had lain; for he ranks it in his order Monostègues, next to the unilocular Ovulites, and says of it: 'C'est une Ovulite également percée des deux bouts, pourvue des larges pores placés par lignes transverses.' How utterly erroneous is this description will appear from the details to be presently given, yet D'Orbigny's authority has given it currency enough to cause it to be accepted by such intelligent palæontologists as Pictet and Bronn, who in the latest editions of their respective treatises have transferred Dactylopora to the place indicated by him, not, however, without the expression of a doubt on the part of Bronn as to whether the true place of the genus is not among the Fistulidæ in alliance with Synapta and Holothuria—a suggestion that indicates a perversion of ideas on the subject for which it is not easy to account. The complex structure of the organism in question was first described and the interpretation of that structure on the basis of an extended comparison with simpler forms was first given, by Messrs. Parker and Jones in so unobtrusive a manner as scarcely to challenge the attention which their investigations deserve, and I gladly avail myself of the opportunity which the present publication affords to give a fuller account, with the requisite illustrations of this remarkable type, the elucidation of which seems to me not unlikely to lead to a reconsideration of the place assigned to many other organisms at present ranked among Zoophytes or Polyzoa;" and then follow nine pages of a most elaborate description of every ridge and furrow, of every elevation and depression to be met with in any of the so-called species, so that probably no single vegetable cell was ever before so minutely described.

The genus is placed the eleventh in order of the family Miliolida, a family which contains some of the most typical of Foraminifers. "It may be conjectured without much improbability," writes Dr. Carpenter, "that Dactylopora is only the single representative of a group whose various forms filled up the hiatus which at present intervenes between itself and its nearest allies among the ordinary Foraminifera." But, writes M. Munier-Chalmas, "the study and comparison of species of Dasycladus, Cymopolia, Acetabularia, Neomeris, &c., in the herbarium of the museum, and in that of M. Ed. Bornet, who placed without reserve at my disposal his library and collections of these plants, proved to me that the species of Dactylopora, Acicularia, Polytrypa, &c., are decidedly Algæ, very nearly allied to species of the recent genera just quoted, if not identical therewith. The accompanying figures show plainly, for example, that the genera Cymopolia and Polytrypa may be united; for the typical species thereof offer in every respect the same generic characters, and there is even a difficulty to find for them sufficiently distinct specific characters. Under the denomination of '*Siphoneæ verticillatæ*' I unite (1) Those green-spore bearing algæ arranged by Harvey in the family of the Dasycladeæ; (2) All those fossil gener related to Larvaria, Clypeina, Polytrypa, Acicularia, Dac-

tylopora, and Uteria. This group at present contains over fifty genera, which are for the most part to be met with in the triassic, jurassic, cretaceous, and tertiary strata. In the number of those actually living there is a notable falling off, there being not more than the seven following genera :—Dasycladus, Halicoryne, Cymopolia

FIG. 1.—Transverse section of a morsel of the calcareous tube of *Cymopolia rosarium*, Lamr., showing the canals which receive the whorl of cellules and the central sporangial cavity. FIG. 2.—Transverse section of *Polytrypa elongata*, Defrance, showing the same portions. FIG. 3.—Part of a whorl of cellules of *Cymopolia rosarium*, separated from the calcareous tube by acid. A, wall of central cellule; B, first row of cellules; C, terminal whorl of cellules, in the centre of which is D, the axillary sporangium. FIG. 4.—Exactly the same parts in *Polytrypa elongata*, obtained from a mould.

(with two sub-genera, Polytrypa and Decaisnella,[1] g.n.), Polyphysa, Acetabularia, Neomeris, and Bornetella,[2] g.n." [Doubtless a few more genera of recent forms yet remain to be described. Thus Chloroclados, of Sonder, appears to be a good and distinct genus allied to Dasycladus.]

"The frond in the *Siphoneæ verticillatæ* is simple or dichotomous ; it consists of a central tubular unicellular axis, around which are arranged the radiary and verticellate ramuli, the exact arrangement of which varies according to the genera and to the species. In most of the species carbonate of lime is found deposited in abundance in the outer walls of the main axis and its ramuli, and this forms around the plant a calcareous envelope, in which is reproduced all the details of its organisation. This mineral coating may consist of one or of two calcareous cylinders. The inner cylinder will be formed by the central axis, and the first row of cells which arise therefrom. The outer cylinder is laid down by the most external of the verticels of cells ; these terminate by a splayed-out enlargement, the lateral edges of which become more or less consolidated with the similar enlargements of neighbouring cells, and by thus causing a reciprocal pressure, very regular hexagonal surface markings are produced. The organs of fructification are themselves surrounded by calcareous material, and assist in the formation of the outer cylinder, a fact easily seen in any section of Cymopolia."

The result of such an organisation is that when the organic vegetable matter becomes destroyed there still remains in those fossil species, which laid down a great deal of calcareous material, as well as in those living species—which lay down more or less of it—a skeleton permeated by canals (rays of the ramuli) and chambers (fructification). This arrangement, which permits of an exact classification of the fossil species, being wrongly interpreted, led even some most distinguished authors to see in these morsels of plants the full organisation of a Foraminifer."

Space will not permit of the table of the twenty-two genera and seven families as detailed in the *Comptes Rendus* being here given, but every botanist will look forward with interest to the promised future detailed contributions of the author on this subject. Here it seems desirable to add that his conclusions are in every particular acquiesced in by one in every way thoroughly able to judge of the facts, Dr. Ed. Bornet, and having written this I feel it almost superfluous to say that on a careful study myself of specimens prepared by M. Munier-Chalmas—for which I take this opportunity of thanking him—I cannot conceive his demonstration as admitting of a doubt.

ED. PERCEVAL WRIGHT

[1] Type, *Dactylopora eruca*, Parker.
[2] Type, *Neomeris nitida*, Harv. MS.

ELECTRICITY AND WATER DROPS [1]

IT has been known for many years that electricity has an extraordinary influence upon the behaviour of fine jets of water ascending in a nearly vertical direction. In its normal state a jet resolves itself into drops, which even before passing the summit, and still more after passing it, are scattered through a considerable width. When a feebly electrified body is brought into its neighbourhood, the jet undergoes a remarkable transformation, and appears to become coherent ; but under more powerful electrical action the scattering becomes even greater than at first. The second effect is readily attributed to the mutual repulsion of the electrified drops, but the action of feeble electricity in producing apparent coherence has been a mystery hitherto.

It has been shown by Beetz that the coherence is apparent only, and that the place where the jet breaks into drops is not perceptibly shifted by the electricity. By screening various parts with metallic plates, Beetz further proved that, contrary to the opinion of earlier observers, the seat of sensitiveness is not at the root of the jet where it leaves the orifice, but at the place of resolution into drops. As in Sir W. Thomson's water-dropping apparatus for atmospheric electricity, the drops carry away with them an electric charge, which may be collected by receiving the water in an insulated vessel.

I have lately succeeded in proving that the normal scattering of a nearly vertical jet is due to the *rebound* of the drops when they come into collision with one another. Such collisions are inevitable in consequence of the different velocities acquired by the drops under the action of the capillary force, as they break away irregularly from the continuous portion of the jet. Even when the resolution is regularised by the action of external vibrations of suitable frequency, as in the beautiful experiments of Savart and Plateau, the drops must still come into contact before they reach the summit of their parabolic path. In the case of a continuous jet the "equation of continuity" shows that as the jet loses velocity in ascending, it must increase in section. When the stream consists of drops following the same path in single file, no such increase of section is possible, and then the constancy of the total stream requires a gradual approximation of the drops, which in the case of a nearly vertical direction of motion cannot stop short of actual contact. Regular vibration has, however, the effect of postponing the collisions and consequent scattering of the drops, and in the case of a direction of motion less nearly vertical may prevent them altogether.

Under moderate electrical influence there is no material change in the resolution into drops, nor in the subsequent motion of the drops up to the moment of collision. The difference begins here. Instead of rebounding after collision, as the unelectrified drops of clean water generally or always do, the electrified drops *coalesce*, and thus the jet is no longer scattered about. When the electrical influence is more powerful, the repulsion between the drops is sufficient to prevent actual contact, and then of course there is no opportunity for amalgamation.

[1] "The Influence of Electricity on Colliding Water Drops." Paper read at the Royal Society by Lord Rayleigh, F.R.S.

These experiments may be repeated with extreme ease and with hardly any apparatus. The diameter of the jet may be about $\frac{1}{20}$ inch, and may be obtained either from a hole in a thin plate or from a drawn-out glass tube. I have generally employed a piece of glass tube fitted at the end with a perforated tin plate, and connected with a tap by india-rubber tubing. The pressure may be such as to cause the jet to rise eighteen or twenty-four inches, or even more. A single passage of a rod of gutta-percha, or of sealing-wax, along the sleeve of the coat is sufficient to produce the effect. The seat of sensitiveness may be investigated by exciting the extreme tip only of a glass rod, which is then held in succession to the root of the jet and to the place of resolution into drops. An effect is observed in the latter but not in the former position. Care must be taken to use an electrification so feeble as to require close proximity for its operation, otherwise the discrimination of the positions will not be distinct.

The behaviour of the colliding drops becomes apparent under instantaneous illumination. I have employed sparks from an inductorium, whose secondary terminals were connected with the coatings of a Leyden jar. The jet should be situated between the sparks and the eye, and the observation is facilitated by a piece of ground glass held a little beyond the jet, so as to diffuse the light; or the *shadow* of the jet may be received on the ground glass, which is then held as close as possible on the side towards the observer.

If the jet be supplied from an insulated vessel, the coalescence of colliding drops continues for a time after the removal of the influencing body. This is a consequence of the electrification of the vessel.* If the electrified body be held for a time pretty close to the jet, and be then gradually withdrawn, a point may be found where the rebound of colliding drops is re-established. A small motion *to* or *from* the jet, or a discharge of the vessel by contact of the finger, again induces coalescence.

Although in these experiments the charges on the colliding drops are undoubtedly of the same name, it appeared to me very improbable that the result of contact of two equal drops, situated in the open, could be affected by any strictly equal electrifications. At the same time an opposite opinion makes the phenomena turn upon the very small *differences* of electrification due either to irregularities in the drops or to differences of situation, and is at first difficult of acceptance in view of the efficiency of such very feeble electric forces. Fortunately I am able to bring forward additional evidence bearing upon this point.

When two horizontal jets issue from neighbouring holes in a thin plate, they come into collision, for a reason that I need not now stop to explain, and after contact they frequently rebound from one another without amalgamation. This observation, which I suppose must have been made before, allowed me to investigate the effect of a passage of electricity across two contiguous water surfaces. The jets that I employed were of about $\frac{1}{15}$ inch in diameter, and issued under a moderate pressure (5 or 6 inches) from a large stoneware vessel. Below the place of rebound, but above that of resolution into drops, was placed a piece of insulated tin plate in connection with a length of gutta-percha-covered wire. The source of electricity was a very feebly excited electrophorus, whose cover was brought into contact with the free end of the insulated wire. When both jets played upon the tin plate the contact of the electrified cover had no effect in determining the union, but when only one jet washed the plate union instantly followed the communication of electricity, and this notwithstanding that the jets were already in communication through the vessel. The quantity of electricity required is so small that the cover would act three or even four times without being re-charged, although no precautions were taken to insulate the reservoir.

In subsequent experiments the colliding jets, about $\frac{8}{100}$ inch in diameter, issued horizontally from similar glass nozzles, formed by drawing out a piece of glass tubing and dividing it with a file at the narrowest part. One jet was supplied from the tap, and the other from the stoneware bottle placed upon an insulating stool. The sensitiveness to electricity was extraordinary. A piece of rubbed gutta-percha brought near the insulated bottle at once determined the coalescence of the jets. The influencing body being held still, it was possible to cause the jets again to rebound from one another, and then a small motion of the influencing body *to* or *from* the bottle again induced coalescence, but a *lateral* motion without effect. If an insulated wire be in connection with the contents of the bottle, similar effects are produced when the electrified body is moved in the neighbourhood of the free end of the wire. With care it is possible to bring the electrified body into the neighbourhood of the free end of the wire so *slowly* that no effect is produced; a sudden movement of withdrawal will then usually determine the coalescence.

Hitherto statical electricity has been spoken of; but the electromotive force of even a single Grove cell is sufficient to produce these phenomena, though not with the same certainty. For this purpose one pole is connected through a contact key with the interior of the stoneware bottle, the other pole being to earth. If the fingers be slightly moistened, the body may be thrown into the circuit, apparently without diminution of effect. This perhaps ought not to surprise us, as in any case the electricity has to traverse several inches of a fine column of water. On the other hand, it appeared that most of the electromotive force of the Grove cell was necessary.

Further experiment showed that even the discharge of a condenser charged by a single Grove cell was sufficient to determine coalescence. Two condensers were used successively; one belonging to an inductorium by Ladd, the other made by Elliott Brothers, and marked "Capacity $\frac{1}{3}$ Farad." Sometimes even the "residual charge" sufficed.

It must be understood that coalescence of the jets would sometimes occur in a capricious manner, without the action of electricity or other apparent cause. I have reason to believe that some, at any rate, of these irregularities depended upon a want of cleanness in the water. The addition to the water of a very small quantity of soap makes the rebound of the jets impossible.

The last observation led me to examine the behaviour of a fine vertical jet of slightly soapy water: and I found, as I had expected, that *no scattering took place.* Under these circumstances the approach of a moderately electrified body is without effect, but a more powerful influence scatters the drop as usual. The apparent coherence of a jet of water when the orifice is oiled was observed by Fuchs, and appears to have been always attributed to a diminution of adhesion between the jet and the walls of the orifice.

Some further details on this subject, and other investigations respecting the phenomena of jets, are reserved for another communication, which I hope soon to be able to present to the Royal Society; but I cannot close without indicating the probable application to meteorology of the facts already mentioned. It is obvious that the formation of rain must depend very materially upon the consequences of encounters between cloud particles. If encounters do not lead to contacts, or if contacts result in rebounds, the particles remain of the same size as before; but, if the issue be coalescence, the bigger drops must rapidly increase in size and be precipitated as rain. Now, from what has appeared above, we have every reason to suppose that the results of an encounter will be different according to the electrical condition of the particles, and we may thus anticipate an explanation of the remarkable but hitherto mysterious connection between rain and electrical manifestations.

A STUDY IN LOCOMOTION[1]

III.

It would be very interesting to pass in review the principal epochs of art and trace out the manner of representing horses in motion in various periods of progress or decadence. But such a study would fail to realise its full value unless carried out by an artist.

Col. Duhousset, who joins the skill of the draughtsman to a perfect acquaintance with the exterior of the horse, has essayed a work of this kind, and in a recent publica-

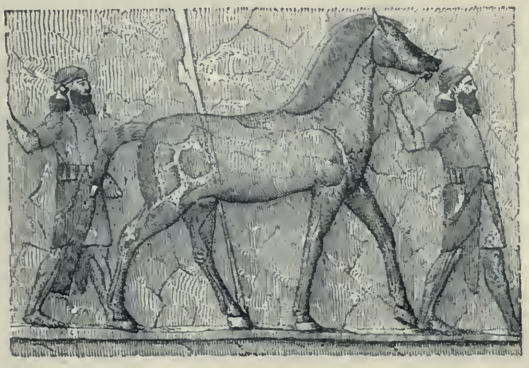


FIG. 20.—Assyrian bas-relief (British Museum). A horse ambling.

tion has pointed out the merits and defects of certain modern artistic performances. M. Duhousset has also gathered together a curious collection of representations of the horse at different epochs of art, and has entrusted to me a few specimens, which I will now exhibit to you. You will see that in a general manner art has progressed, proceeding from simple forms to more masterly delineations.

Figs. 20 and 21 represent horses ambling. Has the artist selected this pace because it was in general use at

FIG. 21.—Egyptian bas-relief (Medinet-Abou). Two horses attached to a chariot, and ambling.

that time? This is scarcely probable; it would seem more likely that he has chosen it because of its extreme simplicity. To seize the moment when the four feet all touch the ground, to repeat in the posterior limbs the attitude of the limbs in front, and, lastly, to represent all the horses harnessed together as keeping exact time in their movements so as to draw them all with a single

FIG. 22.—"The Cavalier and Death," by Albrecht Dürer. Horse at the trot.

profile: this is certainly one way of eluding all the difficulties of the situation.

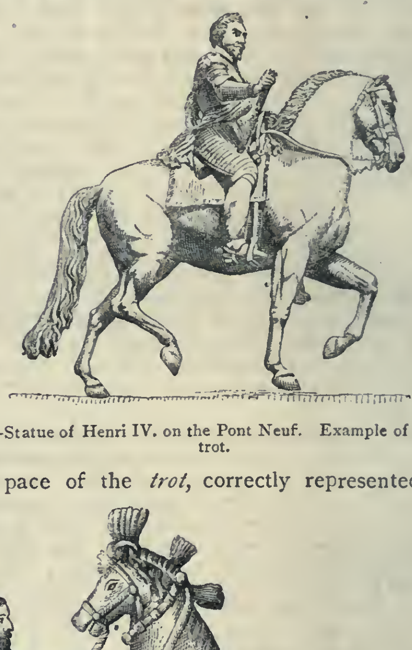


FIG. 23.—Statue of Henri IV. on the Pont Neuf. Example of the correct trot.

The pace of the *trot*, correctly represented in the

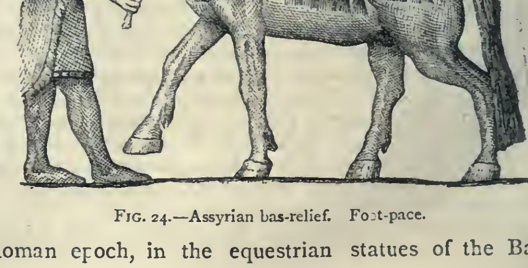


FIG. 24.—Assyrian bas-relief. Foot-pace.

Roman epoch, in the equestrian statues of the Balbi

[1] "Moteurs animés; Expériences de Physiologie graphique." Lecture by Prof. Marey at the Paris meeting of the French Association, August 29, 1878. Continued from p. 467.

which adorn the Naples Museum, reappears in the six-

FIG. 25.—Bas-relief in baked earth, of the Volscian epoch (Velletri). Three horses harnessed together, walking at a foot-pace.

teenth century in a painting by Albrecht Dürer (Fig. 22,

FIG. 26.—Captain of the Guards riding at a foot-pace. (Column of Trajan.)

"The Cavalier and Death"). The classic statue of

FIG. 27.—Mule laden with baggage, walking at a foot-pace. (Column of Trajan.)

Henri IV. on the Pont Neuf is an example of the correct trot (Fig. 23).

But the representation of the *foot-pace*, more difficult than the preceding, is rarely faithful. Examples, scarcely satisfactory, are found in all the epochs; take, for instances, Figs. 24 and 25.

The foot-pace is correctly represented in the two figures borrowed from the column of Trajan (Figs. 26 and 27). This column also displays oxen and other animals faithfully represented.

These paces, it should be noted, are a little varied with respect to the instant chosen; almost invariably the horse raises only one fore-foot.

The *gallop* is in general the pace of which the representation leaves most to be desired. Without speaking of contemporary art, I will refer only to the paintings of the two or three last centuries. The horses therein deemed to be galloping are represented in a sort of prancing attitude, posed upon the two hind-feet, and raising the two fore-feet to an equal height. We have seen, by the preceding notations, that this synchronous action of the right and left limbs does not exist.

In the grandest epoch of Greek art we find admirable representations of the gallop. Fig. 28 is an example. The attitude chosen is the first step of the gallop as in Fig. 16. The first step has been taken. The diagonal

FIG. 28.—Frieze of the Parthenon. (Bas-relief remaining at Athens.) Right hand gallop.

limbs which make the second step are approaching towards the ground, and the right hand fore-foot which will make the last step is held high in air.

I have already admired the reproduction in plaster of another bas-relief from the same frieze, in which a galloping horse is represented with equal correctness, and I was led to believe that in the age of Phidias, artists were in possession of the science of paces. But subsequently, in examining the reproductions of the entire frieze, I have become convinced that the results were obtained by a happy chance, for the greater part of the horses are represented in false attitudes, which is all the more to be regretted in contemplating the exquisite elegance of their forms.

It is incontestable that at the present day, artists make great efforts to represent the horse with truthfulness, and many among them succeed. But I will not permit myself to criticise the works of my contemporaries. Such, then, is the graphic method, and such are its numerous applications, extremely varied, and often of enormous importance. In this discourse, the length of which you will excuse, I have only shown you a little corner of the subject, but that will suffice, I hope, to give you a desire to study more deeply, and in its entirety, a method which appears to me to be full of promise, and to the development of which I have already consecrated much effort.

GEOGRAPHICAL EVOLUTION[1]

IN the future development of scientific geography one of the main lines of advance will be in the direction of a closer alliance with geology. The descriptions of the various countries of the globe will include an account of how their present outlines came into existence, and how their plants and animals have been introduced and distributed. The principles on which this evolutional geography will be founded have regard to the materials of which the framework of the land consists, to the various ways in which these materials have been built up into the solid crust of the earth, and to the superficial changes to which they have been subsequently exposed. The materials of the land consist mainly of compacted detritus, which, worn from previously existing terrestrial surfaces, has been laid down in the sea. Hence the land, as we now see it, has originated under the sea. But the common belief that over the whole globe land and sea have been continually changing places, and that wide continents may have bloomed even over the site of the most lonely abysses of the ocean, may be shown to be incorrect by a consideration of the character of the sedimentary rocks of the land on the one hand, and of that of the deposits of the sea-floor on the other. The sedimentary rocks, even in the most massive palæozoic formations where they attain depths of several miles, are shallow-water deposits, formed out of the waste of the land and always laid down near land. Nowhere among them, even including the thick organically-derived limestones, such as the chalk, is there any formation which properly deserves to be considered that of a deep sea. Recent researches into the nature of the sea-bottom across the great ocean-basins have likewise shown that the deposits there in progress have no real analogy among the rocks of the land. The conclusion to be drawn from the evidence is that the great ocean-basins have always existed, and that the terrestrial areas have also lain on the whole over those tracts where they still exist.

The way in which the sedimentary rocks have been tilted up and made to lie discordantly on each other shows that the marginal belt of sea-floor near the land has again and again been upraised and worn down. The ocean-basins appear from very early times to have been areas of subsidence, while the continental elevations have been lines of relief from the strain of terrestrial contraction. The land has been subjected to periodic movements of upheaval, sometimes of great violence, whereby not only large areas of sea-bottom were raised into land, but where, as huge earth-waves, lines of mountain-chain were ridged up. During these movements great changes were effected in the structure and arrangement of the rocks in the regions affected, original sedimentary masses being rendered crystalline, and even reduced to such a pasty or fluid condition as to be squeezed into rents of the more solid superincumbent rocks. Volcanic orifices were likewise opened, by which communication was established between the heated interior and the surface. The relative dates of these successive terrestrial disturbances can be satisfactorily determined by stratigraphical and palæontological evidence.

The history of the gradual growth of the European continent furnishes many interesting and instructive illustrations of the principles by which evolutional geography is to be worked out. The earliest European land appears to have existed in the north and north-west, comprising Scandinavia, Finland, and the north-west of the British area, and to have extended thence through boreal and arctic latitudes into North America. Of the height and mass of this primeval land some idea may be formed by considering the enormous bulk of the material derived from its degradation. In the Silurian formations of the British Islands alone there is a mass of rock, worn from that land, which would form a mountain-chain extending from Marseilles to the North Cape (1,800 miles), with a mean breadth of over 33 miles and an average height of 16,000 feet, or higher than Mont Blanc. The Silurian sea which spread across most of Central Europe into Asia suffered great disturbance in some regions towards the close of the Silurian period. It was ridged up into land inclosing vast inland basins, the areas of some of which are still traceable across the British Islands to Scandinavia and the west of Russia. An interesting series of geographical changes can be traced during which the lakes of the Old Red Sandstone were effaced, the sea that gradually overspread most of Europe was finally silted up, and the lagoons and marshes came to be densely crowded with the vegetation to which we owe our coal-seams. Later terrestrial movements led to the formation of a series of bitter lakes across the heart of Europe like those now existing in the south-east of Russia. Successive depressions and elevations brought the open sea again and again across the continent, and gave rise to the accumulation of the rocks of which most of the present surface consists. In these movements the growth of the Alps and other dominant lines of elevation can be more or less distinctly traced. It was at the close of the Eocene period, however, that the great disturbances took place to which the European mountains chiefly owe their present dimensions. In the Alps we see how these movements led to the crumpling up and inversion of vast piles of solid rock, not older in geological position than the soft clay which underlies London. Considerable additional upheaval in Miocene times affected the Alpine ridges, while in still later ages the Italian peninsula was broadened by the uprise of its sub-Apennine ranges. The proofs of successive periods of volcanic activity during this long series of geographical revolutions are many and varied. So too is the evidence for the appearance and disappearance of successive floras and faunas, each no doubt seeming at the time of its existence to possess the same aspect of antiquity and prospect of endurance which we naturally associate with those of our own time. The law of progress has been dominant among plants and animals and not less upon the surface of the planet which they inhabit. It is the province of the biologist to trace the one series of changes; of the geologist to investigate the other. The geographer gathers from both the data which enable him to connect the present aspects of Nature with those out of which they have arisen.

[1] Abstract of an Address given by Prof. Geikie, F.R.S., at the meeting of the Royal Geographical Society on March 24, 1879.

GEOGRAPHICAL NOTES

AT a recent meeting of the Board intrusted by the French Government with the care of granting missions for exploring foreign countries, it was decided that none of the regions proposed offered any special field for exceptional services rendered to science. The funds of the Government will be spent neither in exploring Central Africa nor in seeking the north pole, but in excavating Trojan ruins and examining some of the islands of the Asian Archipelago. It was also complained that no qualified traveller had been sent into civilised parts to study the progress of special arts and sciences. Such excursions as the celebrated "Voyage en Angleterre et en Irlande," accomplished by Baron Dupin in 1820 have rendered immense services to French industry, and the memory of it is not extinguished by the sixty years which elapsed. The sending of regular scientific missions abroad was inaugurated in France by the First Republic, for the purpose, not exclusively for cultivating anthropology, but for introducing into France the progress made by the foreign nations.

M. CHARNAY has recently forwarded to the Minister of Public Instruction at Paris a series of communications on the results of his investigations in Java in the summer of last year. He explored the east and west portions of the island, and he claims to have discovered a close affinity between the remains of the civilisation introduced by Hindu Buddhists and that of the ancient Mexican Empire. He also calls attention to the great density of the population of Java. From this island M. Charnay went on to Melbourne, and when last heard from, was engaged in making natural history collections in Queensland and at Thursday Island in Torres Straits.

THE death of the last chief of the Belgian caravan has not abated the resolution of King Leopold and the members of the International Committee for African Exploration. A third expedition is to be sent out immediately, it is said, under the guidance of Mr. Stanley. It is also stated that a new Belgian expedition led by Capt. Popelin will soon start for Zanzibar, in order to work out the plan of establishing a chain of stations right across Central Africa, viz., from Zanzibar to the Loango coast. The King of the Belgians will grant the means for this important undertaking.

THE last *Bulletin* of the Société de Géographie Commerciale de Bordeaux contains a brief paper by M. Albert Merle, advocating the exploration of Ferlo, Senegambia. This is a tract of country between the Senegal and the Gambia, marked in our latest maps, "desert country, no water;" it extends from 14° to 16° N. lat., and its interior is quite unexplored. Several travellers have passed along the outskirts of the region, and from their accounts and from native reports, it appears to be covered with thick forests containing many kinds of valuable trees; tobacco, indigo, and cotton also grow there in abundance. Those of its products which are at present turned to account, find their way to the Gambia, but M. Merle's desire is to divert the trade to the French settlements on the north.

M. PAUL SOLEILLET, the French traveller who left St. Louis in Senegal with the intention of reaching Algeria through the Sahara, according to the last intelligence received in Paris by telegraph, had reached Segou, the capital of the negro state of the same name, and he was proceeding onwards. This adventurous man received only 6,000 francs from the Governor-General of Senegal. The Paris Society of Geography, as a protest against such indifference, resolved to send him, when possible, all the money disposable from the travelling and exploring funds.

THE latest news from Dr. Rohlfs' expedition to Central Africa states that one of its members, Baron Leopold von Csillagh, has left the expedition, and will return to Europe after paying a short visit to Murzuk. News from Tripolis states that the presents sent by the Emperor of Germany, and destined for the Sultan of Wadai, have at last arrived there. The latest papers sent by Dr. Rohlffs contain a valuable zoological report by Dr. Stöcker, the naturalist accompanying Dr. Rohlffs' expedition, besides a number of astronomical observations.

IN the present demand for accurate information respecting the Zulus and their country, it may not be out of place to call attention to a series of papers which appeared in the *Nautical Magazine* for 1853 and 1854, under the title of the "Loss of the Brig *Mary* at Natal, with Early Recollections of that Settlement." These papers were published anonymously, but were written by Mr. C. R. Maclean, now an official in St. Lucia, who more than fifty years ago spent three years with the famous Chaka, then King of the Zulus, and consequently had the best of opportunities for observing the character of the country and the people.

WE regret to announce the death of Dr. Friedrich Wilhelm Vogler of Lüneberg, well known in Germany as the author of several excellent geographical handbooks. Dr. Vogler was in his eighty-seventh year.

THE King of Portugal has presented to Dr. Oskar Lenz, the well-known African traveller, the knightly cross of the Portuguese Order of Christ.

PROF. BASTIAN, whose severe illness was announced not long ago, is in a fair way of recovery. The indefatigable traveller and ethnographer is at Calcutta and intends soon to start for Batavia.

WE learn from the *Colonies and India* that those who took part in the recent expedition from Wellington, New Zealand, to New Guinea, which proved a failure, intend starting another one. They propose to proceed to Astrolabe Bay, and will take with them two whale boats and a long boat, two horses, some goats, &c. The services of a doctor, geologist, and botanist are to be secured, and a carpenter, gunsmith, and one or two other handicraftsmen are to be invited to join.

NOTES

ON Tuesday morning, in the presence of a small number of his sorrowing friends, the remains of the late Prof. W. K. Clifford were placed in their last resting-place in Highgate Cemetery.

THE following grants have lately been made from the Research Fund of the Chemical Society:—10*l*. to Dr. C. A. Burghardt for an investigation into the constitution of topaz; 20*l*. to Mr. Francis Jones for the investigation of boron hydride; 15*l*. to Mr. F. D. Brown for the study of the theory of fractional distillation; 30*l*. to Dr. Dupré for the estimation of organic carbon in air; and 15*l*. to Prof. T. E. Thorpe for the investigation of albietene, the hydrocarbon of nut-pine.

M. BISCHOFFSHEIM, the well-known French Mæcenas of science, has just returned from Mentone, which he visited with M. Lœwy, the Sub-director of the National Observatory, to examine the practicability of establishing an observatory in his mansion. The site was found to be very convenient in all respects, and M. Bischoffsheim resolved to spend a sum of 900,000 francs for instruments, &c. The work is to begin immediately.

M. ANDRÉ, the well-known eclipse and transit of Venus observer, has inaugurated the publication of meteorological readings taken in the Municipal Observatory established at Tête d'Or, in the vicinity of Lyons. The peculiarity of that establishment is that astronomical and meteorological observations are conducted *pari passu* with the same zeal. It is the only place in France where the schemes organised by Leverrier, at Paris, are practised.

THE Select Committee of the House of Commons appointed to inquire into the subject of the lighting of towns by means of electricity, and to which the Liverpool Lighting Bill was referred, has determined to go into the general question, settle the principle, and then leave the thirty-four private Bills which ask for powers to light by electricity to be dealt with by the regular Committees of the House of Commons. The inquiry will commence on the 31st inst. As the evidence will be lengthy and the committee will probably report late in the Session, it is expected that no powers will be granted this Session for lighting by electricity.

THE Werderman light was tried by M. Becquerel in his lectures on electricity, delivered at the Conservatoire des Arts et Métiers on March 19. This apparatus, which will be tried very shortly in Paris, has been introduced into France by Dr. Cornelius Herz. Six Werderman lights were arranged round the chair of the professor and burned with the utmost regularity every time they were lighted. The opinion of M. Becquerel was very favourable indeed; he insisted upon the presence of a micro-

scopic arc giving an exceptional brilliancy to the flame. He reserved the question of cost in comparison with gas, and confined himself to making a comparison between Werderman and his competitors. M. Jamin has presented to the Academy of Sciences a system of his own, which has been described in the *Comptes rendus*, and will be tried by the Jablochkoff Company. Other systems are said to be preparing so that the partial failure of *bougies* is giving rise to a renewed electric light agitation in Paris. The members of the Municipal Council have determined to give fair play to any rational experiments.

As the result of the experiment of lighting the Holborn Viaduct with the electric light it has been found that the cost is seven and a half times that of gas, while the illuminating power is seven times greater. It has been decided not to continue the experiment.

A VALUABLE contribution to the marine zoology of the coast of the United States is furnished by a paper by Prof. H. E. Webster upon the chætopod annelids of the Virginia coast. This contains the result of several years' observation on the coast of Virginia by Prof. Webster, adding many new species to our hitherto published lists.

THE death of Prof. Yarnell, a much esteemed member of the scientific corps of the United States Naval Observatory, took place in Washington on February 27, at the age of sixty-two. The annual volumes of the Observatory contain a great many important memoirs by Prof. Yarnell, and he had just completed another at the time of his death.

IN the Report for 1878 of the "Observations of Injurious Insects," which has been drawn up by Miss E. A. Ormerod and her fellow-workers, and recently published (London: West, Newman, and Co.), a good many facts interesting alike to the agriculturist, entomologist, and meteorologist are recorded. The thanks of all interested in the preservation of food-crops are due to Miss Ormerod for her persistent efforts to promote a knowledge of, and a means to diminish the ravages caused by insect pests, and to all those who have opportunities to assist, such information if sent to Miss Ormerod, Dunster Lodge, Spring Grove, Isleworth, will be sure to be utilised. For the guidance of fresh observers the points required to be observed are pointed out, those particularly wished for being as follows: "1. With regard to weather; a very few lines as to general state through the year, such as any marked succession of warm or cold days, of great rainfalls, or drought. 2. Any observations as to the spread of common crop-insects from common crop weeds. For instance, with regard to observations of charlock and blackthorn in connection with turnip fly and gooseberry caterpillar. These two plants supply food or shelter for two insects that certainly come under the head of 'pests.' Their presence is either agriculturally bad or of little use, but they keep up the supply of successive insect generations in safety, because little noticed on these worthless growths. 3. Observations as to infested farm stores and seeds might throw much light on the intermittent appearance of some destructive insects. Thus the wheat midge *Cecidomyia tritici* is kept safe in the larval state during winter in neglected chaff-heaps, and the red clover weevil may be seen in legions creeping from the recently-stored clover. The amount of loss from this insect has been observed for more than eighty years, and still *Apion apricans* is at work in the clover as hard as ever." Miss Ormerod further points out that often the larvæ and pupæ are both contained in the seeds, and consequently sown with them, and she further remarks that we shall probably find the key to great devastations in what were originally small appearances. "Each note of information," we are reminded, "even if incomplete in itself, will or may probably join on to those of other observers, and thus the circumstances which give rise to insect ravage be gradually more and more clearly made out, till we may hope, if not entirely to check the evil, at least to mitigate it greatly." The special points of interest in the report under consideration are: 1. The spread of the turnip fly in localities where charlock was most prevalent, and attention is drawn to the desirability of eradicating, as far as possible, this food-plant of the insect. 2. The effect of rain or dew in diminishing the spread of the pest by reducing its powers of locomotion; and 3. The observations regarding the destruction of insects by birds. One observer mentions that in the neighbourhood of Plymouth migratory insectivorous birds were very abundant, especially starlings, who congregated in such enormous quantities that the flocks coming in from all quarters to roost in the evenings so completely filled the roosting-trees as to constitute quite a sight. The report altogether is one of much value, and its circulation will, we doubt not, create that interest in the subject which the promoters desire.

MISS E. A. ORMEROD, who has done so much in the field of economic entomology, has recently contributed a brief but interesting paper, entitled "Notes on Economic Entomology," to the Watford Natural History Society. This paper may well be taken as a companion to the "Notes of Observations of Injurious Insects" for 1878.

THE *Chemist and Druggist* gives the following account of an experiment in opium-smoking, made by Dr. Miclucho Maclay upon himself during his stay in Hong Kong:—The experiment was made at the Chinese Club, where every convenience for smoking opium is to be found. Dr. Clouth, of Hong Kong, took the necessary observations, and his notes are recorded below. These may be summarised as follows:—Herr Maclay was in normal health, and had fasted eighteen hours before commencing the experiment. He had never smoked tobacco. Twenty-seven pipes, equivalent to 107 grains of the opium, used by the Chinese, were smoked in two and three-quarter hours, at tolerably regular intervals. The third removed the feeling of hunger caused by his long fast, and his pulse rose from 72 to 80. The fourth and fifth caused slight heaviness and desire for sleep, but there was no hesitation in giving correct answers, though he could not guide himself about the room. After the seventh pipe the pulse fell to 70. The twelfth pipe was followed by singing in the ears, and after the thirteenth he laughed heartily, though without any cause that he can remember. Questions asked at this time were answered only after a pause, and not always correctly. He had for some time ceased to be conscious of his actions. After the twenty-fifth pipe questions asked in a loud tone were not answered. After the last pipe had been smoked he remarked, "I do not hear well." Forty minutes later there was a slight return of consciousness and he said, "I am quite bewildered. May I smoke some more? Is the man with the pipe gone already?" Fifteen minutes later (4.55 P.M.) he was able to go home, and then retired to bed. He woke the next morning at 3 A.M., and made a hearty meal, after his fast of thirty-three hours. During the next day he felt as if he had bees in a great hollow in his head, as well as a slight headache. The organs of locomotion were first affected, next came sight and hearing, but Herr Maclay is very positive that there were no dreams, hallucinations, or visions of any sort whatever.

IT is well known from the equable temperature of the Fiji Islands and the favourable nature of the soil of many parts that the colony is well adapted to the cultivation of various useful plants that require only to be introduced to thrive. These matters have recently been discussed in a little pamphlet published at Levuka, on the agricultural prospects of Fiji. That the productive powers of these islands is very great is here fully exemplified. It is shown that tropical produce of all kinds is capable of being grown on an extensive scale, so that the resources

of the islands might be made highly remunerative. Sugar-cane, coffee, tea, cinchona, and cocoa are the principal staples advocated. Sugar is looked upon in a most favourable light; some parts of the islands, both in richness of soil and climate, as well as in extent, are spoken of as extremely favourable for growing and maturing the cane; so much so as to make all well-wishers of Fiji look for the time when sugar will be made in the islands and "exported by the hundred thousand tons and to the value of millions of pounds sterling." Regarding coffee we learn that the Government have sent large supplies of seed into the interior of Viti Levu to form coffee gardens for the natives. The plants are described as having an extremely healthy appearance. Tea and cinchona could both be grown successfully in Viti Levu over an extent of country roughly estimated at about one hundred square miles. Though many valuable timber trees exist in the islands it is suggested that several well-known Indian trees such as teak, saul, sissoo, toon, and ebony, as well as mahogany, rosewood, and others should be introduced. It is to be hoped that as the resources of Fiji, including those of the forests, become developed, no undue sacrifice of timber will be effected, but on the contrary the trees will be carefully preserved or replanted as others are cut down.

THE class of substances whose fluorescence does not follow Stokes's law, and so which do not emit rays of less refrangibility than the existing rays, has lately been enlarged by Prof. Lommel by addition of one of two new fluorescing substances. That is anthracene blue, an etheric solution of which fluoresces olive green very strongly; it is excited extremely weakly by the blue and the greater part of the violet rays, but very strongly by the orange-green and yellow-green. The second new fluorescing substance is bisulphobichloranthracenic acid, the etheric solution of which gives superficially a beautiful blue, and the interior a greenish fluorescence. It obeys Stokes's law.

THE French Minister of Public Works has not yet answered the inquiries made by M. Giffard as to the probability of the Cour de Tuileries being at his disposal up to the end of September, in order to organise a new series of captive ascents. But M. Giffard, willing to give the preference to his native city, has rejected the advantageous offers made by the German company offering to work his captive balloon, and to pay him a royalty of 33 per cent. on the gross receipts.

AT 12.35 A.M. on the 22nd inst. an earthquake traversed Northern Persia, taking a direction from Tabreez to Zendjan and Mianeh, and shocks continued with more or less severity until Sunday, the 23rd. Several strongly-built houses were thrown down at Mianeh, and in others large rents were made in the walls. The most serious damage, however, appears to have been occasioned in two villages off the road, about four farsachs from Mianeh, named respectively Tark and Manan. These were totally destroyed, and of the 500 inhabitants in the one case and the 600 inhabitants in the other, only a few are reported to have been saved. Mianeh is situated in north lat. 37° 27′, east long. 47° 43′.

IN a report from the Philippine Islands we learn that in the towns of Molo and Javo, both situated close to each other, and distant about three miles from Yloilo, it is very rare to enter a house that has not its loom at work, so large a trade is done in weaving not only in the towns themselves but all over the province. The principal fibre used is that of the pine-apple, and some of the articles manufactured, such as shirts and dresses, are of considerable merit and sell at high prices. In weaving China silk in colours is intermixed with the pine-apple fibre, for the purpose of giving stripes to the dresses and shirts. The value of the Chinese silk so imported varies at from 200,000 dol. to 400,000 dol. (£40,000 to £80,000) per annum.

THE Teplitz thermal question may be considered as being solved in the most satisfactory manner. The Spring Committee established by the Austrian Government declared that the quantity of recovered water is 2,224 cubic feet per hour, which is sufficient for supplying all the thermal establishments in existence before the catastrophe. The temperature has not been altered in any sensible manner. It appears that altogether the catastrophe may be considered as having been in some respects useful. The actual quantity is one-third more than the sum of the several sources which were used before the catastrophe.

WE have received the first number of a new American journal —*Useful Arts*—edited by Mr. J. A. Whitney. It contains a great deal of miscellaneous industrial information, mostly referring to patents.

Gardening Illustrated is the title of a new cheap "weekly journal for town and country."

A METEOROLOGICAL work, entitled "Ergebnisse fünfzigjähriger Beobachtungen der Witterung zu Dresden," with an introduction on meteorology, the atmosphere, meteorological instruments and observations, has just been published by Dr. Adolf Drechsler, the director of the Royal Physico-Mathematical Institution at Dresden.

THE additions to the Zoological Society's Gardens during the past week include a Mona Monkey (*Cercopithecus mona*) from West Africa, presented by Miss Sandford; a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. George Eggar; a Chinchilla (*Chinchilla lanigera*) from South America, presented by Sir Chas. Smith; a Greater-spotted Woodpecker (*Picus major*) European, presented by Mr. H. Laver; a Sumatran Rhinoceros (*Rhinoceros sumatrensis*) from Sumatra, a Tabuan Parrakeet (*Pyrrhulopsis tabuensis*), a Stair's Dove (*Phlogœnas stairi*) from the Fiji Islands, deposited; a Pied Wagtail (*Motacilla yarrelli*), a Reed Bunting (*Emberiza schœniclus*) European, purchased.

SPECULATIONS ON THE SOURCE OF METEORITES[1]

I HAVE recently read M. G. Tschermak's most interesting memoir, "Die Bildung der Meteoriten und der Vulcanismus."[2] I am not competent to offer any opinion on the mineralogical questions involved in his discussion, but the numerous arguments he has adduced appear to me to justify his conclusion that "the meteorites have had a volcanic source on some celestial body." These arguments are briefly as follows:—

Meteorites are always angular fragments even before they come into the air.

Most meteoric irons have a crystalline structure which, according to Haidinger, requires a very long period of formation at a nearly constant temperature. This condition could only have been fulfilled in a large mass.

Many meteoric stones show flutings resembling those seen on terrestrial rocks and which are due to the rubbing of adjacent masses.

Other meteoric stones show a joining together of several fragments analogous to breccia.

Many meteoric stones are composed of very small particles analogous to volcanic tufas.

After glancing at the old theory of the volcanoes in the moon and rejecting as untenable the supposition that meteorites have any connection with ordinary shooting stars, Tschermak concludes—"We may suppose that many celestial bodies of considerable dimensions are still small enough to admit of the possibility that projectiles driven from them in volcanoes shall not return by gravity. These would really be the sources of meteorites." Similar views having been put forward by Mr. J. Lawrence Smith and other authorities it is not unreasonable to discuss the following problem.

[1] Read at the Royal Irish Academy, January 13.

[2] "Sitzungsberichte der mathematisch-naturwissenschaftlichen Classe der kaiserlichen Akademie der Wissenschaften," Wien, 1875. Band lxxi., Abtheilung 2, pp. 661–674.

If meteorites have been projected from volcanoes, on what body or bodies in the universe must those volcanoes have been located?

Let us first take up a few of the principal celestial bodies *seriatim* and consider their claims to the parentage of the meteorites. We begin with the sun. It has been abundantly shown that there exists upon the sun tremendous explosive energy. It is not at all unlikely that that energy would be sufficiently great under certain circumstances actually to drive a body from the sun never to return. We might therefore find upon the sun adequate explosive power for the volcano, but the projectiles are here the difficulty. There are a number of circumstances (notably the breccia-like appearance of some meteorites) which show conclusively that the meteorites have beeen torn from rocks which were already nearly, if not quite, solid, and as it seems in the highest degree improbable that rocks of this nature should exist in the sun, we may conclude that the sun has not been the source of the meteorites.

Can the meteorites have come from the moon? Owing to the small mass of the moon the explosive energy required to carry a body away from the moon is comparatively small. Can such a body fall upon the earth? *To simplify questions of this kind we shall suppose various disturbing influences absent.* We shall suppose that the projectile is discharged from a volcano on the moon with sufficient velocity to carry it therefrom. We shall then omit all account of the disturbing influence both of the sun and moon on the projectile, and we shall suppose that the projectile is really revolving round the earth as a satellite. This projectile will fall upon the earth if its distance from the earth's centre when in perigee be less than the radius of the earth (augmented, perhaps, by the thickness of the earth's atmosphere). It should however be observed that *if the projectile once escaped the earth it would never fall thereon*, hence the question as to whether the moon can be the source of the meteorites now falling appears to be connected with the question as to whether the lunar volcanoes are *now* active. But it is generally believed that the lunar volcanoes are not now active to any appreciable extent (even if the suspected indications of recent change were thoroughly established). It follows that even if the moon has been a source of meteorites in ancient times, we no longer receive a supply from that quarter. There is of course just a possibility that projectiles from the moon which have been revolving round the earth as satellites in elliptic orbits ever since their ejection may, under the influence of *the disturbing causes previously excepted*, gradually change their orbits until they become entangled in the atmosphere and descend as meteorites. It therefore appears to be not quite impossible that even still a meteorite which had its origin in the moon in past ages may occasionally tumble on the earth.

Passing from the sun and the moon let us now bring under review some of the other celestial bodies and see how far they will fulfil the conditions of the question. Is it possible that the meteorites can have been projected from the surface of a planet? In order to get over the difficulties of the great initial velocity which would be necessary to overcome the gravitation of a large planet, it seems natural to inquire if a volcano placed upon one of the small planets could accomplish the task.

It is clearly impossible that a projectile should ever fall on the earth unless the orbit of the projectile cuts the plane of the ecliptic in a point which lies in the narrow ring between 8,000 and 9,000 miles wide which the earth traces out on the ecliptic, but if a meteorite with an elliptic orbit intersect this ring, then, in the lapse of time, it may happen that the earth and the meteorite meet at the intersection of their orbits, in which case of course the long travels of the meteorite will come to an end.

We shall therefore consider the circumstances under which it would be possible to discharge a projectile from the surface of a planet (say Ceres), so that the projectile shall intersect the ecliptic in the ring we have just referred to. The planet being small the initial velocity that would be required to carry a projectile from its surface presents no difficulty; perhaps an ordinary cannon would be sufficient *so far as the mere gravitation to the planet is concerned.* But when we consider the necessity that the projectile must be driven through the ring we have been considering, a vastly more powerful instrument would be required.

Ceres is moving in an orbit (supposed circular and in the ecliptic) with a velocity of about eleven miles per second. A projectile discharged from Ceres will have an actual velocity which is compounded of the velocity of Ceres, with the velocity which is imparted by the volcano. But simple dynamical considerations show that if the projectile have an initial velocity *perpendicular to the radius vector*, differing from about eight miles per second, it can never intersect the ring, no matter in what direction it be discharged.[1] The volcano on Ceres must therefore be adequate to the abatement of the velocity perpendicular to the radius vector from eleven miles per second to eight miles per second, *i.e., the volcano must be at the very least adequate to producing an initial velocity of three miles per second.* As this is quite independent of the additional volcanic power requisite to carry the projectile away from the attraction of Ceres, it is obvious that after all there may be but little difference between the volcano which would be required on Ceres, and that (of six mile power) which would project a body away from the surface of the earth for ever.

Admitting, however, that a volcano of sufficient power were placed upon Ceres, would it be likely that a projectile driven therefrom would ever cross the earth's track? This is a question in the theory of probabilities, and it is not easy to state the problem very definitely. If the *total* velocity with which the projectile leaves the orbit of Ceres be less than eight miles per second, then the projectile will fall short of the earth's track; on the other hand, if the *total* initial velocity exceeds sixteen miles per second, the orbit in which the projectile moves will be hyperbolic, and though it may cross the earth's track once, it will never do so again. Taking a mean between these extreme velocities we may investigate the following problem:—Suppose that a projectile is discharged from a point in the orbit of Ceres in a *random* direction with the *total* initial velocity of twelve miles per second, determine the probability that the orbit of the projectile shall cross the earth's track. When this problem is solved in accordance with the calculus of probabilities it is found that the chances against the occurrence are about 50,000 to 1, *i.e.*, out of every 50,000 projectiles discharged at random from a point in the orbit of Ceres, only a single one can be expected to cross the earth's track.

It is thus evident that there are two objections to Ceres (and the same may be said of the other minor planets) as a possible source of the meteorites. Firstly, that notwithstanding the small mass of Ceres, a very powerful volcano would be required; and secondly, that we are obliged to assume that for each meteorite which could ever fall upon the earth, at least 50,000 must have been ejected.

It thus appears that if the meteorites have been originally driven from any planet of the solar system, large or small, the volcano must from one cause or another be a very powerful one.

There is, however, one planet of the solar system which has a special claim to consideration. On that planet it is true that a volcano would be required which was capable of giving an initial velocity of at least six miles per second; but *every* projectile launched from that volcano into space would, after accomplishing an elliptic orbit round the sun, dash through the track of the earth, and again pass through the same point at every subsequent revolution. It is not here a case of one solitary projectile out of 50,000 crossing the earth's track, but every one of the 50,000 possesses the same property. The planet of which we are speaking is, of course, the earth itself. If in ancient times there were colossal volcanoes on the surface of the earth which had sufficient explosive energy to drive missiles upwards with a velocity sufficient to carry them away from the earth's surface, after making allowance for the resistance of the air, these missiles would then continue to move in *orbits round the sun*, crossing at each revolution the point of the earth's track from which they were originally discharged. If this were the case, then doubtless there are now myriads of these projectiles moving through the solar system, the only common feature of their orbits being that they all intersect the earth's track. It will, of course, now and then happen that the earth and the projectile meet at the point of crossing, and then we have the phenomenon of the descent of a meteorite. This theory, that the meteorites have originated in the earth, was so far as I know first put forward by Dr. Phipson. Mr. J. Lawrence Smith in a letter I received from him some months ago inclines to the same view as at all events one of the probable sources.

It is well to note here the great difference between the lunar theory of meteorites and the terrestrial theory. For the lunar theory to be true it would probably be necessary that the lunar volcanoes should be *still* active. In the terrestrial theory it is only necessary to suppose that the volcanoes on the earth *once*

[1] Disregarding an obvious exception.

possessed sufficient explosive energy. No one supposes that the volcanoes on the earth at present eject the fragments which will constitute future meteorites, but it seems probable that the earth may be now slowly gathering back in these quiet times the fragments she ejected *in an early stage of her history*.

Assuming, therefore, that the meteorites have had a quasi-volcanic origin on some considerable celestial body, I am led to agree with those who believe that most probably that body is the earth.

ROBERT S. BALL

RECENT RESEARCHES ON ABSORPTION SPECTRA

THE numerous absorption spectra of soluble substances which have been described hitherto, have referred as a rule to the solutions of the substances, and but rarely to the solid substances themselves. It is true that certain differences were remarked between the spectra of certain solutions, those of uranium and didymium salts, for instance, and the spectra of the solid salts; yet, on the whole, these differences were so slight that it was generally believed that the spectra were essentially the same. On the other hand experiments had shown that the spectra of solutions differed according to the dissolving medium; indeed Herr Kundt established the fact that the absorption band of a substance in solution lies the nearer to the red end of the spectrum the stronger the dispersion of the dissolving medium. In these experiments the fact seems to have been overlooked that when changing the dissolving medium often the whole character of the spectrum is changed, so that comparison with the former one becomes extremely difficult. Close investigation of these differences was therefore an important desideratum, both for the theory of absorption spectra as well as for practical absorption spectrum analysis.

In the Monthly Report of the Berlin Academy of Sciences, Herr Vogel has recently published the results of such investigations, to which he was led by the remarkable differences between the spectra of solid and those of dissolved substances which he had observed in the case of certain pigments.

For the examination of these absorption spectra Herr Vogel used instruments of but moderate dispersion, which allow of an easier survey of the whole spectrum, and consequent judgment of its general character, than is the case with strongly dispersing spectroscopes. The absorption spectra of solid salts and pigments were obtained from thin layers of these substances, prepared upon glass plates, through evaporation of a few drops of solution. Herr Vogel reproduces the spectra he observed on two plates, which at once show not only *the differences in the spectra of one and the same solid substance and its solution*, but frequently an *extraordinary coincidence in the position of the absorption bands belonging to totally different substances* (for instance, in nitrate of uranium and permanganate of potash). Of several substances, such as iodine, hyponitric acid, and indigo, the spectrum of the vapour is also given for comparison, and in most cases the aqueous, alcoholic, and some other solution of each substance has been examined.

Without entering into the highly interesting details for which we must refer our readers to the original paper, we confine ourselves to stating the results of Herr Vogel's researches, which are the following: 1. Considerable differences exist between the spectra which a substance gives in the solid, liquid, or dissolved and gaseous state. Characteristic bands which are shown in the spectrum of one state are either not reproduced in that of the other (this is the case with chrome alum, chloride of cobalt, iodine, bromine, naphtaline red, fuchsine, indigo, cyanine, aniline blue, methyl violet, eosine, carmine, purpurine, alizarine, santaline), or they reappear in a different position, or different intensity (examples: nitrate of uranium, permanganate of potash, hyponitric acid, alcanna red). Sulphate of copper and chlorophyll show the same absorption both in the dissolved and in the solid state.

2. The spectra given by the same substance when dissolved in different media are the same in some cases (purpurine in alcohol or sulphide of carbon, aldehyde green in water or alcohol, methyl violet and indigo-sulphuric acid in water or amylic alcohol); in other cases they differ only in the position of bands (chloride of cobalt, fuchsine, coralline, eosine and iodine green in aqueous or alcoholic solutions); and again in others their character is totally different, so that no point of coincidence remains (iodine in sulphide of carbon or alcohol, naphtaline, aniline blue, purpurine, hæmatoxyline, brasiline in water or alcohol).

3. The rule established by Kundt, viz., that the absorption bands of a body in solution lie the nearer towards the red end of the spectrum the greater the dispersion of the dissolving medium is in the region of the bands, is not confirmed in many cases; on the contrary, in some instances the absorption bands move towards the blue in a solution of greater dispersion (nitrate of uranium and blue chloride of cobalt in water and alcohol); in other cases their position remains unaltered for various media (hyponitric acid in air and benzol, indigo-sulphuric acid and methyl violet in water and amylic alcohol, aldehyde green in water and alcohol, purpurine in sulphide of carbon and alcohol). In some cases a great difference in the sense of Kundt's rule becomes apparent, while in others for the same spectral region but a very trifling one appears, according to the nature of the pigment (coralline and fuchsine). Indeed it happens sometimes that certain bands are in the same position with different dissolving media, while others which are *simultaneously* visible are displaced (nitrate of uranium in water and alcohol, oxide of cobalt in glass and in water, protonitrate of uranium in neutral solution and in a solution of oxalic acid, chlorophyll in alcohol and ether).

4. The position of absorption bands in the spectra of solid and dissolved bodies may be only exceptionally deemed characteristic for any certain body. Totally different bodies show absorption bands in exactly the same position (solid nitrate of uranium and permanganate of potash in the blue; naphtaline red and coralline in the yellow; indigo, aniline blue, and cyanine in the orange; aldehyde green and malachite green in the orange). Closely related substances sometimes show remarkable differences in the position of their bands under prefectly equal conditions (solid uranium salts).

5. The rule set up for absorption spectra, "each body has its own spectrum," can be admitted only with great restrictions. The great number of polychromatic substances show different colours and different spectra in the solid state, according to the direction in which they are observed. Most other bodies show different spectra in the solid state from those of their solutions, and in the latter case again different ones according to the dissolving medium, and the question arises which of all these spectra is the body's "*own*" spectrum.

The most important difference of the spectra of elements in a state of incandescent vapour, the position of the spectral lines, ceases to be characteristic in the case of absorption spectra of liquid and solid bodies. In the latter spectra, however, the characteristic differences shown by the spectra of incandescent vapours cannot be expected. It is known that metals, which give such remarkably different spectra in the state of incandescent vapours, all give qualitatively the same spectrum as incandescent liquids or solids, viz., a continuous one; for this reason the absorption spectra of these bodies cannot show any remarkable characteristic differences, whatever quantitative differences may become apparent with regard to the absorbed colours. If these well-known facts show that already with regard to elements the laws applying to the spectra of gases do not apply to those of liquids and solids, then Herr Vogel's investigations prove that in the case of compound bodies simple relations between the spectra of their different aggregate states are still less frequent and occur only exceptionally.

The analysis of absorption spectra therefore is based not so much upon the recognition of the position of the absorption bands of a substance, as upon the *changes in the spectra* of the same body which take place under the influence of various dissolving media and reagents. Thus cyanine and aniline blue dissolved in alcohol give a very similar spectrum, dissolved in water a totally different one. The absorption bands of oxyhæmoglobine disappear with reducing agents; those of carmine, which are in a similar position, do not; the band of brasiline disappears when acetic acid is added to the solution, that of fuchsine does not, &c., &c.

The position of bands becomes more characteristic for the recognition of a body, if the latter shows *several* absorption bands. But even here we should go too far if from the accidental coincidence in the position of bands of two different bodies we were to draw conclusions regarding any similarity or chemical identity between them (this has indeed been done in certain cases, particularly with blood and chlorophyll). A conclusion regarding such similarity or identity is only justified if *the same bands show equal intensities and analogous changes under the influence of the same reagents*.

INTELLECT IN BRUTES

We have received so many letters on this subject that we are compelled to content ourselves with giving the following extracts :—

The Rev. George Henslow comments as follows on some of the cases already adduced :—

I would not assert that what I call "practical reasoning"—that is, reasoning applied to objective facts directly apprehended by the senses—is fundamentally different from "abstract," *i.e.*, with no objective fact immediately present to consciousness; but they certainly do represent two stages in our own mental development.

E. H. Pringle's account of "Bully" shows nothing beyond what is common to dogs in practical cunning (*i.e.* reasoning) with objective things, (1) his "lady" friend, (2) best road to take so as not to be seen, (3) the person to be avoided, (4) E. H. Pringle's eye to be eluded. In the only point where abstract reflection was really required Bully failed, just as a child would, *viz.*, in shamming sleep. For, it is not enough *to lie down* and *shut one's eyes*. This is all a child is told to do, or is conscious of doing on going to sleep. The relaxation of every muscle follows spontaneously. Hence, as children do not think of this when pretending (since it requires reflection) they can so easily be detected by some rigidity of the muscles in the face or in breathing which at once betrays them. I have known a child overdo it by screwing up its eyes in order to appear *very fast asleep!* Exactly so, too, Bully was totally unable to think of the importance of putting his ears in repose.

Dr. Rae's dog only strengthens my case, for it clearly associated the bell with a *particular* maid, whereas a reasoning human being would have *generalised* that since the maid was in the room, the bell could be rung for some one else. Hence the dog proved its impotency in all power of generalisation, which is a pure form of abstract reasoning.

Dr. Muirhead's donkey was solely concerned with immediate sense-objects, the gate and the cows, and required no abstract reflection. That horses and donkeys can discover how to open gates is by no means uncommon.

Lastly C.M.'s cat and Mr. Belsham's kitten are the only cases I have yet seen which show a *primâ facie* evidence against the distinction I propose to draw. But, that a half-grown kitten could go through such a process of reflection analogous to what I gave for a hypothetical untaught dog ringing a bell, would be so astounding, that all possible explanations must be eliminated first, before it is credited.

Is it not far more probable that the cat and the kitten discovered by accident that the door was opened when a knock was made, and that this discovery arose from the common habit of cats to play with anything suspended within their reach? That animals discover facts, and then use them, will not be disputed, like the dog that, on discovering a stream carried him down too far on swimming across it, ran a mile up stream ever afterwards to allow for the current. Again, that animals mimic, as do parrots and apes, is common enough, but they do not know why they do it. A monkey might knock at a door after seeing a man do it, but, I believe, could have no similar motive as the man, until (like the kitten) it should accidentally discover for itself what the real use was, or else unless it be taught to do it.

In re rats gnawing pipes. I have just heard of a mouse gnawing through a gas-pipe. May it not be accounted for by the fact that, although the upper incisors of a rodent, by working on the lower, keep the chisel-like ends in order; yet this may be assisted by gnawing wood, lead, or other hard substances? Does not this account for rabbits, though well fed on cabbage and bran, &c., still persisting in gnawing their hutches?

I will, in conclusion, give another case to illustrate the want of abstract reflection: this time in a lady (aged thirty), whose mental powers were curiously arrested. Looking at the picture of a shark in the sea, with a pig in its mouth, in "Masterman Ready," and knowing that the pig had been dropped from the wreck to see if it would swim to shore, she *naïvely* asked, "Is the shark carrying the pig to the shore?" The idea of the shark eating the pig would only arise from the abstract reflection on the habits of sharks, which was not suggested by the story; the single objective fact present to her mind was that "the pig had to get to the shore."

Mr. Arthur Nicols writes :—

I cannot understand practical *reasoning*, but a practical result of reasoning upon either simple or abstract ideas is intelligible. Can we conceive any human being reasoning more correctly than a dog did in the following instance?—Towards the evening of a long day's snipe-shooting on Dartmoor, the party was walking down the bank of the Dart, when my retriever flushed a widgeon which fell to my gun in the river, and of course instantly dived. I said no word to the dog. He did not plunge in after the widgeon *there*, but galloped *down* stream about fifty or sixty yards, and then entered the water, and dashed from side to side —it was about twenty or thirty feet wide—working up stream, and making a great commotion in the water, until he came to the place where we stood. Then he landed and shook himself, and carefully hunted the near bank a considerable distance down, crossed to the opposite side, and diligently explored that bank. Two or three minutes had elapsed, and the party was for moving on, when I called their attention to a sudden change in the dog's demeanour. His "flag" was now up, and going from side to side in that energetic manner which, as every sportsman knows, betokens a hot scent. I then knew that the bird was as safe as if it was already in my bag. Away through the heather went the waving tail, until, twenty or thirty yards from the bank opposite to that on which we were standing, there was a momentary scuffle; the bird just rose from the ground above the heather, the dog sprang into the air, caught it, came away at full gallop, dashed across the stream, and delivered it into my hand. Need I interpret all this for the experienced sportsman? The dog had learned from long experience in Australia and the narrow cañadas in the La Plata that a wounded duck goes down stream—if winged, his maimed wing sticks out, and renders it impossible for him to go up—and will invariably land, and try to hide away from the bank. But if the dog enters at the place where the bird fell, the latter will go on with the stream for an indefinite distance, rising now and then for breath, and give infinite trouble. My dog had found out all this long since, and had proved the correctness of his knowledge times out of number, and by his actions had *taught me* the whole art and mystery of retrieving duck. His object—I say, without a doubt, because I had had numberless opportunities of observing it—was to flurry the bird and force it to land by cutting it off lower down the stream. Then assuming, as his experience justified him, that the bird had landed, he hunted each bank in succession for the trail, which he knew must betray the fugitive.

Mr. A. Petrie writes :—In my own family we had a tabby cat, who, when turned out, would let herself in at another door by climbing up some list nailed round it, then pushing up the click-latch, pushing the door, with herself hanging on it, *away* from the post, so as to prevent the latch falling back into its place, and then dropping down and walking back to the fire. I knew a Skye terrier, who, being told to carry a fishing-rod, carefully experimented along its length, to find its centre of gravity, then carried it on till his master came to a narrow path through a wood. Here Skye considered, dropped the rod, took it by the end, and dragged it under him lengthwise, till the open road was gained, when he took the rod by the centre of gravity again, and went on. This could not be a copy of human actions, but the result of original reasoning.

Mr. Henry Cecil gives the following on the authority of the late Mr. Dawes the astronomer :—

Being busy in his garden, and having a large bunch of keys in his hand, he gave it to a retriever to hold for him till he was at liberty. Going into the house soon after he forgot to reclaim the keys. The remembrance of what he had done with them only returned to him when he required to use them in the evening. He then recalled that he had given them to the dog, and forgotten to take them again. Calling him, and looking impressively in his face, he said, "My keys! fetch me my keys." The dog looked wistful and puzzled for a moment, and then bounded off to the garden, his master following. He went straight to the root of an apple-tree, scratched up the keys, and brought them. May we not fairly put into words the dog's train of reasoning thus: "My master has given me these keys to hold; he has forgotten them; I cannot carry them all day; but I must put them in safety where I can find them again?"

Mr. W. S. Chamberlayne writes that many years ago, taking an afternoon ride through a wood in the Bahamas, he came to a gate which was kept closed by a small iron hoop hung over a post and the end of the gate. To open the gate he leant over his horse's neck and lifted up the hoop, shutting the gate

and replacing the hoop when he had passed through. On returning from his ride the gate was still shut, when, to his surprise, his horse, without any hesitation, took the hoop in his mouth and tried to lift it off the gate. He, however, was not successful in his efforts, and Mr. Chamberlayne had to finish the operation for him, but the exhibition of memory was certainly remarkable.

Mr. T. B. Groves, of Weymouth, sends the following account given to him by a relative, a gentleman well known in the district, and who would be everywhere accepted as a trustworthy and competent observer:—

In the wine-cellar two vessels, one an open earthen jar containing hazel-nuts, the other a wooden sieve, tub, or something of the kind, full of wine-corks, stood side by side. It was observed that the nuts were gradually diminishing, owing to the depredations of mice; but after a time this seemed to have altogether ceased, and it was inferred that the difficulty of egress had caused the mice to abandon the enterprise as soon as the level of the nuts had reached a certain depth from the mouth of the jar. Matters so remained for some little time; but afterwards, on visiting the cellar, it was found, to the owner's great surprise, that his nuts had now entirely disappeared, and in their place were discovered the corks! The only explanation that could be suggested was this: that the mice, *reflecting* on the difficulty of making their exit from the partially-emptied jar, had *conceived* and carried out the plan of providing for their escape by dropping into the jar from time to time sufficient corks to enable them to make a safe retreat with their plunder.

Mr. R. Howson sends us the story of a terrier-like dog of no particular breed, named Uglymug, who had a poodle for companion. Whenever Uglymug saw signs of a family meal being laid out, he inveigled the poodle into a labyrinthine shrubbery under pretence of seeking for rats, and when the latter was fairly intent on its game, Uglymug sneaked back to enjoy all by himself what he could get from the family table.

V. I. writes:—The following instance will show that in the case of the mule intelligence has a limit. We had a mule who could take the staple out of a gate and open it (he never shut it). This mule used to go to the water-butt, turn the brass tap, and drink, but never turned the water off. Common sense would have forbidden a human being neglecting such a precaution.

Mr. E. Parfitt, of the Devon and Exeter Institution, writes of a favourite cat:—She would frequently come and sit near the door opening into the library of the institution. The door only divides my house from the library; puss would place herself here mostly at dinner-time, and, as I am informed, not before; she would wait here until she heard my footsteps down the library; she would then proceed directly to the kitchen, and inform the servant, either by mewing or looking up into her face. She would then come to me and tell me in her way that she had ordered dinner. I have seen her scores of times trotting along the passage to the kitchen, when I have opened the library door, to inform the servant that I was coming. How Topsy ascertained the time to proceed to the door I do not know, except that she saw that dinner was preparing; but how did she know the time it would be ready and the time that I was expected to come in?

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

The Association for promoting the Higher Education of Women in Oxford is to open two halls in Oxford in October for the reception of lady students. One of these is to be an "Academical house on the principles of the Church of England," and in the other, "Somerville Hall" (after Mrs. Somerville), care will be taken that members of different religious denominations are placed on the same footing. The charges in the latter will be considerably lower than in the former.

Two Combe Exhibitions of 35*l.* each will be open for competition in May next at Trinity College, Oxford. Candidates will be at liberty to offer classics, mathematics, chemistry, and physics, a period of history, or any two or more of these branches of study. There is no fixed limit of age. The examination, which will be combined with the ordinary Matriculation examination, will commence on Tuesday, May 20, at 9 A.M. Names, with subjects offered and testimonials of good conduct, to be sent to the president not later than May 12.

The Astronomer-Royal continues to give evidence of his intense desire for the promotion of sound mathematical training, and has published to the Cambridge Senate his views as to the papers set in the Smith's Prize examinations of recent years, classifying the questions set, and showing that several subjects, more valuable, in his opinion, to men of science and to students, than all the others together, have had no questions set upon them. Among these are attractions, higher dynamics, perturbations, figure of the earth, thermodynamics, waves and tides, sound, physical optics, &c. He says very pertinently: "The use of an examination is to test the power of a candidate to command the application of mathematics when required. The use of publication of examination is to guide students in the subjects recommended for their study. The guidance which too many of these subjects intimate is this: that clever and abstruse algebra, without any reference to its benefit as an application of a tool to other purposes, is the *summum bonum*." He believes this guidance is against the instincts of many residents at Cambridge and the desires of undergraduates.

Progress is evident at Cambridge in response to the memorial we recently referred to against the compulsory study of Greek by all undergraduates. Very few votes prevented reform years ago; no doubt the claims of science students and of liberty for all will now be more fairly listened to. The syndicate on the subject includes Dr. Humphry, Professors Liveing and James Stuart, and Mr. Todhunter, and thus the real interests of mathematics, physics, biology, and medicine, as regards the education of students, as well as the progress of science, will be sure of recognition.

The examiners in the Cambridge natural sciences tripos this year are Dr. Humphry, Prof. Bonney, Mr. J. F. Walker, (Lecturer on Chemistry at Sidney Sussex College), and Mr. Yule, of Magdalen, Oxford, the foregoing being re-appointments, and Prof. Liveing, the Rev. J. W. Hicks (University Demonstrator of Chemistry, and Lecturer on Botany at Sidney Sussex College), Mr. W. Garnett, Demonstrator of Experimental Physics, and Mr. F. M. Balfour; the latter three are fresh nominations.

In the last Cambridge Local Examinations (December, 1878), among 626 senior boys there were 92 candidates for the chemistry paper and 44 for practical chemistry; of 997 senior girls, 29 took the paper and only 4 the practical examination; 21 boys and no girls entered for experimental statics and dynamics, &c., 38 boys and 24 girls for heat, 30 boys and 4 girls for electricity. The result is that only 3 boys, 2 from the Liverpool Institute and 1 from Newton College, Devon, obtained the mark of distinction in the section "natural philosophy," in which all these subjects are included; and no girls. It should be added that a pass may be attained on two of these subjects, and only three in all may be taken by any candidate. Is it possible to show more strongly the lack of attention to and interest in the elementary forces of nature in English schools and by English parents? These are boys and girls between sixteen and eighteen years of age, most of them supposed to be ready, or almost ready, to leave school and take part in the battle of life. Among 3,329 junior boys and 1,483 junior girls, 423 boys and 13 girls took the chemistry paper, 169 boys and 1 girl the practical chemistry, 76 boys and no girls statics, &c., 178 boys and 12 girls heat. Seven boys, the majority from Liverpool College, and no girls, obtained distinction. We do not become further consoled by finding that 15 senior boys and 79 senior girls took zoology, 11 boys and 177 girls botany, 24 boys and 150 girls geology; for girls have no *more* right to a scientific training than boys. Most likely, however, boys and their teachers will seek to know more of the life and the past history of the globe when they find that girls can really hold their own in and enjoy these studies, and look with amazement on men for being so unwilling to learn or teach them. Among the juniors, 75 boys and 148 girls took zoology, 45 boys and 238 girls botany. These numbers, however, represent no great attainments as yet, for the standard of passing is very low; severity would only kill the tender growth. But evidently there is in secondary schools little belief in the educative and attractive power of the study of nature. Why is it not considered that *mathematicians* are fostered by neglect and hindrance? It appears to be thought capital training to produce physicists and naturalists. Really, conservatism and unwillingness to take a little trouble are the enemies.

A REVISED schedule of subjects in natural science for the *ordinary* B.A. degree at Cambridge has just been issued. This is for the third or final examination, and a pass in one subject is sufficient to give a degree. Is it supposed at Cambridge that a year is to be fully employed in dealing with botany in an *elementary* manner? The schedule says the questions (all elementary) will include the description and classification of plants; the form, structure, and development of stem, root, leaf, flower, and fruit; inflorescence, cross-fertilisation, germination, and nutrition. Twenty-one "natural orders" are specified for special attention, including one cryptogamic group, Filices. This seems a vague syllabus, not likely to encourage the study of botany. How much and how little knowledge of physiology and histology will satisfy such terms as "nutrition," "structure"? No doubt the present is better than the old in omitting to insist on technical terms, some of them antiquated. But cannot more definite requirements be suggested for ensuring some practical insight into vegetable life on the part of the man who is to be stamped as an elementary botanist? Surely the best way is to let the knowledge be good, and sound, and practical, as far as it goes, giving some training in scientific method, and capable of further development in after life. We believe many would welcome a change giving the ordinary B.A. for a lower standard of attainment in the first part of the natural science tripos, thus doing away with the recognition of *dilettante* work in a single subject as a sufficient basis for a B.A. degree. A very satisfactory schedule is presented for zoology, requiring a knowledge of the anatomy of certain selected principal types, as well as the characters of orders, and the comparative anatomy and functions of the systems and organs as exemplified in the animal kingdom. Further, the general development of the embryo chick, the leading facts and conclusions respecting the geographical distribution of animals are included in the subjects. The schedule is to be discussed next Saturday.

THE Cambridge Council of the Senate has framed a draft statute to carry out the grace passed in December last in favour of the appointment of a general Board of Studies, representative in character, to report upon the proposals of each special board of studies as they arise, and so aid in holding the balance among the various interests concerned. The draft statute provides that the new Board shall consist partly of persons appointed on the nomination of the Boards of Studies, but abundant freedom is left to the senate to add other members and to vary from time to time the composition, mode of appointment, and duties of the new board.

AT the next meeting of the Governors of Addenbrooke's Hospital Mr. J. W. Cooper will propose: That a memorial be presented to Her Majesty's Commissioners for the University of Cambridge, under the seal of the Governors, representing that Addenbrooke's Hospital is extensively used as a place of study by the Medical Students of the University; that it is essential in the interests of the Medical School that it should not cease to be a recognised place of medical study; and, further, that as large endowments have been left to various colleges for the promotion of medical study, some adequate endowment should be made for Addenbrooke's Hospital out of the funds at the disposal of the Commissioners. There cannot be much chance of success for such a proposal unless it be made more definite. The hospital can only properly benefit by educational endowments by being the locus of the study and appliances of research in therapeutics, sanitation, and pathology.

GEOLOGICAL students at Cambridge will have plenty of work provided for next term. Prof. Hughes will give one course on the geology of the neighbourhood of Cambridge, and another stratigraphical course, beginning with the Permian, and ascending. Prof. Bonney will continue his lectures on elementary physiography, and will give weekly demonstrations on microscopic lithology. Mr. Tawney will be demonstrating the principal genera of fossil invertebrata; and both he and Dr. R. D. Roberts will give practical instruction in petrology. Lectures begin April 25. The first geological excursion of the term is fixed for Saturday, May 3.

MR. THOMAS W. BRIDGE, B.A., of Trinity College, Cambridge, and Demonstrator of Comparative Anatomy in the University of Cambridge, has been appointed to the Professorship of Zoology at the Royal College of Science at Dublin, vacant by the resignation of Dr. Leith Adams, F.R.S.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 13.—"The Contact Theory of Voltaic Action," No. III. By Professors W. E. Ayrton and John Perry. Communicated by Dr. C. W. Siemens, F.R.S.

The authors commence by referring to the experiments that had been made prior to 1876, on the difference of potentials of a solid in contact with a liquid, and of two liquids in contact with one another, and they point out that :—

1. The earlier experiments were not carried out with apparatus susceptible of giving accurate results.

2. Owing to the incompleteness of the apparatus assumptions had to be made not justified by the experiments.

3. *No direct* experiments had been performed to determine the difference of potential of two liquids in contact, with the exception of a few by Kohlrausch, using a method which appeared to the authors quite inadmissible as regards accuracy of result.

In consequence of this great vagueness existed as to whether the contact difference of potentials between two substances, when one or both were liquids, was a constant depending only on the substances and the temperature, or whether it was a variable dependent upon what other substance was in contact with either. Some authorities regarded it as a variable, Gerland considered he had proved it to be a constant, but first, the agreement of the value of the electromotive force of each of his cells with the algebraical sum of the separate differences of potential at the various surfaces of separation, and which was the test of the accuracy of his theory, was so striking, and so much greater than polarisation, &c., usually allows one to obtain in experiments of such delicacy, that one could not help feeling doubtful regarding his conclusions; secondly, his apparatus did not allow of his experimenting with two liquids in contact, consequently he could not legitimately draw any conclusion in this latter case. And although Kohlrausch had made some few experiments on the difference of potentials of liquids in contact, still since he employed moist blotting-paper surfaces instead of the surfaces of the liquids themselves, the authors considered for that reason alone, if for no other, that his results did not carry the conviction the distinguished position of the experimenter might have led them to anticipate.

They therefore designed a method and an apparatus for carrying it out, by means of which they could measure the difference of potentials, in volts, at each separate contact of dissimilar substances in the ordinary galvanic cells, from which they could ascertain whether the algebraical sum of all the contact differences of potential was, or was not, equal to the electromotive force of the particular cell in question. From the results they obtained, and which are given in Papers Nos. I. and II., *Proc.* Roy. Soc., No. 186, 1878, they concluded within the limits of their experiments that if $\overline{AB}$, $\overline{BC}$, $\overline{CD}$, &c., were the contact differences of potential measured *separately* of the substances A in contact with B, and neither in contact with any other conductor, B in contact with C, &c., then, any one or more of the substances being solid or *liquid*, if any number $A, B, C \cdots\cdots K$ were joined together, and the electromotive force of the combination $\overline{AK}$, measured, the following equation was found true :—

$$\overline{AK} = \overline{AB} + \overline{BC} + \overline{CD} + \cdots\cdots + \overline{JK},$$

which proved that each surface of separation produced its effect independently of any other.

Their method, by which any single contact difference of potentials was measured, was as follows :—Let 3 and 4 be two insulated gilt brass plates connected with the electrodes of a delicate quadrant electrometer. Let 1 under 3, and 2 under 4 be the surfaces whose contact difference of potential is to be measured; 3 and 4 are first connected together and then insulated, but remain connected with their respective electrometer quadrants. Now 1 and 2 are made to change places with one another, 1 being now under 4 and 2 under 3, then the deflection of the electrometer needle will give a measure of the difference of potentials between 1 and 2.

The apparatus employed by the authors in the present investigation is then explained in detail, and it is shown how, by improving on their earlier form, they have removed a difficulty which formerly existed, and which prevented their previously experimenting on pairs of substances having very different weights, such as a vessel of mercury and a sheet of metal.

The authors explain that the results they have obtained in this investigation have divided themselves into three groups :—

1. The contact difference of potentials of metals and liquids at the same temperature.

2. The contact difference of potentials of metals and liquids when one of the substances is at a different temperature from the other in contact with it—for example, mercury at 20° C. in contact with mercury at 40° C.

3. The contact difference of potentials of carbon and platinum with water, and with weak and with strong sulphuric acid.

They mention, however, that they give only the results under head No. 1 in the present communication, reserving those they have obtained under heads Nos. 2 and 3 for a future occasion.

Then follow arranged in the order in which they were obtained from January to May, 1878, some 150 results of experiments (each result given being on the average the mean of ten observations), representing the contact differences of potential of nine solids and twenty-one liquids. The authors explain that many of the results they obtained are not mentioned in the paper, having been rejected on account of inaccuracies arising from the great delicacy of the experiments. These remarks especially apply to the authors' attempts to measure the contact difference of potentials between a liquid and a paste—for example, mercury and mercurous sulphate paste, great difficulty being introduced by the extremely thin layer of water on the surface of the paste acting inductively instead of the paste itself. They mention that this difficulty is a very good example of the inaccuracies that must have been introduced by former experimenters using a moist blotting-paper surface instead of the surface of the liquid itself.

A large number of discordant results were obtained in March, 1878, and their explanation led to the interesting result that the apparent contact difference of potentials between a metal and mercury, as measured inductively, varied much with small additions of temperature. The investigation of this apparent change of contact difference of potentials with temperature led to a consideration of the contact difference of temperature of mercury with air, since, of course, in all these inductive experiments two air contacts are included in the result.

It has usually been thought that the differences of potential of liquids in contact with one another were so small as to be almost inappreciable in comparison with the differences of potential of metals in contact; but the authors have ascertained, among other results, that strong sulphuric acid in contact with distilled water, solutions of alum, copper sulphate, and zinc sulphate, has a measured difference of potentials of 1·3 to 1·7 volts, or an electromotive force more than twice as high as that of zinc and copper in contact. And hence the great importance of an apparatus that can directly measure the difference of potentials of two liquids in contact.

Zoological Society, March 18.—Prof. St. George Mivart, F.R.S., vice-president, in the chair.—The Secretary called the attention of the meeting to the herd of Japanese Deer (*Cervus sika*) in the park of Viscount Powerscourt, at Powerscourt, in Ireland, now about eighty in number, and gave an account of their introduction and history, from particulars supplied to him by Lord Powerscourt.—A communication was read from Dr. G. Hartlaub, containing the description of a new species of Barn Owl, from the island of Viti-levu, which he proposed to call *Strix oustaleti*.—Mr. Edward R. Alston read a paper on female deer with antlers, showing that these weapons are not unfrequently abnormally developed in fertile females of certain species of *Capreolis* and *Cariacus*, and giving reasons for believing that in the ancestral forms of deer they were probably common to both sexes.—Mr. Sclater made remarks on some of the rarer parrots living in the Society's Gardens. The whole series of this group in the Society's collection was stated to consist of 170 individuals belonging to ninety-eight species.—A communication was read from Prof. Garrod, F.R.S., containing notes on the visceral anatomy of the Tupaia of Burmah (*Tupaia belangeri*). The cæcum coli in this animal was stated to be small, whilst in a specimen of *T. tana* it was ascertained to be wholly wanting.—A second communication from Prof. Garrod contained notes on the anatomy of *Helictis subaurantiaca*, in the course of which he showed that the hippocampal gyrus of the brain is partly superficial in this animal, which is not the case in any other carnivorous animal yet recorded.

Linnean Society, March 20.—William Carruthers, F.R.S., vice-president, in the chair.—The Rev. G. E. Commesford Casey was elected a Fellow of the Society.—A paper by Mr. Fred. Smith, on new aculeate hymenoptera from the Sandwich Islands, collected by the Rev. T. Blackburn, was read. The author states the general aspect of the series is certainly North American, with mixture of a few South American forms. The ants are most diverse in character, some being cosmopolitan in range. The house ant of Madeira is common, and the little European ant (*Ponera contracta*) unexpectedly turns up here.—Some observations on the reproduction of ferns, by Mr. T. R. Sim, were also read by the Secretary in the absence of the author. Among the great collection of living ferns at Kew a marked feature is the large number of species that regularly bear adventitious buds. Of a thousand species there grown barely fifty are ever found without buds, and some forms produce them regularly, though the normal forms do not. The above number seems very high when compared with Phanerogams, where adventitious buds, with some few exceptions, may be said never to be normal. Among viviparous ferns the contrary obtains, and the buds are always on the same part of the plant in all the individuals of a species. *Polystichum angulare*, for example, bears a bud on the rachis in the axil of almost every pinna on the lower part of the frond, in some all up the rachis. Some Aspleniums produce them on the veins of the upper surface of frond, but never directly through from a sorus. Great variety in position, however, is manifested in different genera and species where budding occurs, various examples of which the author gives. Where buds become detached, considerable difference obtains as to size and stage of separation, whereof many instances are pointed out and other curious instances of deviations related by the author. In commenting on the subject, Sir J. D. Hooker stated his belief that ferns at Kew were more bulbiferous than in their natural state, possibly from more constant nutrition and warmth.—The fifth contribution to the ornithology of New Guinea, namely, recent collections from the neighbourhood of Port Moresby, was read by the author, Mr. R. Bowdler Sharpe. The interesting series dwelt on were obtained by Mr. Kendal Broadbent, and usefully compare with those got by Signor Albertis from the Fly River. A parrot of the genus *Aprosmictus* closely resembles one from the Fly River, but nevertheless is specifically distinct, offering thus a parallel case to the crowned pigeons, *Goura albertisi*, inhabiting Port Moresby, and *G. sclateri*, the Fly River. At present the affinities of the South-Eastern species seem to be with those of Australia, a few to those of the Aru Islands.—Mr. W. T. Thiselton Dyer exhibited *Helichrysum vestitum*, a perennial everlasting, from the Cape of Good Hope.

Anthropological Institute, March 11.—Mr. E. Burnett Tylor, D.C.L., F.R.S., president, in the chair.—The president read a paper entitled "The Geographical Distribution of Games," in which attention was called to the games of Polynesia and America as proving that a drift of civilisation from Asia reached these regions before they were known to Europeans. The draughts played in the Sandwich Islands and New Zealand were not our modern game, but apparently some variety related to the ancient classical game (which is alive in Egypt to this day). It may have reached the South Sea Islands from Eastern Asia, together with kite-flying, at which they were experts, and which they perhaps had before the comparatively modern time when it reached England.

Royal Microscopical Society, March 12.—Dr. Beale, F.R.S., president, in the chair.—The following papers were read:—Contribution to the knowledge of the British Oribatidæ, by A. D. Michael, F.R.M.S.—The development and retrogression of fat cells, by G. and F. E. Hoggan.—Microscope with swinging sub-stage and improved motions, by J. Beck, F.R.M.S.—The use of osmic acid in microscopical preparations, by T. J. Parker, F.R.M.S.—Other papers by Prof. Keith, Mr. Tolles, and Mr. Crisp were taken as read, or postponed in consequence of want of time.—The new $\frac{1}{18}$ oil immersion objective by Zeiss was exhibited, with remarks by Prof. Abbe on the Stephenson homogeneous immersion system.—A large number of objects were exhibited illustrative of the papers read and otherwise, together with microscopes and apparatus by Mr. Crisp.—Lord Justice Bramwell and six other gentlemen were elected Fellows.

Photographic Society, March 11.—James Glashier, F.R.S., in the chair.—Papers were read: On coloured glass suitable for the developing-room, and on the employment of quinine as a substitute, by Capt. Abney, R.E., F.R.S., who, in illustration of his paper, exhibited photographs of the solar spectrum taken through various stained glasses, and stated that a combination of cobalt and stained red glasses secures immunity from the actinic action of light, and that collodion-films on both sides of a glass, stained with either magenta, aurine, or chrysoidine,

practically are also non-actinic. Quinine, he found, cuts off the ultra-violet rays, and no others.—Mr. C. Bennett, on the gelatine emulsion process, enforced the fact that the extreme sensitiveness of his process was produced by the long time he allowed the gelatine and salts to emulsify or ripen before eliminating the bromide and silver not taken up.—Mr. W. Wainwright, jun., note on Bennett's gelatine emulsion process; also, Mr. Howard Grubb exhibited some new forms of stereoscopes, one designed to exhibit pictures of a much larger size than ordinary.

Institution of Civil Engineers, March 11.—Mr. Bateman, president, in the chair.—The paper read was on movable bridges, by Mr. James Price, M. Inst. C.E.

PARIS

Academy of Sciences, March 17.—M. Daubrée in the chair.—The following papers were read:—On an electric burner and blowpipe, by M. Jamin. Two carbons are supported vertically abreast, hinged below, and drawn together at the top by a spring. A current is sent up one (A), down the other (B), then round a rectangular circuit inclosing the two, and passing first round A; by current attraction the carbons are drawn apart, and the arc appears at the top and descends gradually, consuming one or both carbons. When the action of the rectangle is sufficient, the arc driven beyond the points is like a gas flame, and M. Jamin receives it on a piece of lime, magnesium, or zirconium, getting intense light. It is also so hot as to fuse the lime, and the author recommends it as a blowpipe to chemists and physicists.—On a meteorite belonging to the group of eukrites, which fell on July 14, 1845, in the Commune of Teilleul (Manche), by M. Failli.—M. Larrey communicated a letter from M. Tholozan, Persia, on the plague, which he shows to have sprung up and died out in certain localities in the absence of restrictive measures. The French Government have sent Dr. Zuber to Astrakhan to study the disease.—On a new type of anomalous stems, by M. Cornu. This relates to supplementary cortical ligneous bodies anastomosed together, in certain *Sempervivum* and *Crassula*; their *rôle* seems to be to strengthen the fragile stems when they have to bear a large inflorescence.—On the amyloid granules of the yolk of eggs, by M. Dareste. He urges reasons for thinking the granules starch, and not lethicine (as has been affirmed). They are difficult to study.—On the correspondence between Chladni's acoustic figures and liquid systems produced on vibrating circular plates, by M. Decharme. It was stated that the Italian Society of Natural Sciences had formed a service of antiphylloxeric vedettes, to survey vineyards, and report the first suspicions of the disease.—On a new catadioptric telescope, by MM. Paul and Prosper Henry. The tube of a reflecting telescope is hermetically sealed by means of a thin crown glass lens of the same size as the mirror, very slightly concave, and not detracting from the optical power of the instrument. The instrument has given remarkable results.—Demonstration of the convergence of a double series met with by Lamé in his researches in mathematical physics, by M. Escary.—On the integration of a differential equation, by M. Halphen.—On the determination of the imaginary roots of algebraic equations (concluded), by M. Farcas.—On a system of light signals permitting the determination of differences of longitude between different stations not connected electrically, of a triangulation of parallel or meridian, by M. Liais. This system has been adopted in Brazil. M. Liais shows the advantage of making rhythmic signals commanded by a clock, and received at the other station by a chronographic inscription. In this way there is no variable personal equation to be concerned about. The point is to make a screen, with aperture, beat seconds (*e.g.*, by a clock commanding an electro-magnet) so as to give an instantaneous appearance of light each second. The receiving-station may either register with a chronograph or (better) compare directly the clocks of the two stations by the method of coincidences; a screen, with aperture, passing before the objective of the telescope, and the light seen only when the two clocks are in coincidence.—On the distribution of heat on the sun's surface; results of the first series of observations at the Imperial Observatory of Rio de Janeiro, by MM. Cruls and Caille. These researches fully confirm the results got by Secchi, though there are some differences as to absolute value of radiations. The absolute radiation of the whole disk was estimated at $\frac{22}{100}$, the absorption, $\frac{78}{100}$.—Determination of the approximate value of a coefficient relative to the viscosity of water, by M. Geoffroy.—New experiments on telephones without a diaphragm, by M Ader. He gets better results than with an ordinary telephone from a thin piece of wire (with small helix round it) fixed at one end in a wooden board (a microphonic speaker being used), better if the two ends are in contact with metallic masses. Voice is reproduced, too, from a mere bobbin without core, if the windings are very free. He supports M. du Moncel's view that the sounds in the telephone are from contractions and elongations of the magnetic rod.—M. Du Moncel described some observations in the same sense.—M. Resis presented a note on a hydro-electric telephone, in which the variations in intensity of the current are reproduced by variations in resistance of a liquid column, which serves as receiver (without any electro-magnetic organ).—On new combinations of hydrochloric acid with ammonia, by M. Troost. The two specified are the tetra and hepta-ammoniacal chlorhydrates.—Combinations of phosphuretted hydrogen with cuprous chloride, and its determination in gaseous mixtures, by M. Ribau.—On the crystalline form of combinations of stannmethyls and their homologues, by M. Hiertdahl.—On a new process of treatment, by the dry way, of iron and copper pyrites, by M. Simonin. This relates to Mr. Holloway's method.—On the state in which precious metals are found in some of their combinations; ores, rocks, products of art, by MM. Cumenge and Fuchs.—On the constitution of coal, by M. Guignet. He treated powdered coal with phenol, nitric acid, &c. With the latter he obtained, *inter alia, trinitroresorcine* (oyxpicric acid), probably from resinous or waxy matters retained in the coal. No resorcine was found.—On alcoholic fermentation, by MM. Schutzenberger and Destrem. Yeast prevented from developing and multiplying still retains its power of decomposing sugar; and yeast acting on sugar deassimilates more nitrogen than that kept in presence of water, but without sugar and oxygen.—On the determination of glucose in the blood, by M. Cazeneuve. He criticises Bernard's method (by cupropotassic liquor) as inexact, and thinks the study of glycæmia should be taken up again when a more precise method is acquired.—On the derivatives of normal methyloxybutyric acid, by M. Duvillier.—Analysis of some fodders, and observations on damage done to Italian beans by weevils, by M. Grosjean.—Comparative evolution of the male and female genital glands in the embryos of mammalia, by M. Rouget.—On the non-excitability of the grey cortical substance of the brain, by M. Couty.—Note on the history of peduncular expansions, by M. Bitot.—On the nature of the albumen of hydrocele, by M. Bechamp.—Experimental researches on a leptothrix found during life in the blood of a woman attacked with grave puerperal fever, by M. Feltz.—On the modifications of the physical properties of starch, by M. Musculus.—On ferruginous particles observed in dust brought by a blast of sirocco to various parts of Italy, by M. Tacchini. This was in February. He thinks the phenomenon of so-called *meteoric* spherules must in many cases be attributed to this phenomenon.—Morphology of the dental follicle in vertebrates, by MM. Legros and Magitot.—Pathogeny and treatment of intermittent convergent strabism, without operation, by use of mydriatics and myosics, in children, by M. Boucheron.

CONTENTS

THURSDAY, APRIL 3, 1879

COLOUR IN NATURE

The Colour Sense: its Origin and Development. An Essay in Comparative Psychology. By Grant Allen, B.A. (London: Trübner and Co., 1879.)

THIS interesting and suggestive work deals with the whole question of colour in nature, and more especially with its manifestations in the organic world and the complex colour reactions between plants and animals. It traces the origin of the colour sense in insects to their visits to primeval flowers in order to feed upon the pollen, and in birds to their seeking for fruits, whose seeds they dispersed and whose colours were developed to attract them. It thus attempts to show that the very existence of most of the brilliant colours of the organic world is due to the influence of the colour sense in animals. The author adopts, with some reservations, Mr. Darwin's theory of sexual selection to account for the colours of most animals, and he endeavours to show that only those groups display beautiful colours in which a taste for colour has been aroused by the influence of flowers, fruits, or brilliant insects, their habitual food. All these subjects are treated in a very thorough manner, with a wealth of illustration, a clearness of style, and a cogency of reasoning, which make up a most attractive volume; and though we may not agree with all the author's conclusions, and may even doubt the accuracy of some of his facts, we cannot but admit that he has placed the whole subject before us in a way that must engage the attention both of the man of science and the general reader. We will now proceed to give an outline of the whole work, dwelling here and there on the more interesting points, and especially on those where we venture to differ from the conclusions arrived at.

After an introductory chapter, the contents of which are above indicated, an excellent account is given of the nature of light, and of those peculiarities of the æther-waves which produce in us the sensations of light and colour. The third chapter deals with the organ of vision, giving an account of its earliest appearance and progressive complexity in the animal kingdom, and of the structure of the eyes of the higher animals, and the relation of their parts to the perception of light and of differences of colour. One of the most important facts here brought out is, that the complex mechanism required to produce vision has been several times independently evolved—the eye of the bee, of the cuttle-fish, and of the eagle have each apparently been separately developed from unlike remote sightless ancestors.

The next chapter is a long and very interesting one, on "Insects and Flowers." It deals with the origin and development of these two classes of organisms and their actions and reactions on each other. It is full of interesting facts; and the discussion of the mode of origin of the colours of flowers by a reference to the conditions under which colour appears normally in living plants is especially instructive; the generalisation being arrived at that the leaves which create or store up energy for the plant are green, while whenever leaves lose this function and become expenders of energy they lose the green tint and acquire various other colours; growing shoots, young leaves, buds, stamens and stigmas, and their protecting scales, are almost always variously coloured. The rudiments of colour being thus always ready in the floral organs, it is not surprising that flowers have been separately developed in monocotyledons and dicotyledons, and also probably many times over in each of these divisions.

In this part of his work the author exhibits his tendency to trust far too much to negative evidence, especially to that afforded by geology. He speaks of the carboniferous epoch as presenting a green jungle of ferns and club-moss, "in which there is no trace of bee or moth or joyous butterfly;" while "scarlet berry and crimson blossom, gorgeous bird, and painted insect were all equally absent from the unvaried panorama of green overhead and brown beneath." As the flora preserved to us in the coal-measures was almost certainly that of swamps only, we cannot possibly tell what existed on the uplands and mountains of that period. The enormous differentiation of flowering plants, and the comparatively little change they seem to have undergone during the whole tertiary period would lead to the inference that they may have already existed in some variety during the carboniferous epoch; while the actual discovery of a butterfly in the lower oolite, and of a well preserved wing of what appears to be a large moth in the carboniferous shales of Belgium,[1] renders it quite possible that coloured flowers and gay butterflies *were* then in existence. The statements as to the time when the different orders of insects first came into being (quoted at p. 68) are quite worthless when we consider how rare must be the conditions leading to the preservation of winged insects, and they are already contradicted by well-known palæontological facts. Another statement that seems equally open to doubt is, that even in early tertiary times there were no orchids (p. 97), a statement founded on the generalisation that entomophilous monocotyledons are later productions than entomophilous dicotyledons, because the perianth of the former is usually less specialised. But surely in orchids the perianth is more highly specialised than in any existing flowers whatever; and if we take into account the world-wide distribution of these plants, their immense richness in genera and species, and their wonderful complexity of structure, we must consider them as among the most ancient instead of the most recent of flowers. They are also exceptions to the general rule of the size of the flower being in inverse proportion to its special adaptation to insect fertilisation; of which the large but simple lilies and tulips as contrasted with the small but complex labiates, are quoted as examples.

The next chapter, on the colour sense in insects, sets forth both the direct and the indirect evidence on this question; such as Sir John Lubbock's experiments on bees and wasps, the mimicking insects which deceive other insects, the clear relation of coloured flowers to the visits of insects, the fact of insects often visiting hundreds of the same species of flower in succession, &c. This chapter concludes with a striking picture of the vast effect which has been produced on the appearance of external nature by insect agency, "which has turned the

[1] *Breyeria borinensis*, "Annales de la Société Entomologique de Belgique," t. xviii. Pl. v. (Photograph).

whole surface of the earth into a boundless flower-garden," supplying insects from year to year with pollen or honey, and itself gaining in return a renewal of life by means of the baits that it offers for their allurement. "If," adds Mr. Allen, "any man can seriously doubt that these changes are really due to a colour sense in the little creatures which live upon the beautiful flowers; if he can imagine that the plant has produced its gorgeous petals for no other purpose than that of suicidal wastefulness; that the *Mantis* has grown into the perfect semblance of a leaf from pure wanton causeless mimicry; that the lurid red of fly-fertilised blossoms bears its likeness to the mangled flesh of animals by a simple freak of creative power; then the whole science and philosophy of the last hundred years have been thrown away upon him, and he may return at leisure to the blind and hopeless chance of the eighteenth century atheists."

The relation of birds and mammals to fruits is next discussed, and this is shown to be in many respects parallel to that of insects and flowers, only those fruits being conspicuously coloured which are edible, and the dispersal of whose seeds is effected by the birds or other animals which eat them. The whole of this subject is very well treated, but the evidence that fruits in general have been modified both in edibility and attractiveness in relation to the animals which feed upon them, is by no means so clear as in the case of flowers. With regard to small and hard-seeded fruits, such as our strawberries, currants, and raspberries, our hips and haws, our yews and cranberries, this is no doubt the case, since they are carried away by birds and vegetate after passing through their bodies. It is also the case with such fruits as the nutmeg, whose bulky seeds pass undigested through the stomachs of the great fruit pigeons, but whether the same rule applies to most of the larger fruits may be doubted except when they have hard, stony seed-coverings, as in the case of plums and apricots, which evidently protect the seeds from being eaten, or, if eaten, from being digested. But the majority of the larger fruits are eaten by mammals, and it is doubtful whether their seeds can survive the process. Such are oranges and shaddocks, and gourds of various kinds, while many large bright-coloured fruits of the tropics do not seem to be eaten at all. Many of these are very round and smooth, and may get dispersed by mere rolling down hill, as occurred with the mango in Jamaica,[1] or by being accidentally disturbed by the feet of animals. It is to be observed, too, that the fruits of trees are usually so abundant that, if eatable, there is no danger of their not being eaten even if uncoloured, as in the case of our acorns, beech-nuts, and chestnuts. An immense number of the tropical fruits eaten by monkeys and parrots are not coloured, and the half-developed seeds are often alone eaten; while in others, as the jack-fruit, bread-fruit, and durian, the large seeds are as eatable as the pulpy mass, and the edible nature of the fruit must be injurious rather than otherwise as leading to the destruction of seeds. This need be no difficulty when we consider that with forest trees, which live for several centuries, there is only vacant space for young trees at long intervals, and thus no rigid selection of seeds takes place tending to secure them from being destroyed as food for animals.

[1] See Sir Joseph Hooker's lecture at the Royal Institution on "The Distribution of the North American Flora."

On account of the fondness of most birds and other animals for the very same fruits which we like best, Mr. Allen maintains the general community of taste in all animals. I have, however, usually found monkeys eating fruits which were very disagreeable to me, and the theory is hardly consistent with the fact that many nauseous fruits are bright-coloured. Thus the *Citrullus colocynthus* of Palestine has a beautiful fruit of the size and colour of an orange, but, according to Canon Tristram, "nauseous beyond description to the taste,"—while the *Solanum sanctum*, generally called the "Dead Sea apple," is almost equally disagreeable, but is of a brilliant red colour. Now if these fruits are eaten by any animals their taste must be very different from ours, while if they are not, these fruits have become strikingly attractive from other causes than to induce animals to eat and disperse them. This latter view is supported by another fruit, also found in Palestine, the *Calotropis procera*, which is as large as an apple and bright yellow, but is full of thin flat seeds winged with exquisitely fine silky filaments. Here, then, the seeds having special powers of dispersal by the wind do not need the aid of animals, yet the fruit is most attractively coloured. This is one of the Apocynaceæ, which are usually poisonous, and I have observed brilliantly coloured fruits of the same order in the tropics, but some of these are known to be eatable. Taking into consideration all the facts, it seems probable that attractive fruits are more abundant among the smaller trees and shrubs of temperate lands than in the forests of the tropics, and that their colours are largely due to those adventitious causes which our author has himself so well elucidated. When their distribution has been aided by birds their colours, their edibility, and the non-digestibility of their seeds would all be increased by natural selection. The dry fruits of herbaceous plants, in which the struggle for existence is probably more severe, have no doubt often been prevented from acquiring bright colours by natural selection in order to protect their seeds, just as so many insects and birds have acquired brown or green protective tints.

A curious point in relation to this question, and one that has not been noticed by our author, is the very different characteristic colours of fruits and flowers. I have tabulated the colours of these, under four heads, taken from two books of manageable size—Hooker's "British Flora" and Mongredien's "Trees and Shrubs for English Plantations." The colours of the two classes I find to be as follows, dividing the purples between the red and blue to the best of my judgment, and taking black among fruits as corresponding to blue in flowers.

Flowers.	White.	Yellow.	Red.	Blue.
British Flora	292	228	168	123
Trees and Shrubs ...	160	73	62	37
Totals	452	301	230	160
Fruits.				
British Flora	2	3	33	24
Trees and Shrubs ...	5	11	35	21
Totals	7	14	68	45

Here we see that white and yellow which immensely preponderate in flowers are very scarce among fruits, among which red and blue (or black) predominate, the two colours which are far less common in flowers. We must

conclude, either that there is not a community of taste in colour between insects and birds, or, that what may be termed the normal colours of both have been more or less intensified and utilised by natural selection in order to attract insects and birds respectively.

The next chapter, on the colour-sense in vertebrates, clearly establishes the fact of the possession of this sense by all vertebrate animals, but more especially by birds and reptiles. The evidence of such a sense in mammalia generally is very scanty, though it undoubtedly exists in monkeys; while there are good reasons for believing that it is more acute in birds than even in ourselves. Birds on the whole need to perceive colour more than any other animals, both because the insects and fruits and buds on which so many of them feed are small variously-coloured objects, and because from their habits they require to see and recognise these objects from a considerable distance. It is therefore a remarkable confirmation of the modern theory—that the cones of the retina are colour organs while the rods are only light organs, that in birds the cones are three times as numerous as the rods, while in mammals they are less numerous. Nocturnal birds, such as owls, however, have very few cones, while nocturnal mammals have none. The *macula lutea*, a central yellow spot consisting largely of cones, is found in man and monkeys only, while it exists in all diurnal birds, and these in addition have their cones furnished with variously-coloured globules, which are supposed to give a still more perfect perception of colour. The eye of the chameleon is as perfect as that of a bird, and this accords with its capacity of colour change, and the extreme accuracy with which it detects and captures insects. Mammals, on the other hand, even the insectivorous and frugivorous kinds, have very little occasion for a refined colour sense, since the great mass of creeping insects are of obscure colours, while the squirrels and allies feed on brown nuts rather than on coloured fruits. The evidence seems to show, therefore, that a tolerably perfect colour-sense has only been attained, among mammalia, in the monkeys and man, while even in these it is probably very inferior to that of birds. It seems probable, therefore, that the prevalence of colour-blindness is really an indication of the colour sense in man having been a comparatively recent development, instead of being, as Mr. Allen thinks, a disease of civilisation. An acute colour sense is certainly not of the first importance to savages; and though our author has adduced valuable evidence that most savages distinguish colours just as well as we do, it is very important to ascertain whether colour-blindness exists among uncivilised peoples to a greater or a less extent than among Europeans.

The next chapter, on the direct action of the colour sense upon the animal integuments, deals with the theory of sexual selection as advanced by Mr Darwin, and endeavours to support it by a variety of general considerations. Many of these arguments are very weak, and are often founded on insufficient or erroneous facts, some of which I shall endeavour to point out. The great aim of this chapter is to prove that the colours of animals are intimately associated with the colours of the objects they feed upon. Butterflies and moths being the most beautifully coloured of all insects and feeding on flowers, is held to be the first great fact in support of this view; and this is backed up by the remark that "the colours of caterpillars are mostly protective, being due to natural selection alone, while those of butterflies are mostly attractive, being largely due to sexual selection." To this we must altogether demur, as slurring over what is really a stupendous difficulty in the way of the theory. So far from the colours of caterpillars being "mostly protective" every entomologist knows that a large number of caterpillars in every part of the world are conspicuously coloured, and what is more to the point that their colours are as brilliant and varied as those of butterflies themselves, if we take into account the nature of their integument, the small amount of surface, and the uniform cylindrical form of their bodies. The caterpillar of *Papilio dissimilis*, for instance, on a bluish green ground has a series of broad irregular longitudinal bands of the richest orange yellow, and between these there are a number of round red spots; while those of many of the Euplæas are adorned with exquisite pink and yellow markings, and with a number of long fleshy processes of equally brilliant colours. Owing to caterpillars being so difficult to preserve, and being rarely collected and figured in their native countries, comparatively few of them are known, but it is certain that they often exhibit the most brilliant hues and the most exquisite patterns; and as they may be said to feed invariably on green leaves, while sexual selection cannot affect them, the natural inference is that the same general laws which produce colour in them are quite sufficient for the production of even more varied hues in the perfect insects, whose expanded wing surfaces, ever varying in size, form, and neuration, offer a field so much better fitted for its development.

In beetles the appearance of colour is also attempted to be correlated with their flower-haunting habits by means of equally doubtful facts. The magnificent Buprestidæ and Longicorns are, as far as my experience goes, almost wholly wood-feeders, frequenting the bark of dead trees, and very rarely found on flowers; the Cleridæ and Silphidæ feeding on dead animal matter, are often brilliantly coloured; and generally in beetles, the absence of colour may be traced to the need of concealment and protection, while whenever a special mode of protection exists, whether by nauseous secretions, hard integuments, rapid flight, or facilities for concealment, then colour appears in infinitely various phases; and this law generally prevails throughout the whole insect-world. In his argument in favour of bright hues being attractive to the opposite sexes of insects, Mr. Allen seems always to forget that it is the male that is attracted to the female, and not *vice versâ*; and when he says (p. 158) that he "cannot see why Mr. Wallace, who allows the attractive nature of colouring in flowers, should deny its attractive nature in the question of sex," I reply, that in flowers colour enables the insect to recognise the species, but no one has ever asserted that insects improve and alter the colour of flowers by their preference for certain varieties of colour irrespective of the honey or pollen produced; and in like manner I maintain that the colour of an insect is a guide to easy recognition by its mate, but that there is not one single particle of evidence to show that minute differences in the colour of the same species are observed by insects, still less that such differences are so important

to them as to lead to the rejection of a healthy and well-organised mate; yet unless this is the case, the whole theory of sexual selection falls to the ground.

Again, the general connection between coloured flowers and coloured insects is by no means so general and constant as Mr. Allen supposes. Perhaps the richest displays of gay flowers in the world are to be found in temperate Australia, in South Africa, and in the South European Alps, yet in all these countries the butterflies are very inferior to those of tropical forests, where flowers are comparatively rare. In the forests of Para, for instance, gay flowers are very scarce, as noticed by Mr. Bates as well as by myself, yet the butterflies are endless in their variety of lovely hues. Of course there *are* bright flowers in the tropics, and as travellers notice these whenever they see them and also notice the handsome butterflies, it is easy to infer, as is here done, that the two invariably go together. We may also remark that the sexual allurement of a peculiar odour given out by special patches of scales on butterflies' wings has been discovered by Fritz Müller in the genera *Mechanitis*, *Dircenna*, and *Thecla*, all very brilliantly coloured groups, a clear indication that colour is not a sexual allurement, or we should find it most developed, not in conjunction with, but in the absence of, the attraction of odour.

We must now pass on to the vertebrates, and we here find very good evidence adduced of the existence of a colour-sense in fishes, reptiles, and birds, as we should expect from the known structure of their eyes; while in the case of mammals it is far less decisive. The attempt to associate the brilliant colours of these animals with their food and surroundings, acting through sexual selection, is, however, what we have now to consider; and though many alleged facts are adduced in support of it, several of them are as doubtful and inconclusive as in the case of insects. We shall confine our attention to the birds, which are the stronghold of the theory, and are so much more completely known than the less highly-organised fishes and reptiles. Mr. Allen claims the parrots as fruit-eaters, but they are really seed-eaters, their bills being specially formed to crack the shells and extract and grind up the kernels of nuts and other fruits. They do not therefore aid in the disposal of seeds, as they feed on brown nuts or unripe green fruits from which they extract the seeds, much more frequently than on coloured ripe fruits. The general green colour of parrots is undoubtedly protective, and this green colour is lost, and vivid tints appear just in proportion as, owing to various conditions, the need of concealment diminishes. This is especially the case in countries where mammals are few and a low type of organisation prevails, as in the Australian region, in Madagascar, and in South America; while in Africa and Asia, where a higher type of organisation prevails, the colours of parrots are more sober and protective. A little further on we find the Australian honey-suckers noted for their magnificent coloration; the fact being that they are decidedly a dull-coloured group, hardly superior to our thrushes, and not equal to our finches. Yet they are as universally flower-feeders as the humming-birds themselves; and the total absence of brilliant colour from these birds, which are *the* characteristic family of Australia, and have been developed in correlation with the brilliant Australian flora, absolutely negatives the idea of colour in birds being dependent on the amount of colour in the food and surroundings of certain groups. Again, the ground-feeding pheasant family are passed over as containing only one brilliant bird, the peacock, whereas it abounds in species of the most gorgeous colour. Such are the Impeyan pheasant of the Himalayas, whose metallic plumage is that of a gigantic sun-bird; the golden pheasant, the silver pheasant, and Reeves' pheasant of China, all unsurpassed for gay and conspicuous colouring; the glorious crimson and white-spotted tragopans, the elegant peacock-pheasants, and the intensely brilliant fire-backed pheasants of the Malay countries—together composing a group of birds whose colours are unsurpassed for beauty and splendour, and thus are directly opposed to the general gloom and absence of colour in their habitual surroundings.

In treating of mammals we find an equal want of discrimination in estimating comparative colour and conspicuousness. The tigers, the zebras, the beautifully marked antelopes, and the spotted deer and giraffes, which are really among the most brightly-coloured of all mammals, are passed over as less beautifully coloured than the squirrels and monkeys, in order to support the theory that arboreal mammals feeding on fruits should be (though unfortunately for the theory they are not) the most brightly coloured. Monkeys, as a rule, are very dingy brown or black, about one or two per cent. of the species having patches of bright colour on the bare skin of various parts of their bodies, while the nut-eating squirrels as a whole are certainly not superior to the grazing antelopes. In the summary of facts given at pages 184 and 185 there are many errors. *Scissirostrum Pagei* does not "belong to a family generally dull," while it is itself decidedly dull-coloured; the "pretty cigana" is a very plain coloured bird; Santarem, of which it is said "the pastures are destitute of flowers, and also of animal life, with the exception of a few small plain-coloured birds," is one of the richest localities for flowering shrubs in South America, and one of the few places where I remember the conspicuously coloured fruits on many of these shrubs, while the butterflies in the adjacent forests are gorgeous in the extreme; and lastly, the "gay-coloured squirrel," for which I myself am made responsible, is one of the dullest of the group, pretty indeed as are all squirrels, owing to its brown and yellowish ringed tail, but in no sense whatever "gay," while I certainly say not a word about its feeding on "bright-coloured fruits."

Such mistakes as these pervade this portion of the work, and are made the foundation for repeated argument and illustration; and they serve to show how impossible it is even for the most earnest and enthusiastic student to make a few months' labour suffice for a correct appreciation of the bearing of the overwhelming mass of facts presented by the countless species of the animal and vegetable world. I have marked a number of other passages to which I altogether demur, but many of them involve arguments which would extend far beyond the limits of an article. For the same reason I can only briefly refer to the concluding chapters on the "Colour Sense in Man," in which the theory of Mr. Gladstone and the German philologists is disproved in a manner which is absolutely conclusive.

In the summary and recapitulation we find all the facts

and arguments we have referred to marshalled in an imposing array, and finally summed up in the following condensed formula :—

"Insects produce flowers. Flowers produce the colour-sense in insects. The colour-sense produces a taste for colour. The taste for colour produces butterflies and brilliant beetles. Birds and mammals produce fruits. Fruits produce a taste for colour in birds and mammals. The taste for colour produces the external hues of humming-birds, parrots, and monkeys. Man's frugivorous ancestry produces in him a similar taste; and that taste produces the final result of human chromatic arts."

Although I totally differ from Mr. Allen's conclusions as to the production of the varied colours of the animal world, I must express the extreme pleasure with which I have read his book, which I most cordially recommend to all who love colour, and can enjoy a thoroughly well-written volume on a most interesting but difficult subject.

ALFRED R. WALLACE

GEODESY

Die geodätischen Hauptpunkte und ihre Coordinaten. Von G. Zachariæ. (Berlin: Oppenheim, 1878.)

THE science of geodesy, though far from a popular one, exercises something like a fascination over its own devotees. It is not a standstill science; how to devise instruments—theodolites, altazimuths—which shall excel their predecessors; how to use these instruments so as to eliminate the sources of possible error they individually present; how, having got the observations, to eliminate in the use of them, their own errors as far as possible; and finally, how, after obtaining final results, to express the degree of reliance to be placed on them: these are all ever-fresh questions, capable, many of them, of engaging—as one may, for instance, see in the works of the late Prof. Hansen—considerable mathematical ability. The work before us is of Danish origin, and it is clear that the Danish meridian arc and the geodetic operations connected therewith have been executed in a thoroughly scientific manner. To those who are employed in geodetic operations, this treatise will be most welcome. In an introductory chapter we have the definitions of the mathematical surface of the earth, expressions for the radius of curvature and various lines connected with the spheroid, and remarks on the deviation of the actual surface from that of a true spheroid. The first section treats of the method of laying out a triangulation, of the measurement of angles, and of the measurement of base lines, together with the calculation of the probable errors of results. The second section deals with the calculation of triangles: after giving Legendre's theorem, the writer shows how spheroidal triangles may be computed as spherical, and gives the expressions for the differences between the angles of a spherical triangle and a spheroidal triangle having sides of the same length, with any position in azimuth. Then the method of calculating a triangulation by least squares is entered into. The third section deals with the subsequent expression of the results in the form of co-ordinates—of the method of calculating differences of latitude and longitude. Throughout the work, in all formulæ which are approximative, the nature or order of the terms omitted is expressed by a neat notation which is very useful. The fourth section is devoted to the measurement of heights, and levelling operations and calculations; the subject is gone into thoroughly, including the investigation of the coefficient of terrestrial refraction and the errors which may accumulate from various sources. The last part of the section is devoted to the consideration of the "Schlussfehler," or "error of close" in levelling. This error may arise from mountain attraction, or may exist even without it. We know that at the surface of the spheroidal earth the equipotential surfaces—take any two of them a few hundreds or thousands of feet apart—are not parallel, but the distance between them at any point is inversely proportional to gravity there. If P, Q be two points on the higher of two equipotential surfaces, p, q, their projections on the lower, then levelling from p to Q, if we in imagination take the path, $p\,P, P\,Q$, we have $p\,P$ as the height of Q above p; then continuing the levelling from Q by the path $Q\,q$, $q\,p$, to p, it is clear there will be an error in the close of the levelling of the amount $Q\,q - P\,p$. Practically, of course, this is very small. An error of close of levelling may occur in working over a mountain; the attraction of the mountain deflects the vertical, and too small a height is the result; of course if the hill is symmetrically shaped, the same amount of error is involved on both sides, and there would be no discrepancy in results obtained by levelling over and round or through the hill. But generally the error on the two sides is not the same. In the work before us the case is supposed of levelling being carried over a mountain-chain of uniform triangular section. In the triangular section $A\,B\,C$, C being the ridge and $A\,B$ the base, suppose levelling to be started from A the foot of one slope, along a level surface *through* the mountain, or, which is the same, along a level surface round it, to B, a point on the *same* level-surface as A; then up the slope from B to C, then down the other slope from C to the starting-point A. Then the error of close, or the "Schlussfehler," is a certain multiple of the integral of the difference between the horizontal component of the attraction of the hill at any point as P on the slope and the horizontal component of the attraction at p, which is the projection of P on the level surface $A\,B$, multiplied by the element of horizontal distance, and taken from A to B. So that if we do not misunderstand the writer, the numerical examples of "Schlussfehler," given at p. 290, are very much too large. In fact the before-mentioned multiple of the difference of potential at A and B, when added to the right-hand member of the equation (3) on the page referred to, very nearly cancels that term.

The fifth and last section of the work treats of the influence of small alterations of the spheroid of reference on the reduced triangulation, and of the determination of the elements of that particular spheroid which is most in accord with the results of the triangulation under consideration. The formulæ throughout the work are very neatly developed and the typography is admirable.

A. R. C.

OUR BOOK SHELF

A History of the Birds of Ceylon. By Capt. W. Vincent Legge, R.A. Part I. Imp. 4to. Pp. 1–345. (London: Published by the Author, 1878.)

THE many interesting papers on Ceylonese birds published during the last few years by Capt. Legge in the

Ibis and in the Indian journal *Stray Feathers*, will have prepared his readers for an excellent account of the habits of the birds of that island. The avifauna of Ceylon did not attract the attention of the naturalist to any great extent after the year 1854, when Mr. E. L. Layard published his valuable notes in the *Annals of Natural History*, and brought to light a number of new and interesting forms, until in 1872 Mr. E. W. H. Holdsworth presented to the Zoological Society an excellent memoir, embodying not only all that was known on the subject, but adding greatly to our knowledge from the results of his five years' residence in the island. Ornithologists, however, could hardly have expected from the published accounts that so much would remain for Capt. Legge to do in a field often supposed to be tolerably well exhausted by the labours of the two naturalists mentioned above, and it is impossible to speak in too high terms of the volume which our author has now presented to the public, and which, in our opinion, is one of the best ornithological works ever yet produced. Not only does Capt. Legge excel in his descriptions of the habits of the Ceylonese birds, which he has himself studied in the jungle during his seven years residence in Ceylon, devoting his whole leisure time to the pursuit of ornithology, but on the return of his regiment to England he at once sets to work to complete the scientific history of the birds with whose life-history he is already so well acquainted. The result of the patient labour which he has devoted to both branches of the subject is apparent on every page, and from the peculiar relations of the avifauna of Ceylon this task has been by no means an easy one. Possessing only a limited number of peculiar forms, the birds of Ceylon indicate affinities on the one hand to those of Southern India, and on the other to those of the Malayan Peninsula, while in the higher ranges a decided Himalayan element crops up. Sometimes the differences between the forms of birds inhabiting these regions and Ceylon are found to be of specific importance, but more often the variation does not extend beyond the recognition of a climatic race or sub-species. Not one of these difficult questions is shirked by the author, who diligently compares the subject of every article with the allied forms of surrounding countries, so that many of his descriptions amount to monographic revisions of genera and species of the highest importance to the student of Indian ornithology.

Commenced originally with the idea of providing a handbook to the birds of Ceylon, for the use of the numerous planters and civil servants interested in the study of birds (and Capt. Legge's work shows that these are already a goodly company), this production not only fulfils the author's original purpose, but forms a most valuable addition to the series of similar works, such as Buller's "Birds of New Zealand," Dresser's "Birds of Europe," &c. We regret to see that the great length of the articles, which the conscientious researches of the author have obliged him to write, has already forced him to give to the one part now published no less than 340 pages, which was as much as the entire book was expected to occupy. Involving as this does a serious pecuniary loss, we feel bound to call attention to the fact, as evidencing the self-sacrificing enthusiasm with which Capt. Legge regards his subject, and when we state that the coloured plates, which give illustrations of every peculiar Ceylonese species, are in Mr. Keuleman's very best style, we can add nothing more to recommend the work to the attention of the scientific public. R. BOWDLER SHARPE

Sur la Structure et les Modes de Fécondation des Fleurs. Par L. Errera et G. Gevaert. 1e partie. (Bruxelles, Mayolez, 1879.)

IF the value of a work is to be judged by the extent of the original research to which it has given birth, then surely few have been published possessing so high a value as those of Mr. Darwin. The little volume before us is a direct outcome of attention directed by Darwin's writings to the subject of the fertilisation of flowers. Without pretending to any novel discovery, it gives a clear and succinct *résumé* of our present state of knowledge of the subject, the writers themselves confirming Darwin's observations on some important points. Appended is an interesting morphological study and comparison of two species of *Pentstemon*, *P. hartwegi* and *gentianoides*, of which the authors consider the latter to be a derivative from the former. They regard the natural order Scrophulariaceæ as being the offspring of certain forms belonging to the Solanaceæ.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Deltaic Growth

THERE can be no doubt that there are conditions very favourable to the rapid deposition of sediment brought down by the Irrawaddy, Sittang, and Salween rivers, in the Gulf of Martaban—as a reference to the map will show—but the amount can hardly be as great as that afforded by the following results, which, I anticipate, may be of some interest to your readers.

While proceeding northwards towards Rangoon from Penang, in the s.s. *Fitzpatrick*, Commander Humphries, I observed, shortly after we entered, discoloured or muddy water in the open sea, lat. 15° 15′ N.; the soundings taken were only from fifteen to sixteen fathoms in places where the Admiralty charts showed from thirty-two to thirty-four fathoms, just *double*, or a difference of 100 feet.

The chart was based on surveys originally made by Captains Ross and Crawford, probably thirty years back, and lately issued in an amended form, supposed to have been "corrected to date." PAT. DOYLE

Jordan's Hotel, Rangoon, February 27

Atmospheric Pressure

DANS le No. de la NATURE de 6 courant, p. 420, àpropos d'une appréciation des observations météorologiques de *l'Hydrographic Office* à Pola, on lit le suivant :—

"The amount of this third maximum is very small, and the evidence yet adduced is not sufficient to determine whether it is a real increase of atmospheric pressure or merely an apparent increase due to undetected instrumental errors."

Nous avons remarqué ce maximum secondaire du baromètre dans une recherche sur les variations diurnes de la pression à Lisbonne, avant de savoir qui M. Ricatscheff avait fait une mémoire sur ce sujet, et nous pouvons affirmer que le dit maximum n'est dû à des erreurs de l'instrument :—

La recherche que nous avons fait a été dans une série de 12 années d'observations horaires, déduites du barographe (système photographique) et nous avons constaté le suivant :

1. Pas une seule des 12 courbes de la pression atmospherique, correspondantes aux 12 mois de décembre ou aux 12 mois de janvier, a laissé d'exhiber le dit maximum secondaire entre les 13 et 15 heures.

2. Les valeurs des erreurs probables des pressions moyennes dans les heures (13, 14, 15) sont encore inférieures aux erreurs probables des autres heures du jour.

3. En groupant les pressions horaires des jours sereins et calmes des mois de janvier et de décembre, pendant toute la série de 12 années, le maximum en question est ressorti plus régulier et beaucoup plus developpé que dans les courbes des pressions moyennes de les mêmes mois. Il me semble donc démontré l'existence de ce maximum secondaire, très difficile à expliquer, et que rendra plus obscure l'explication de la double oscillation diurne du baromètre. JOAO CAPELLO

Observatorio do Infante D. Luiz, Lisbonne, 14 mars

On the Pupation of the Nymphalidæ

IN NATURE, vol. xvi. p. 502, I called attention to some observations and experiments of mine on the pupation of several species of *Nymphalidæ* and *Pieridæ*, the results of which were: (1) That, in the species referred to, a connection (larvo-pupal ligament) exists between the larva-skin and the chrysalis which is the sole support of the suspensus chrysalis during the final process of pupation, namely, during the extraction of its tail from the larval skin and fastening the anal hooks in the supporting silk; (2) That this ligament is not confined to the *Suspensi*, but exists also in species of the *Succincti* where it has not the same function to perform; and (3) That nevertheless, in these latter cases, when other support is withdrawn by cutting the girdle before pupation, this ligament is capable, in the majority of cases, of fulfilling the same office as it does in the *Suspensi*.

These views have recently received important confirmation and extension at the hands of Mr. W. H. Edwards, of West Virginia. I refer in the first place to a paper of his in the *Canadian Entomologist* for December, 1878, which is reprinted in the *Entomological Monthly Magazine* for the present month. Here Mr. Edwards describes the ligament as found by him in *Grapta interrogationis* and *Danais archippus*. In the latter it is black. Of the former Mr. Edwards writes: "When I lifted the flap of skin entirely clear of the struggling segments, and cut it off a little below the tail, the bendings and contortions were not interrupted by my interference, nor was the effort to reach the silk in the least abated. Held firm by the stretched ligament, which was in plain view, the body rose, and the tail, which had got well outside the padded skin and was, before complete extrication, bent backward, now bent forward, and by the upward swing, was brought exactly to the silk. Several times as I was lifting, the skin and chrysalis together were dislodged, and fell into my hand. Then by drawing the skin back the ligament was exposed, and it was distinctly seen that it was attached to the chrysalis by the pointed ends of the ridges before mentioned and that there was no other connection between skin and chrysalis." As regards the second and third points mentioned above, Mr. Edwards writes to me as follows:—"I experimented on *Papilio ajax* and *P. asterias*, also following your illustration with Pieris, and was successful in discovering the membrane in both species. *Ajax* has the terminal part of the chrysalis remarkably short, but although I cut the band and let the larva hang, the chrysalis generally succeeded in reaching the button of silk. So with *Asterias*, in which the terminal joint is longer. I have no doubt all the Papilionidæ possess this membrane, and probably the Satyridæ."

We have now the existence of the ligament demonstrated in three species of *Suspensi*, viz., *V. urticæ*, *G. interrogationis*, and *D. archippus;* and in three or four *Succincti*, namely, *Pieris brassicæ*, *Papilio ajax*, and *P. asterias*, and probably also in *A. cardamines;* all of which latter also pupate more or less successfully when artificially converted into suspensi by cutting the loop. To the case of *A. cardamines*, which, when pupating as an artificial suspensus, does not remove the tail of the chrysalis from the pocket of the old larva skin, there is a parallel in *Pyrarga egeria*, and perhaps also in *Epinephele janira*, in regard to the former of which Mr. Newman states ("British Butterflies," p. 85):—"The skin of the caterpillar always remains attached to the anal extremity [of the chrysalis], even after the butterfly has escaped." In reference to *Janira* he writes (*op. cit.*, p. 92):—"Three of my specimens changed to chrysalids," &c.; "in two instances the skin of the caterpillar remained, enveloping the anal extremity, so that the chrysalis could not be suspended; in the third it hung for a time from a blade of grass, the skin still enveloping the anal extremity, but attached by its anal hooks to silken threads on the grass."

Mr. Edwards promises further researches during the coming season, for which he has favourable opportunities in his locality, and I think we may confidently look for interesting and perhaps important results. The question of rank in the diurnal lepidoptera is one that has been much contested (see Wallace's essay on "The Malayan Papilionidæ," &c., "Contributions to the Theory of Natural Selection," p. 133), and will probably be decided differently, according to the standard of perfection set up; but the question of *derivation* is probably more capable of definite solution, and on this point the suspensory ligament seems well calculated to afford important guidance. In the meantime more extensive observations and experiments are wanted, and will no doubt be afforded by those interested in the subject when their attention has been once directed to the matter.

Milford, Letterkenny, March 26 J. A. OSBORNE

Tides at Chepstow

I AM unable to find any certain record of "exceptionally high tides" at Chepstow. They must be of rarer occurrence than is commonly supposed. A very high one is mentioned as having flooded the lowest parts of the town January 29, 1846; and a very low neap tide is noted in a wharfinger's books, on March 19, 1876. Measurements of these cannot be obtained; but the highest known rise of the tide at Chepstow since the erection of the iron passenger bridge in 1816, has not exceeded fifty feet; and probably has never attained such an elevation even with the conjunction of much fresh water inland, and a stiff S.S.W. breeze.

Fair proof of the accuracy of this statement is afforded in the elevation of the railway bridge that spans the Wye about two miles and a half from its mouth. By the requirements of the Admiralty, a clear headway of fifty feet had to be left above the highest known tide. Besides attention to these requirements, the contractors had to provide approaches to the roadway of the bridge, involving, on the Gloucestershire side, a long and deep cutting through limestone rock, and on the Monmouthshire side a lofty embankment. A very large outlay of money depended on the determination of the "highest known tide," for it regulated the point of suspension for the bridge, and the level of the approaches. Yet, as may be seen in the official report appended to these notes, *forty-four feet* only were allowed for the "rise of the tide." The readers of NATURE may refer for a drawing of the bridge, plans, and a detailed report, to the *Illustrated London News* of July 24, 1852, to satisfy themselves.

Through the kindness of Mr. Henry Gillam, and of Messrs. Miller, the lessees of the salmon fisheries in the Severn and in the Wye, I have received measurements of this day's tidal range in both rivers, taken personally by those gentlemen. Amongst well-known points I cite the height at Portskewitt landing stage, New Passage, on the Severn, 39 feet 3 inches; at Chepstow railway bridge, 39 feet; at Chepstow Passenger bridge, about a quarter of a mile higher up the river, 35 feet.

In the geographical part of Knight's Cyclopædia, article "Chepstow," the rise of water at Chepstow Bridge at high tide is given as "fifty feet, being the greatest tidal rise in Europe."

The highest reliable measurements that I have met with for the tidal rise in the Severn are 47 feet 6 inches, marked on the Portskewitt landing-stage.

For accidental phenomena affecting the tidal levels, I refer to the following extract from the Bristol Tide Book:—

By a careful comparison of the differences of the predicted and observed heights of tide at Bristol with the contemporaneous heights of the barometer, Mr. Bunt found that a depression of one inch in the mercurial column is accompanied by an elevation of fourteen inches, nearly, in the height of the tide. Hence, by observing the state of the barometer a few hours before high water, we obtain the following correction of the height predicted in the tide table:—

When the barometer stands at	Inches.		Inches.
	28 6	Add to the predicted height	20
	29 0		14
	29 4		8
	29 8		3
	30 0		0
	30 4	Deduct from the predicted height	6
	30 8		11

Our highest tides for 1879 are marked in the table as occurring Monday, March 10, and Tuesday, April 8. JOHN YEATS

Chepstow, March 24

P.S.—Through the kindness of the four brothers Miller, I am this morning in possession of measurements of yesterday's tide in the Severn and Wye, taken, by the request of the firm, at distant stations. I enclose one, at Tintern Abbey, taken by Wm. Bowen, a regular correspondent of the Meteorological Department.

March 25

Tintern Abbey, near Chepstow, March 24

MR. ALEXANDER MILLER

DEAR SIR,—As requested, I have taken the height of the flow of tide this morning and find it 15 feet 1 inch above present level

of river, which is about 2 feet 9 inches above summer low-water level.

Yours truly,
WILLIAM BOWEN

The pier master (W. Mants) at Clevedon, near Weston-super-Mare, reports that he timed the rise of the tide there on March 10 from two hours flood, and found that it flowed thirty feet perpendicular in two hours and forty-five minutes. On March 24 the tide rose thirty-eight feet at the Clevedon Pier Head.

J. Y.

Ice Pearls

A PHENOMENON of singular beauty presented itself on the morning of March 24. A patch of meadow land, several acres in extent, had been inundated so far as to leave, pretty regularly distributed throughout, stalks of last year's grass projecting several inches above the surface of the pond. During the preceding night the temperature had been below freezing-point, but the wind which rippled its surface prevented the pool from freezing, while it alternately raised and depressed the stalks of grass. The water thus collected by the bending and rising grass-heads formed into large shining beads of ice which lay at the point of junction of the stalk and the pool. The effect was as if each projecting stalk had unfolded a white flower floating on the water, and when a gleam of sunshine smote the surface of the pool, the effect was resplendent.

J. SHAW

Tynron, Dumfriesshire

Unscientific Art

MR. COPPOCK's explanation (NATURE, vol. xix. p. 484) has occurred also to me; but may I be allowed to remind him that in consequence of the internal construction of the marine barometer (the pipette and the contraction in the tube), when it is sloped the mercury rises and falls very slowly. As it naturally rises and falls at a decreasing rate, if the barometer be sloped for a few seconds it takes a comparatively long time for the mercury to resume its original position. I have just sloped one of Adie's marine barometers at 30° from the vertical, and I find it takes more than ten minutes to recover itself. I do not know what may be the actual practice on board ship, but I cannot but think that a plan which renders a barometer useless for ten minutes to another or the same observer must be an unusual one.

JOHN W. BUCK

New Kingswood, Bath, March 28

SCIENCE AND WAR—SIGNALLING BY SUNSHINE

THE use of the heliostat in the field adds one more to the many applications of science made by our soldiers and sailors. Signals by sunshine may be no novelty, but the present Afghan campaign and the Zulu war will henceforth be cited as the first in which the heliostat was employed as an implement of warfare. There can be little question as to its value to the soldier, for it affords at once a ready and far-reaching mode of signalling; but sunshine is an obvious *sine quâ non* to its use. In this country, where the Astronomer-Royal tells us the number of hours of sunshine in the week sometimes does not go beyond the units, the heliostat would furnish but an irregular means of telegraphing, and interruptions in the service would be both frequent and prolonged. But in India, on the other hand, at special seasons, at any rate, sunshine is the rule rather than the exception, and consequently the heliostat furnishes an excellent means of communication which our scientific soldiers have done well to make use of.

Heliostat stations are established at this moment throughout the Khyber Pass, and General Sir Sam. Browne, at Jellalabad, has his orders passed up to him by flashes of light from Peshawur and Ali Musjid. Lord Chelmsford has of late also been furnished with heliostats, in order to provide him with better means of commuication along the Tugela. The plan of working is very simple. The mirror of the heliostat is placed so as to reflect the sun's image to a distant station, and when the instrument has once been set the clockwork arrangement, it need not be said, suffices to maintain the mirror in its proper position. In this way the distant station in question always sees the dazzling ray reflected from the mirror, except when the latter is purposely obscured. The appearance and disappearance of the bright spot or flash constitute the signals. There is no need for any superintendence when once the apparatus has been put in working order, and a trained signalman suffices for the duty. The ordinary Morse alphabet supplies an intelligible code, and no one out of the line of signals can read or understand the message. As a substitute for the dot and dash, which go to make up the ordinary written Morse code, the light is shown for short and long intervals; thus the light shown for a short period followed by a long period signifies A, while B is represented by a long period followed by three short ones; in the case of C, long, short, long, short signals are made in turn; and to form E, the letter most frequently used, the light is permitted to shine for one single short period only.

The intensity of these sunshine signals can scarcely be imagined by any one who has not seen the heliostat in working order, and the distance to which they might be made to travel, could suitable stations be provided, is practically unlimited. But everybody has noticed at one time or another, just before sunset, the light striking vividly against the windows of a house. In this case the burning spot may be seen for miles away, and forms the most striking object in the whole landscape. The heliostat signal is obviously brighter still than this, and the appearance and non-appearance of the light is to be appreciated at ten or twenty miles distant without the aid of telescope or binocular.

Signalling by the aid of a mirror is among the earliest experiments of telegraphy, nor, if we are to believe travellers, is the use of a reflecting surface in this way new in warfare; it is only the heliostat, indeed, which we can claim to have been the first to employ in the field. Several instances are on record of polished metal surfaces being used in this manner by savage nations, and it is but two years ago that the United States forces captured a tribe of Indians to whom the use of the mirror was not unknown. These were the Nez Percés Indians, and, according to latest accounts, they were still confined by the American Government in a camp near Fort Leavensworth, where, however, they were left pretty well to their own devices. According to the *New York Daily Graphic* their chief carried with him a looking-glass, "used to direct military manœuvres in battle, by means of reflected rays of light. Their various significations, however, have never yet been found out by the white man," we are told. These are not likely to have been very complicated. The difficulty, in fact, is not so much in reading light-signals of this kind as to reflect the rays in precisely that direction in which the party for whom they are intended happens to be located. How the chief of the Nez Percés managed to do this with his hand-mirror is rather what "the white man" would like to understand.

One other incident in the history of light-signals deserves to be mentioned. When Admiral Sheriff was stationed at Gibraltar in 1835, he made a series of experiments with a view to employing light as a means of telegraphy. His signals were made by an ordinary toilet looking-glass from his bedroom window, that looked out upon the Mediterranean, and by the aid of this simple apparatus he was enabled to communicate with a friend at Tangiers. His light-signals travelled from "the Rock" right across to the African mainland, a distance of something like twenty miles, and were read and answered without difficulty by his colleague on the opposite shore.

Besides the heliostat, our troops in the field are provided with flags and lamps for signalling by day and night. The flags are made four feet square, so as to be

visible at some distance, and they are white or black, to be used according to circumstances; moreover, the signalmen are furnished both with binoculars and telescopes to enable them to read the signals from remote stations. At night either a bright colza light is made use of, or a spirit flame, into which is blown from to time a mixture of powdered magnesium and resin. A short puff or a long puff constitutes short and long signals, which are displayed, as before, in accordance to the Morse code. Every battalion of infantry and regiment of cavalry in the British army has a proportion of its men trained as signallers, so that these can act at once on taking the field. Their duty is to communicate between outlying pickets and the fighting column, and to do duty where there is no telegraph. For let the field telegraph of an army be ever so well ordered, there is always plenty to do for the army signaller; and he will doubtless find in the heliostat a means of fulfilling these duties with increased efficiency. H. BADEN PRITCHARD

FLOODING THE SAHARA

THE French scheme of turning part of the Algerian Sahara into an inland sea continues to attract considerable attention in France, and scarcely a week passes without some allusion being made to it in the Paris Academy. At a recent sitting M. de Lesseps read a letter from Capt. Roudaire in which the latter gave some details of the results of his sounding of the soil at various points, sands and marls being the beds most commonly met with. At one place, four metres below the surface, plenty of potable water was met with, which will be a great saving in carrying on the work.

At the same sitting MM. Ch. Martens and Ed. Desor presented several considerations against carrying out the plan, their opposition to it being shared by several other French men of science. They have themselves examined part of the ground which it is proposed to put under water, so that their opinions ought to have some weight. While giving every credit to M. Roudaire for the accuracy of the survey which he is carrying out, they, however, point out the difficulty of perfect accuracy, which in this case is all important, in the classic country of mirage, where the surface of the ground is constantly altered and deformed by reflection and refraction. Moreover, they point out that to the south of the projected sea is the Wed-Souf, where are ripened the dates known as Tunis dates, the culture of which is a very special one. The least error in surveying, it is shown, might lead to the destruction of this culture, by allowing the waters of the Mediterranean to penetrate the soil where the date-trees are grown, and thus destroy them. The authors do not attempt to touch the argument that even in historical times part of the Sahara now being surveyed was really a great lake; but they point out that there are proofs that in prehistoric times there must have existed an interior sea, at an epoch when the hydrographical conditions of Europe were very different from what they are now. In 1863, when exploring the region between the oases of Guemar and the south extremity of the Shott Mebrir, they found the gypsum beds of the plateaux ended in regular lines like sedimentary beds, and from the soil they collected the *débris* of shells, truly marine, such as *Buccinum giberrulum*, Lam., and *Balanus miser*, L. Above these shells, in the sand, they found *Cardium edule*, better preserved than they had ever seen it. Thus they found fossils characteristic of salt water, and of those which are a mixture of salt and sweet. The retirement of the waters from the Sahara the authors attribute to the elevation of the land, which is even yet below the level of the Mediterranean, and is to a great extent a network of salt lagoons.

It has been said that the creation of an interior sea, of 13,280 square kilometres, would change the pluviometric condition of the country, and even that of the whole of Algeria. This MM. Martens and Desor regard as a great illusion. Although the laws of the general atmospherical movements are little known, yet it is admitted that the Atlantic is the great reservoir from which come the vapours which are resolved into rain over the European continent. They believe that this is also the case for Africa. The Mediterranean is really only a Gulf of the Atlantic, and they do not believe that an addition of 13,000 kilometres will add anything to its climatic influence. Long calculations have been made as to the quantity of water that would be evaporated by the new sea; but the authors point out that the predominating wind in the region is north, and that if it were rendered either too cold or too moist it would injuriously affect the date-culture carried on in the south. The surroundings of interior seas, like the Caspian and Aral, are steppes noted for their aridity; the shores of the Mediterranean suffer in the same way when, as last year, the rains of the north do not extend to the south. For these reasons MM. Martens and Desor think it would be a mistake to insist on the creation of the interior Saharan Sea.

In a subsequent sitting, however, it should be said, M. Favé endeavoured to show that their fears were groundless, especially with regard to the accuracy of the survey; he thinks that the work in connection with the Suez Canal showed that perfect confidence may be placed in the methods of surveying adopted.

THE LONGEST TUNNEL IN THE WORLD

SCHEMNITZ, the principal mining city of Hungary, has celebrated the opening of the Joseph II. Mining Adit, the deepest gallery of efflux of that place, and the longest subterranean work of this kind in the world.

Its excavation was commenced in the year 1782, during the reign of the Emperor Joseph II., whose name it bears, and has been continued since that time, but with varied energy. The most rapid progress was made within the last five years, so that its completion on September 5th, 1878, was a kind of surprise, and was saluted by guns, which caused a great joy in the city, because it announced a new era for the mining operations of the whole district.

Works of such importance deserve to be installed with solemnity, and a festival was arranged for the purpose on October 20-22, 1878. Prof. Szabó, one of the guests from Budapest, delivered a report to the Royal Hungarian Society of Naturalists, as a representative of that body, and we shall not hesitate to communicate an extract of this.

As the mining operations were progressing in depth, there was at the same time a well regulated system of sinking shafts and driving tunnels employed. The Joseph II. Adit is the eleventh of that kind; it lies 200 metres deeper than the Francis Adit, which was until now the principal gallery of efflux for the mines of Schemnitz. This was excavated between the years 1494 and 1637 to a length of 1,968 metres; but a greater extension was given to it by continuing the works from 1747 till 1765. After this period the mines of Schemnitz proved to be so lucrative, that the idea of undertaking some greater work for securing the future prosperity of the mines was conceived, and so the plan was fixed of driving a tunnel at the deepest possible level, which could convey the waters to the valley of Gran, the lowest point indeed which could be obtained within a practicable distance.

They commenced boring the tunnel west from Schemnitz, near the village of Voznitz, on the left bank of the Gran. The height of it is three metres, the width 1·6 metres. About the lower third is destined to convey off the waters, while the upper two-thirds are separated from this by a platform, and adapted for transporting the ores.

According to the original plan it could have been finished in thirty years at the cost of 1,215,000 florins

The cost per metre would thus have been 87fl. 86kr., and indeed such was the case in the first eleven years; but after the French revolution the value of money was greatly changed, and the prices became so high, that in the next thirty-three years very little was done, the yearly progress not being more than 61·4 metres, and the cost per metre 371fl. 52kr.

From 1826 the works were carried on with greater energy at the cost of 260fl. 40kr. per metre till 1835, when the progress again became slow, and remained so for the next eighteen years, only seventy-two metres being worked yearly, at the cost of 313fl. 45kr. per metre.

But after the middle of the present century the sense of the decline of the mines from their former state of prosperity was so prevalent, that the director of the district, M. Russegger, well known on account of his scientific travels in Europe, Asia, and Africa, proposed that they should again devote greater energy to the works in question, as most of the mines were under water, and the raising of this by machines caused an outlay which the mines were not able to bear. For the next twelve years the yearly progress was 293·2 metres, at the cost of 237fl. 63kr. per metre. During the next five years after Russegger's time only 141·1 metres were worked out yearly.

The Hungarian government has through the last ten years again developed greater activity in this work, and the parliament has at its request granted the yearly sum of 100,000 florins for the purpose.

In the year 1874 there were still 2,326 metres to be worked out, which would under ordinary circumstances have been a task of eleven years; but in 1873 experiments were made in boring with machines, which method was tried for the first time in the Mont Cenis tunnel, with surprising success, then in the St. Gothard railway tunnel, and lastly in the "Sutro" gallery (Nevada). After many trials they succeeded in finding out the most convenient arrangement, and the whole work was done in three and a half years. With this method the entire tunnel could have been finished in 27 years.

The length of the	Mont Cenis tunnel is ...	12,233 metres.
" "	St. Gothard is	14,920 metres.
" "	Sutro gallery of mines is	6,147 metres.
" "	Joseph II. Adit is ...	16,538 metres.

The total cost amounts to 4,599,000 florins.

The importance of this tunnel is very great, firstly as regards *geology*. The geological and orographical literature of that country is very old; Schemnitz has been repeatedly visited by distinguished men of science from all quarters of Europe, but the difficulties and complications of its geological structure are so great that there is still much to be done. One of the greatest obstacles in the way of investigation is that the surface is very seldom well exposed; dense forests and products of decomposition of the rocks cover many of the slopes. The tunnel furnishes a section more than ten miles in length, and gives not only valuable information as to the downward prolongation of the lodes known in the upper levels, but some new ones have been traversed, and the entire series of rocks, with their mutual limits as well as modifications and occasional transitions is to be seen without interruption.

It is important secondly as regards *mining*. A new region has been made accessible, and the master-lodes can now be worked to their full extent, while in past years all activity was absorbed by the unlucrative Adit itself. Now the works again promise a long continuance. All the machines used in raising the waters are put away, and thence an outlay of more than 100,000 florins is saved yearly.

The last and not least advantage consists in enriching the miner with new means of working. The application of mechanical boring may be considered as forming for him a new era, just as did the introduction of gunpowder; he will now much more easily undertake the driving of adit-levels, whenever this is feasible, and so, it is to be hoped, that the neighbouring old mining cities will successively have their galleries of efflux too, which is the essential condition of the restoration of their prosperity in mining.

OUR ASTRONOMICAL COLUMN

BRORSEN'S COMET.—From an observation at Kremsmunster by Prof. Strasser on March 14, it appears that this comet has passed its perihelion several hours later than the time calculated by Dr. Schulze of Dobeln, the corrections to the ephemeris on that date being – 31s. in right ascension and – 3′·5 in declination; yet observations at Rome on February 17 and at Arcetri, Florence, on March 10, give different corrections. For the present, as the ephemeris is sufficiently near for finding the comet, the following positions and distances may be extracted from it:—

oh. Berlin M.T.	Right Ascension. h. m. s.	Declination North. ° ′	Log. distance from Earth.	Log. distance from Sun.
April 4	2 55 29	25 44	9·9789	9·7767
„ 5	2 59 16	26 59		
„ 6	3 3 5	28 15	9·9663	9·7824
„ 7	3 6 58	29 32		
„ 8	3 10 55	30 49	9·9538	9·7898
„ 9	3 14 56	32 7		
„ 10	3 19 2	33 25	9·9415	9·7986
„ 11	3 23 13	34 44		
„ 12	3 27 31	36 2	9·9295	9·8086
„ 13	3 31 58	37 22		
„ 14	3 36 32	38 41	9·9179	9·8196

On March 10 Dr. Tempel estimated the comet brighter than a star of the eighth magnitude, the theoretical intensity of light at the time being 1·18; the maximum value attained this year is 3·33 on April 14, and during the latter half of April and the whole of May the comet will no doubt be well observed; from April 14 to June 10 it will be constantly above the horizon of Greenwich. At its next return in 1884, its apparent track in the heavens is not likely to be a favourable one for observation, and as long a course of observation as is practicable at the present appearance will be desirable for carrying forward the elements of the orbit to 1890.

MIRA CETI.—In 1879 and 1880 the minima of this variable occur at times when the star will be too near the sun to be observable, but the maxima, according to Argelander's formula of sines, take place under very favourable circumstances for accurate determination, in 1879 on September 11, and in 1880 on August 11. From the observations of Dr. Julius Schmidt at Athens, it appears that this formula, which had given the epochs of maximum in 1876 and 1877 (two) earlier than the observed times by 17·7, 16·8, and 19·4 days respectively, was only in error in this direction four days in 1878.

Among variable stars now favourably situated for observation, may be mentioned Lalande 23617 and 23726, the former has been rated from 6m. to 9m., and the latter from 5m. to 8m. Also Lalande 26211, which has been noted as high as 6m. and as low as 9m.; the variation, however, appears less decided in this case, though Bessel estimated the star 8m.; Lalande's 9m. may perhaps be considered a misprint, as there are known to be similar cases in the "Histoire Céleste."

THE MINOR PLANET HILDA.—A new determination of the orbit of this, the most distant member of the minor planet group, by Kühnert, of Vienna, assigns a period of revolution of 2,861 days, or 7·832 years, and an aphelion distance of 4·52; at this point of its orbit the planet is distant from the orbit of Jupiter only 0·85, so that considerable perturbations are possible. The search for Hilda at the present opposition, so far as we know, has been unsuccessful.

EXPERIMENTAL RESEARCHES ON THE REPULSION RESULTING FROM RADIATION

IN previous papers[1] I have described my earlier experiments with the radiometer, and I then showed that the movement of this instrument was due to the presence of residual gas. I have since examined the repulsion exerted by a standard flame shining on pith and mica disks, coated with various powders, chemical precipitates, &c., and suspended *in vacuo* in a torsion apparatus, and I propose in this and succeeding papers to give an account of these experiments, and of the concluding researches on the repulsion resulting from radiation.

The apparatus I used to get quantitative measurements of the repulsion produced by radiation on various kinds of disks, and coated with different substances, is similar to one I have already described, but in order that the experiments may be better understood, it is shown in Fig. 1. I append the following description:—

ab is a horizontal glass tube containing the beam,

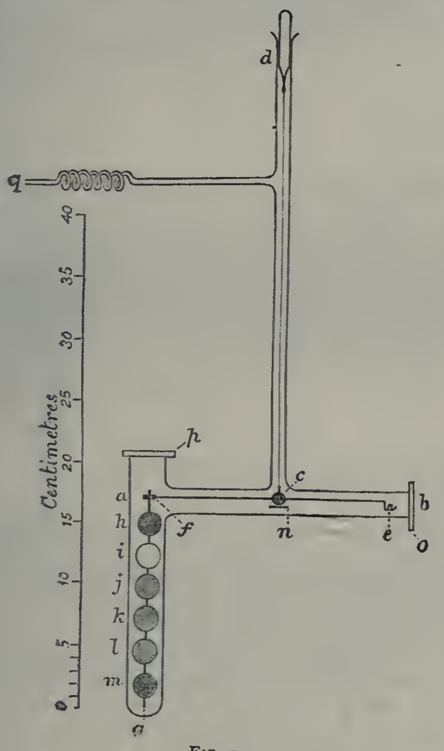


FIG. 1.

which, in this case, is made of straw, so as to secure lightness with absence of flexure under the comparatively heavy weights it sometimes has to bear; *cd* is a fine torsion fibre drawn from flint glass, to which the beam is suspended; it is cemented at *d* to a well-ground stopper, so as to admit of adjustment. When in position, cement, made by fusing together eight parts by weight of resin, and three parts of bees' wax, is run round the stopper. At *c*, the point of junction between the torsion-fibre and the straw beam, is a silvered glass mirror. At the end *e* of the beam, is a small pan to hold the weights counterpoising the disks, which are suspended to the other end. A flat stirrup of aluminium at *f* fits stiffly on the straw beam, and carries a flat glass fibre, *f g*, cemented to it so as to allow of no play, the straw beam, the aluminium hook, and the glass fibre being perfectly rigid. The experimental disks are fixed on the glass fibre by means of a touch of cement at the back. The vertical tube is arranged to hold six disks, the top one, *h*, being always the same standard lamp-blacked pith; the others, *i*, *j*, *k*, *l*, and *m*, being changed each time. A small magnet, *n*, attached to the central mirror, and controlled by a bar-magnet outside, gives the power of bringing the beam to zero, should it happen to get out of adjustment, without having to melt the cement and alter the angle of the torsion fibre by turning the stopper *d*. Plate glass caps at *o* and *p*, cemented to the ground edges of the tubes, give access to the interior; *o* allows the counterpoises to be adjusted in the pan, and *p* allows the aluminium stirrup to be unhooked, and the whole of the disks to be lifted out together. The apparatus is connected to the mercury pump by the arm and spiral *q*. The weights and dimensions of the various parts of the apparatus are as follows:—

Weight of straw beam, mirror, magnetic needle, aluminium stirrup, and flat glass fibre, &c.	7·25 grains.
Average weight of six plain mica disks ...	2·40 ,,
Average weight of six plain pith disks ...	0·59 ,,
Length of straw beam, from centre of counterpoising pan to centre of disks	17·0 centimetres.
Length of arm from centre of suspension to centre of pan	7·6 ,,
Length of arm from centre of suspension to centre of disks...	9·4 ,,
Glass torsion fibre—Length	23·0 ,,
,, ,, Thickness	0·0013 inch.
Torsion with a glass weight hanging from it	½ oscillation in 15·75 seconds.

Fig. 2 shows the apparatus fitted up for experimentation. The disks are shown in position at *a*; a brick wall, *b c*, has holes pierced through it in two places, as shown, one hole, *d*, being opposite the centre mirror, and the other, *e*, opposite the disks. The aperture *d* is lined with card, lampblacked inside, and the interstices between it and the bricks are well plugged with cotton wool. A water cell at *d* prevents radiant heat from the lamp getting to the apparatus. Through the hole *e* pass six card tubes, lampblacked internally, 20 millims. diameter, and 23 centims. long. The tubes are firmly cemented to the wall, so that each shall be exactly central with its corresponding disk, and the outer end of each is closed with a cork. The space between the tubes and wall is well stuffed with cotton wool. The apparatus, being once fixed in position, is surrounded on all sides, as well as above and below, with cotton wool. Outside this is a row of glass bottles filled with water, and in front of all is a wooden screen. When protected in this manner, the inside of the apparatus is found to be free from disturbances caused by changes of temperature. When the disks have to be changed, air having been let in through the pump, access is easily obtained to the glass cap *p* (Fig. 1), and the cement being softened by heat, and the cap removed, the disks are lifted out together by seizing the aluminium stirrup with forceps. A fresh set of disks being introduced, the apparatus is again packed up and re-exhausted.

A lamp at *f* throws a narrow beam of light on the mirror of the apparatus, through the aperture *d*. The ray is reflected to the scale *g*, where its deflection from zero shows the angular movement of the torsion beam when one of the disks is repelled by radiation. The scale is 1½ metre from the reflecting mirror.

A standard candle (the kind employed in gas photometry, and defined by Act of Parliament as a "sperm candle of six to the pound, burning at the rate of 120 grs. per hour") is supported on a heavy stand, *h*, and can be raised or lowered by means of the sliding piece, *i*. Another sliding piece, *j*, carries a pointed wire projecting from it. The upright rod of the stand is graduated and numbered, so that when the sliding piece *j* is at mark 1,

[1] NATURE, vol. xv. pp. 224, 299.

the point of the wire is on the prolongation of the axis of tube and disk No. 1, and so on. Then, by sliding the candle up till the most luminous part of the flame is level with the point of the wire, it is known that the light will shine full on the disk under experiment. A half cylinder, *k*, covered with black velvet, protects the candle from draughts. The candle-stand *h* slides along a straight edge, *lm*, screwed to the bench, so graduated that by bringing a mark on the sliding stand to one of the divisions, it indicates the number of millimetres separating the surface of the experimental disk from the centre of the candle flame. The experimental powders are laid on one surface of mica or pith disks as a water paint, no cement being used to promote adhesion. Disks of mica or thin metal were punched, while other materials were cut or filed into the shape of disks 17·25 mm. in diameter.

The exhaustion, which had to be effected after each change of the experimental disks, was carefully brought to the same degree both by actual measurement on a McLeod gauge, and by getting the same repulsion on the standard black disk. In this way all the different results were fairly comparable one with the other. The presence of aqueous vapour was specially guarded against by means of tubes containing phosphoric anhydride.

To show the effect of residual gas intending to equalise the amount of repulsion on variously coloured surfaces, I devised an experiment with pith disks, one being lampblacked and the other retaining its natural white surface, the standard candle being at the same distance in each case. When the exhaustion is good enough to cause a fair repulsion, the ratio between the amplitude of swing when the black is exposed, and that when the white is exposed, is as 100 : 55·5 ; at a little higher exhaustion the ratio is, Black : White : : 100 : 42·5 ; at a still better exhaustion the ratio is, Black : White : : 100 : 35. The results of the quantitative examination of the repulsion resulting from radiation when falling on about 100 different substances I have arranged in fourteen tables, for details of which I must refer to the Bakerian Lecture for 1878. The repulsion is measured, first when no screen is interposed, and secondly, when a cell of water is inserted in the path of the rays. In comparing the two results it must be remembered that the actual amount of repulsion on the standard lampblacked disk, when the water screen is interposed, is only one-twelfth of the amount obtained when no screen is in the way, the distance of candle and

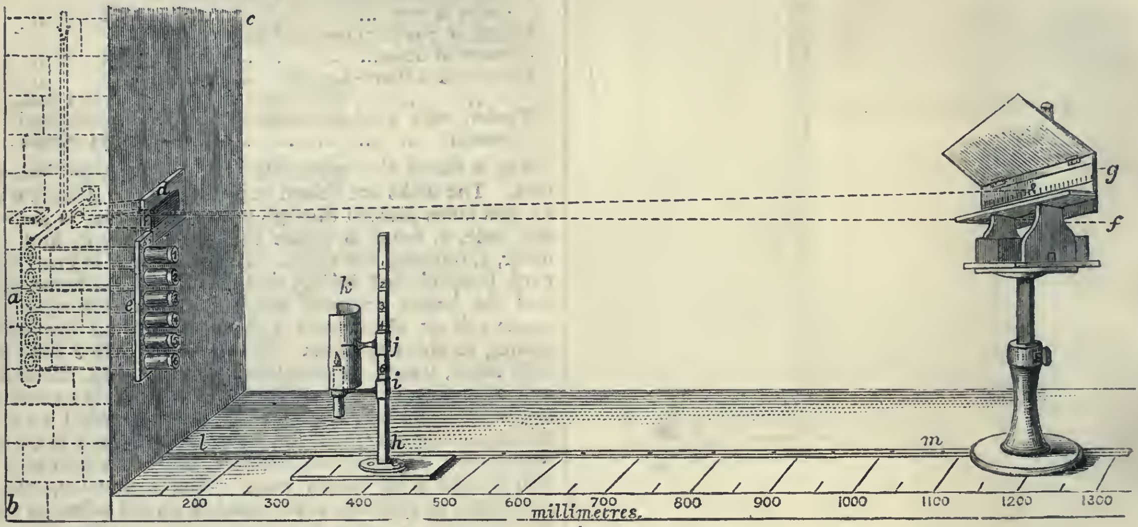


FIG. 2.

other things being equal. In order therefore to compare one with the other the result behind water must be divided by 12.

TABLE I.—*Results of the Examination of Black Powders*

Compared with lampblack = 100 these have an average value of 92·2, which becomes 99·1 by the interposition of water.

TABLE II.—*White Powders*

These have an average value of 33·5, which is reduced to 8·3 behind water. The powerful absorption for the invisible heat rays which white powders exercise is somewhat remarkable. Assuming that the ultra red rays from a candle are almost entirely cut off by a water screen, the comparatively strong action (33·5) produced by the naked flame must be mainly due to the absorption of the invisible heat-rays ; and when these are cut off by water, the action is diminished nearly fifty times. With black powders the water only diminishes the action about eleven times.

TABLE III.—*Red Powders*

Amongst these precipitated selenium is noteworthy. To the naked flame its value is 35·8, but when a water screen is interposed, the action becomes 69·5, in comparison with standard lampblack = 100. Omitting selenium, the mean action on red powders without a water screen is 32·2, and with a water screen, 24·9.

TABLE IV.—*Brown Powders*

Amongst these, peroxide of thallium is remarkable as being repelled under the influence of radiation to a greater extent than any other body hitherto examined, its value being 121·7, in comparison to lampblack = 100. Brown powders behave most like black, the averages of the columns *without* and *with* a water screen being 92·7 and 94·5.

TABLE V.—*Yellow Powders*

Among these, anhydrous tungstic acid resembles scarlet selenium in its anomalous action, the figures being, *without* water, 50·8, and *with* water, 72·2. The averages of the other yellow powders are 35·7, and behind water, 13·8.

TABLE VI.—*Green Powders*

These show some discrepancies, which will be referred to farther on.

TABLE VII.—*Blue Powders*

These show a much stronger proportionate action behind a water screen than with no screen, the averages being 55·8 and 65·2.

TABLE VIII.—*Dyes and Colouring Matters of Organic Origin*

Among these may be noticed saffranin, and a product of the decomposition of chlorophyll, which show an increased ratio or action when the heat-rays are cut off by water. Leaving out these, the mean actions of the other substances are, with no screen, 44·5, with a water-screen interposed, 28·1.

TABLE IX.—*Metals prepared in different Ways and coated with Lampblack, Mica, &c.*

Curious results are shown with iron and with gold, the former metal chiefly absorbing the invisible heat rays, whilst the latter metal is principally acted on by the luminous rays.

TABLES X. AND XA.—*Various Silver Salts*

The chloride, bromide, and iodide of silver in their different states were exposed to the standard candle after being submitted to the action of magnesium light, sunlight, and daylight. The results show how readily a change in the state of the surface is detected by an increased amount of repulsion under the influence of radiation.

TABLE XI.—*Selenium—Crystalline and Vitreous*

The former is in the state most sensitive to light action. With the crystalline disk results have been obtained which seem to show that the impact of light on its surface produces a superficial disturbance there and in the adjacent gaseous molecules, which takes some time to subside. This is connected with the change in electric conducting power of crystalline selenium—a change which, when the element is transferred from light to darkness, also takes some time to subside.

TABLE XII.—*Miscellaneous Substances—Pith, Mica, Charcoal, Glass*

The complicated nature of these actions was well shown in the results I obtained with three pith disks, the first being plain white, the second lampblacked on the front, and the third lamblacked on the back. The first was repelled with a power of 17·7, the second, which was the standard, with a power of 100, whilst the third was not moved at all. The repulsion exerted on the white surface must have been the same in each case, but the pressure *behind* the pith caused a radiation of heat from the back surface, which produced molecular pressure just sufficient to neutralise the pressure in front.

To show that physical condition has more effect in causing repulsion than chemical composition, I experimented with various kinds of charcoal. I found that the repulsion suffered by cocoa-nut shell charcoal is much less than that of white pith, being only 11·6 against 17·7. At the same time a radiometer made of cocoa-nut shell charcoal, lampblacked on one side, was only moderately sensitive, instead of being superior to one made of pith lampblacked on one side. The low figure shown by the charcoal was caused by its density enabling it to conduct heat from one surface to the other. Molecular pressure is therefore generated on both the back and front surfaces, and the figure I obtained is simply the difference between the two opposing actions.

I used other screens, besides water, to filter the radiation of the candle before it fell on the disk. I, however, preferred water. It is almost perfectly opaque to the invisible heat rays, and therefore its employment allows easy discrimination between actions due to heat and to heat and light combined; secondly, it is colourless, and having no selective action on any visible ray of light, it can be used in conjunction with any coloured powder without complicating the results. Alum acts in a similar manner to water; coloured solutions act as water with a super-added action due to their colour. Very thick plates of glass have less action on the invisible heat rays than a thin layer of water. Sulphate of copper, in a solution so weak as to appear only slightly green, has a very strong action when artificial light is used, as it cuts off the lowest visible red rays as well as the ultra red.

I found that the substances I had experimented on might be divided into two classes.

1. *Negative*, those in which the repulsion behind water is *greater* in proportion to the standard than when no screen is present.

2. *Positive*, those in which the repulsion in proportion to the standard is *less* behind water than when no screen is present.

Amongst Class 1 may be mentioned copper tungstate, saffranin, scarlet selenium, and copper oxalate; these are more affected by light than by invisible heat. Amongst Class 2 I may mention pale green chromic oxide, persulphocyanogen, hydrated zinc oxide, barium sulphate, and calcium carbonate; these substances are more acted on by the ultra-red rays than by the luminous rays. To render these differences of action more comparable, I divided the averages obtained by the water

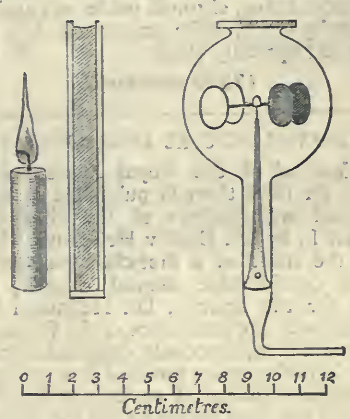


FIG. 3.

screen by twelve. Uniting the two classes together, the figures then became as follows:—

	No screen.	Water screen interposed (5 millims.)
Lampblack (*standard disk*) ...	100·0 ...	8·3
Chromic oxide, pale green ...	71·5 ...	1·7
Copper tungstate	51·2 ...	6·4
Persulpho-cyanogen	43·9 ...	1·0
Saffranin	41·0 ...	4·3
Hydrated zinc oxide	40·5 ...	1·2
Barium sulphate...	37·4 ...	0·3
Selenium, precipitated	35·8 ...	5·8
Copper oxalate	30·1 ...	3·3
Calcium carbonate	28·5 ...	0·3

An examination of this table shows that the results can be proved by balancing one powder against another in a radiometer. A bulb was therefore blown on the end of a wide tube, as shown at Fig. 3. The top of the bulb was opened and turned over to form a lip; this was ground smooth and polished, so as to be readily closed by cementing on it a piece of plate glass. A glass stem supports a fine needle in the centre of the bulb, and on this rests a glass cap, to which is attached four radial arms of aluminium. To these arms disks of mica or pith can be fastened so as to form the movable fly of a radiometer. The disks can be changed by uncementing the glass top, and lifting the fly

out with tweezers. The lower part of the tube is drawn out for connection with the mercury pump. The powders used for experiment were carefully painted on the opposite sides of pith or mica disks, only water or alcohol being used.

Disks coated on alternate sides with chromic oxide and precipitated selenium move in one direction to the naked flame of a candle, and in the other direction when a water screen is interposed. With saffranin and hydrated zinc oxide the instrument does not move at all when exposed to the naked flame, but revolves when a water screen is interposed. With thallic oxide and Magnus's green platinum salt, the instrument moves strongly when no screen is interposed, but is stopped with a water screen. These results are all in conformity with the figures.

A pith radiometer coated with precipitated selenium and chromic oxide was exposed to the radiation from a colourless gas flame from a Bunsen burner, coloured intensely green by thallium. To the eye, by this light, the chromic oxide looked nearly white, and the selenium black. The rotation due to the repulsion of the chromic oxide was, however, apparently as strong as when the non-luminous flame was used. This experiment proves that certain substances have an opposite absorptive action on rays of dark heat to what they have on light, and that an optically white body may be thermically black, and *vice versâ*. In this case, for instance, chromic oxide was optically green, and thermically black, while scarlet selenium was thermically white and optically black. W. CROOKES

(*To be continued*)

METEOROLOGICAL ORGANISATIONS

IN the *Journal* of the Royal Statistical Bureau of Prussia for 1878, there is published a report on the meteorological organisations of the chief countries of Europe, Part I., by Dr. Gustav Hellmann, who is rapidly coming to the front as a first-class meteorologist. In addition to considerable mental capacity and much enthusiasm for the science, Dr. Hellmann has, at the instance, and with the assistance of the Prussian Minister of Public Instruction, especially qualified himself for the work by undertaking tours more or less prolonged, in the countries the meteorological systems of which he reports on. These in the Part before us are the various systems in France, Great Britain, Belgium, and Holland. With the aid of a renewed grant he sets out on a second tour, this time through northern Europe, especially Russia, for the purpose of presenting similar reports on the meteorology of these countries. This action on the part of the Prussian Government has been taken, in view of a contemplated reorganisation of its meteorological system, so that when the time comes, the system may be established, not at haphazard, but on a sure basis, founded on the fullest knowledge of the requirements of the science, and on the best means to be adopted for its healthy development.

The Weather Telegraph systems of France, Great Britain, Belgium, and Holland, are fully detailed, very special attention being given to the weather warnings of France, carried out for the benefit of agriculture and horticulture. This system of weather warnings, which is so peculiarly adapted to the wants of Germany, was, as our readers are aware, the last gift to meteorology of the great Leverrier, to whom, in its practical bearings, meteorology stands so deeply indebted.

As regards France, meteorology would appear to have a most hopeful future before it, as evidenced by the mental activity brought to bear on the science, the fertility of resource in devising new methods and subjects of observation, the breadth of view shown in making the study of weather and climate subserve great public interests, and withal by the pecuniary assistance liberally and heartily given by Government and other bodies intrusted with the public funds, to the observatories, societies, and associations in various parts of France that are doing its meteorological work. Among the more special work France is doing may be noticed the application of the electric thermometer to the observation of the temperature of the air at great heights and of the soil at great depths; the establishment of several stations in Paris for the investigation of the chemistry and micrography of the atmosphere in their relations to the health of the city; and the establishment of high-level stations, which has been done largely through assistance given from the public purse.

We note with the liveliest satisfaction the great increase of meteorological stations over these four countries, the introduction of instruments for continuous observations in regions where they were much required, and a more adequate observation of the rainfall, particularly in the British Isles, where about 2,100 rain-gauges are at work, and in the river-basins of France, where the rainfall is noted at 1,111 stations.

Forcible attention is directed to the fact that in some cases the reduction of the observations and publication of the results are not carried out, or carried out very imperfectly, so that no little difficulty is experienced when conducting climatological inquiries, in obtaining the data from considerable portions of Western Europe. This defect ought to be rectified without delay.

Reference is made to international stations, or stations at which observations are made for purposes of international meteorology. But on looking at the diverse hours of observation adopted by the different European systems, it is evident that the attempt recently made to found an international meteorology must be regarded as a failure, since the prime and most elementary condition of uniformity as regards hours of observation has been neglected, the just views on this vital point propounded by Humboldt and the meteorologists of his time being at present, if appreciated, entirely set aside.

MYCOLOGY[1]

IT is perhaps not generally known how very numerous are the specimens comprised under the branch Mycology. The mycological herbarium which is in the course of transmission to Kew consists of at least 10,000 species, of which 7,500, comprising the Hymenomycetes and Ascomycetes, have already been forwarded. But not only are many species very beautiful in form and colour, but the subject is one of great interest both in a physiological and economical point of view, apart from mere distinction of species and nomenclature, and, therefore, while especial journals are devoted to entomology, malacology, algology, and other branches of natural history, it is quite right that we should have one devoted to fungi. M. Roumeguère ought, however, to have mentioned that England already possesses one in Grevillea quite equal to the French journal, which has appeared with great regularity ever since 1872, and is monthly instead of trimestrial, of the existence of which he could scarcely be ignorant, as it is referred to more than once in the number before us.

The Journal before us commences with a paper on the much-vexed question of the real nature of lichens, in which the author is altogether opposed to Schwendener's theory of their parasitic growth on Algæ. There are two points which ought to be noticed: that the growth of Gonidia from Hyphæ was observed by Mr. Berkeley, as recorded in the "Introduction to Cryptogamic Botany,"[2] while the stem of the curious

1 "Revue Mycologique: Recueil trimestriel consacré à l'Étude des Champignons." Par M. C. Roumeguère. (Paris: J. B. Baillière et Fils.)

2 "Int. Crypt. Bot.," p. 373, Fig. 78*a*.

3 "Int. Crypt. Bot.," p. 341, Fig. 76.

genus, *Emericella*,[3] a Lycoperdoid, is composed of bodies which closely resemble *Palmella botryoides*, Grev. The fungus was found on the leaves of *Euphorbia neriifolia*, in the hot country of Secunderabad, a very unlikely locality for a *Palmella*. Though the observations in the paper are not absolutely convincing, they are highly worthy of consideration, and may induce the advocates of the theory of Schwendener to reconsider the matter and make fresh experiments.

The other papers in the number do not require any especial consideration, though it may at once be allowed that they contain much matter of interest, scarcely, however, so much as regards physiology as might have been wished, for that, after all, is the point which is most likely to engage general interest. It is very unfortunate that the Tulasnes, after doing so much for mycology, have of late retired almost entirely from their former line of study. It is impossible to give too much credit to the result of their researches, or the admirable drawings with which they are abundantly illustrated. There are, however, other labourers in the field who are carrying on their work, amongst whom it is impossible to neglect De Bary, even when such observations as those on the development of *Ascobolus* may require confirmation. They are too curious and important to be set aside without fresh examination, and whatever views may prevail as to the nature of lichens, it is so certain that they are essentially fungals, that the origin of the fructification must be the same, or at least analogous. Much remains to be done as to the impregnation of the English fungi, for Mr. W. Smith's ingenious paper on the fructification of *Agaricus lacrymabundus* cannot at present be received as more than a mere speculation. De Bary's observations on the supposed occurrence of asci in *Agaricus melleus* are confessedly due to the presence of a parasitic Hypomyces. M. Cornu, however, is attending to this as to many other objects of interest; while van Tieghem is adding daily to our knowledge of the different kinds of *Mucedines*, and Drs. Cunningham and Lewis are working effectively in India. It is to be regretted that Mr. Renny has never published the numerous new genera he has figured in this department, which vie, for beauty and singularity, with anything which has been recorded by van Tieghem. Mr. Abbay has lately made some curious observations on the germination of the spores of *Hemileia*, which is so destructive in the coffee plantations of Ceylon. He does not, however, seem to be aware, that Mr. Thwaites had already observed that the species in germinating always produce a Penicillium, though Mr. Abbay has much extended his observations.

Brefeld and Rees have made many valuable observations with respect to the production of asci with sporidia in yeast. Rees, however, states that under the most advantageous conditions he has never been able to induce the globules to send out threads of mycelium. This was, however, done by Mr. Hoffman, of Margate, the account of whose observations, in company with the author of the present notice, are recorded in the article "Yeast," in the "Cyclopædia of Agriculture," the same manipulation showing that the Sclerotium of onions is a condition of a minute Mucor. Their success depended upon having the disk of water in which a very limited number of yeast globules were inclosed, being surrounded in a sealed cell with an atmosphere of air.

These observations should not be closed without a notice of Woronin's very complete observations on the disease commonly known as the club in cabbages. He not only succeeded in discovering the fungus to which it is due, but was enabled to complete his experiments by its actual cultivation. The figures which accompany his memoir are beyond all praise. We may expect more from his hand on these obscure productions. The genus *Protomyces* will probably afford some unexpected results, and we may yet hope for something more satisfactory on the nature of the bodies which are so common on the roots of Leguminosæ.

Finally, M. Cornu's researches in the Saprolegniæ have increased our knowledge of these curious organisms, most authorities being now of opinion that they are aquatic fungi; while many other valuable communications, of almost equal interest, are necessarily omitted.

M. J. BERKELEY

GEOGRAPHICAL NOTES

THE April number of the Geographical Society's new periodical contains Mr. Comber's paper on his explorations inland from Mount Cameroons, and his journey through Congo to Makuta, the late Capt. Patterson's notes on the Bamangwate country, South Africa, and Gen. Kaye's paper on the mountain passes leading to the Valley of Bamian, all of which were read at recent meetings. These are followed by some remarks on the colouring of maps, by Prof. Cayley, the Council's memorial respecting professorships of geography, &c. The geographical notes contain several items of interest. In one are some useful explanatory remarks respecting Major Pinto's reported "solution of the Cubango mystery," while another fixes the locality of Lake Chaia (not marked on any of our maps) near which Lieut. Wauthier died and Mr. Penrose was murdered. There are also same particulars respecting Japanese colonisation in the Island of Yesso, and Mr. Alex. Forrest's expedition to explore and survey the unknown tract of country between the De Grey and Victoria rivers in North-western Australia. The present number is illustrated by two maps, the one of Mount Cameroons and the neighbouring country, from a drawing by Mr. Comber, and the other of the Bamangwate country, also from new material.

THE *Globe* gives the following as the official programme that has been drawn up for the "Imperial Expedition" to Central Asia, under the command of the Grand Duke Nicholas Constantinovitch. The staff of the expedition will be an engineer from the Ministry of Railways, an officer of the Baltic fleet, a surveyor, a naturalist, an archæologist, a geologist, a painter, a correspondent, and a topographer. The aim of the expedition is to select the route of the Central Asian Railway, to examine the navigability of the Oxus, and to decide the possibility of diverting it into the Caspian. The route will be from the River Ural to Karasugai, on the Syr Daria, thence *viâ* Tashkend and Samarcand to the Oxus at Kunduz (Afghanistan); afterwards along the river to Khiva, and across the Kara Kum to Krasnovodsk. The work of the expedition will be: 1. To collect information as to the cost of the railway, the ability to obtain materials for its construction, whether fuel exists on the route, and the amount of labour obtainable. 2. To investigate the speed of the Oxus, the height of its banks, the population of the nearest towns and settlements, and the existing commerce on the river. 3. To examine the Khiva oasis, the floods of Sari Kamish, and the ancient bed of the Oxus, commonly known as the Uzboc. 4. To carry out astronomical observations all the way along the route, to make military plans, to sketch the features of the country, to collect objects of mineralogical, zoological, geological, and archæological interest, and to keep a journal of daily events. Finally, in collecting information respecting the ancient course of the Oxus, to decide whether it can be diverted afresh into the Caspian without detriment to the Khivan oasis. It seems possible, however, that in case of certain Eastern complications the expedition may develop into a military one against Merv.

FURTHER news has been received from Dr. Crevaux, the explorer of French Guiana. He returned to Guiana about the middle of last year for the purpose of exploring

the Oyapok, the second river of the colony, the basin of which was comparatively unknown. On August 21 Dr. Crevaux was at the mouth of the river. Crossing a second time the Tumuc-Humac range, which separates the waters of the Oyapok and Maroni from those of the Amazon, he descended the Kou, an affluent of the Yari, the course of which was unknown. Arrived at the Yari, which on a former occasion he had ascended only as far as Yacouman, he followed its course to its sources, which he reached on October 24 last, after a journey of about 170 miles on that river. On that date he wrote a note in pencil to Paris, announcing the result of that part of his journey, but predicting for the remainder of his exploration considerable difficulties, which, however, he has succeeded in surmounting. A short letter just received by *La Nature* intimates the return of Dr. Crevaux to Sainte Marie de Belem on January 9 last, and contains a topographical sketch of the region explored. After having crossed the secondary chain which separates on the west the Yari from the Parou, another almost unknown affluent of the Amazon, Dr. Crevaux completely explored this considerable watercourse, and afterwards, descending the river, he re-explored the lower course of the Yari.

A TELEGRAM addressed by Gordon Pasha from Abu-jerad, on the White Nile, to the president of the Italian Geographical Society, announces that Signor Matteucci, the leader of the Italian scientific expedition, having received permission to enter Abyssinia, had started from Adowa and landed at Massowah.

THE French traveller, M. le Comte de Semellé, arrived at Fernando Po on February 13 from the Upper Niger and Binué. He started on his expedition in May of last year, and had been engaged in pursuing his researches up to the time he returned to Fernando Po. He has forwarded to England and France an account of some of his discoveries.

THERE seems every likelihood that an attempt will be made to train African elephants as bearers of burdens, and indeed it is stated that an association has been formed for the opening up of African trade by this means. This seems to us a much more sensible and practicable plan than the construction of a railway, which has been so prematurely proposed in some quarters. With the aid at first of Indian trainers we see no reason why the African elephant should not be made as useful as his Indian brother.

THE intended laying of a telegraphic line from Aden to Zanzibar, and from Zanzibar to Port Natal *viâ* Mozambique and Delagoa Bay has promoted a project for connecting the Mascarene Islands either with Zanzibar by the Comoros or with Delagoa Bay through Madagascar by a special line. It is said that the general council of Reunion will take the lead. The aggregate population of the Mauritius and Reunion is more than half a million, and the trade of these two islands with Europe reaches annually a large sum. The density of population is very great.

MGR. LAVIGERIE, Archbishop of Algiers, has informed *Les Missions Catholiques* that it is in contemplation to increase the staff of the French Algerian Missionary expedition in Central Africa to ten priests, one of whom is to found a depot in the neighbourhood of Zanzibar for the missions of the interior.

WE gather from the *Colonies and India* that some interesting papers have appeared in the Ceylon press relative to the suitability of that island for the growth of Australian trees. The blue gum-tree does not seem to flourish under an elevation of 3,000 feet. The *Casuarina* grows freely even by the seashore. The *Grevillea robusta*, one of the most beautiful and most useful of Australian trees, had thriven well in Colombo itself, though it will not stand the full force of the sea breezes.

THE steam traffic in the Indian Archipelago has so largely increased since the opening of the Suez Canal that the roads of Batavia are found insufficient for the accommodation of the vessels, and the Netherlands Government have accordingly found it necessary to undertake the construction of a new port. This, we learn from the Manila papers, is situated in Cape Tanjong Priok, to the east of Batavia, and is to have communication with that city by means of a canal and a railway. The work of construction was commenced in 1877, and 3,000 men are at present employed on it. The new port, which is to be named after Prince Henry, will, it is expected, be finished in 1885.

THE last number of *Le Globe* contains the first instalment of a sketch by M. Veniukof of geographical discoveries in Asiatic Russia, translated by M. Metchnikof.

THE French Alpine Club has organised a tour for school boys for the Easter holidays. The excursionists will travel on foot in the two departments of Loiret and Loir et Cher, visiting Orleans forest, the banks of the Loire, Chambord Castle, and the forest of Fontainebleau, where the last frosty weather produced such extraordinary disorders. The regulations will be sent on request to the Secretaire-General of the Alpine Club, 31, rue Bonaparte, Paris. No limit of nationality is imposed. The excursion will last for seven days and be conducted by a staff of competent teachers.

A SPECIAL congress on the means of creating an inter-oceanic canal across the Darien Isthmus will be opened shortly by the Society of Commercial Geography of Paris.

BY a census taken in December last it appears that the population of Japan now numbers 34,338,304 souls. Yedo, which at one time had the reputation of being the most populous city in the world, contains 1,036,771 inhabitants and 236,961 houses.

NOTES

THE Copernican Society at Thorn has resolved to begin an international collection of funds for the erection of an observatory in that town, as a lasting monument to the great reformer of astronomy.

M. BAILLAUD, Professor of Astronomy to the Faculty of Sciences of Toulouse, has been appointed director for five years of the observatory of that city.

THE anniversary meeting of the Chemical Society was held on Monday, Dr. Gladstone, F.R.S., president, in the chair. The President presented his Annual Report on the state of the Society, which he characterised as affording ground for congratulation, the past year having been one of quiet prosperity. The Society numbers now over 1,000 members. Sixty-eight papers have been read and two lectures delivered by H. C. Sorby and S. H. Vines; the Faraday Lecture was delivered by Prof. Wurtz. The improved condition of the Society's library and journal was touched upon. In conclusion, the President urged the Fellows not to rest satisfied with the present attainments of the Society, but to promote research, and especially a general scientific culture in the workers, a culture which should promote largeness of view and prevent each investigator looking on his own subject as one of prime importance, to the exclusion of all others. The Report of the Research Fund Committee was then read, with a brief account of the investigations carried on in connection with the fund. After the customary vote of thanks to the officers, council, &c., the following officers and council were elected for the ensuing year:—President—Warren De La Rue, F.R.S. Vice-presidents—F. A. Abel, C.B., Sir B. C. Brodie, E. Frankland,

J. H. Gladstone, A. W. Hofmann, W. Odling, Lyon Playfair, A. W. Williamson, F. Field, J. H. Gilbert, N. S. Maskelyne, H. E. Roscoe, R. Angus Smith, J. Young. Secretaries—W. H. Perkin and H. E. Armstrong. Foreign Secretary—Hugo Müller. Treasurer—W. J. Russell. Other Members of Council—M. Carteighe, A. H. Church, W. H. Bartley, C. W. Heaton, E. Riley, W. C. Roberts, W. A. Tilden, W. Thorp, T. E. Thorpe, J. L. W. Thudichum, R. V. Tuson, R. Warington.

THE eminent algologist Dr. Rabenhorst has been compelled, in consequence of continued ill-health, to resign the editorship of the monthly cryptogamic journal *Hedwigia;* and it has now passed into the hands of Dr. G. Winter of Zürich, well known for his contributions to various departments of cryptogamic literature.

THE *Library Bulletin* of Harvard University is a publication much more interesting and valuable than its title would seem to imply. It is edited by the librarian, Mr. Justin Winsor, one of the greatest living authorities in all matters connected with libraries. We have before us No. 10 of the *Bulletin*, containing first of all a list of the more important accessions to the library between October 1878 and January 1879. This is a model list, and several of the entries are really elaborate essays, as that under the heading Maps, on Early Globes. The supplement to this list is devoted to articles, some of them of great scientific value. For example, under the title of "References in Analytic Geometry," we are furnished with a minute analysis of Descartes' Geometry. Prof. Goodall, under the title of "Floras of Different Countries" gives bibliographies of the Floras of Africa and America. In the supplement there is much other bibliographical material of literary and artistic value, each subject being continued in the supplements of successive *Bulletins*, until completed. Mr. Winsor intimates that he is preparing a list of all editions of Ptolemy's Geography, and desires detailed information of any editions that may be in foreign libraries. Altogether this *Bulletin* is one of the most valuable bibliographical publications we know of.

THE death is announced of Prof. Karmarsch, the well-known technologist. He was born in 1803. In 1823, when he was twenty years of age, appeared his first work, and his labours ended in 1872 with the publication of his greatly-valued "History of Technology." He was long director of the Hanover Polytechnic School, which was founded under his superintendence. He retired from active life in 1875.

THE French Minister of Public Works has given the required authorisation to M. Gaston Tissandier to establish Giffard's balloon in the Cours des Tuileries, and the works are progressing with great activity. The ascents are to begin on the first days of May, the price is to be reduced to 10 francs, and the admission fee for spectators to 50 centimes.

THE *Daily News* New York correspondent telegraphs that Mr. Edison has exhibited the working of his incandescent light in the illumination of his laboratory and factory, with excellent results, furnishing fourteen of the new lamps each from 18 to 20 candle power, on one circuit, giving a steady white light, much superior to the carbon, and equal to double the number of gas-jets. The generator was an ordinary Gramme machine of 2½-horse power. Mr. Edison states that he can now supply light for practical domestic use at less than half the cost of gas, but is experimenting for further improvements in the lamp and economy in the generator. He has discovered a new alloy, platinum and iridium, by the use of which he increases the number of lamps per horse power at least 50 per cent.

THE April number of *Mind* contains a paper of much interest by Mr. G. Stanley Hall on Laura Bridgman, the much-written-about American girl, who at an early age was deprived of nearly all her senses but that of touch; the paper is the result of a recent visit to Laura. Prof. Bain commences a series of papers on the life and character of John Stuart Mill.

THERE is a short and interesting article in the *Sanitary Record* of March 28 on the Registrar-General's method of estimating populations. It shows that the true method is not to take the average rate of increase during any decade to ascertain the increment of the following decade. The true rate of increase is obtained by the difference between the logarithms of any two decades.

THE number of lights for electric light-houses in France is to be increased by two important constructions, one on Planier Isle, off Marseilles, and the other at the mouth of the Gironde. There are at present in existence in France only three, one at Cape Grisnez and two at Cape La Heve, off Havre. These lighthouses are supplied with Alliance electro-magnetic engines.

THE session of the delegates of the Sociétés Savantes will be begun as usual at the Sorbonne after Easter, and M. Ferry will deliver an address on the occasion of the distribution of prizes.

A STRIKING and highly promising line of research has been recently adopted by Mr. Muybridge, of San Francisco, at the instance of Governor Stanford, viz., the instantaneous photography of animals in motion. Some of his earlier photographs of a fast-trotting horse presented attitudes wholly unexpected, and they were even thought absurd. The method latterly adopted seems to have been making the horse trot or gallop past twelve cameras, arranged in series, and by breaking threads stretched across its path, release an electric current, which effected exposure of the plates. Thus the flying steed was obtained in every position. Mr. Muybridge gave public exhibitions of what had been done. The small negatives were magnified into life size, and projected on a screen, so that every motion was visible. These exhibitions do not seem to have been appreciated by the San Franciscans. The *Scientific American*, however, and afterwards *La Nature*, have published cuts taken from the photographs, and much general interest has been awakened in these researches. Among those specially interested is Prof. Marey, who desired to be put in communication with Mr. Muybridge, as he wanted to ask his aid in solving certain physiological problems, so difficult to solve otherwise; *e.g.*, questions connected with the flight of birds. He had been dreaming of a kind of photographic gun, to seize the bird in an attitude or series of attitudes of flight. What beautiful zootropes, too, might be had! Mr. Muybridge's cartoons representing the fast gallop give a key to the breaking down of so many horses. It appears as though one fore-leg had to sustain the whole of the weight of horse and rider while the body is moved along five feet. And just before the foot is raised, a perpendicular from it would strike the back of the saddle; so that there is immense leverage, the centre of gravity being thrown so far forward of its support, and the tendons must have a terrible tension. These inquiries are being further developed by the liberality of Governor Stanford and the skill of Mr. Muybridge, and valuable results may doubtless be looked for.

AMONG recent lectures delivered at the Sorbonne was one by Prof. Marey, who has so admirably applied the graphic method in physiology. His subject was the circulation of the blood, and though (the auditory containing ladies) experiments involving the presentation of blood were naturally proscribed, he was able to give several striking demonstrations. One of these consisted in showing on an illuminated surface the phases of the heart beats of his assistant's and his own pulse, the beats being transmitted across the hall, by a thin tube, the pulsations actuating a small inscribing style placed before the electric lamp. He also exhi-

bited a number of his ingenious apparatus illustrating the circulation, working them with water.

THE "Sixth Annual Report" of the Michigan State Board of Health is of much more than local interest. The Board seems to be a body who have a very thorough and comprehensive idea of their duty, which is very faithfully carried out by their secretary, Mr. H. B. Baker. The Report contains inquiries into all sorts of subjects connected with the sanitary condition of the people of Michigan, and many of the results obtained are of general interest. For example, with regard to earthen vessels used for domestic purposes, we are told that to the glazing material used for the inside the oxide of lead is sometimes added, making, with the alkaline silicates, borates, &c., a very fusible and closely adhering glazing. But its use is very dangerous, especially if the vessel contains acid substances, such as pickles with vinegar; the glazing decomposes, and lead salts form, which either dissolve or become mechanically suspended in the contents of the jar, and there is great danger of chronic lead-poisoning. The Report also contains some useful remarks on the various substances used to enamel iron vessels. There is also a very careful study of the climate and topography of the lower peninsula of Michigan, the meteorology of Michigan for 1877, and other information of much value.

A NEW form of water-level indicator, we learn from the Society of Arts *Journal*, has lately been designed and constructed by the India-rubber, Gutta-percha, and Telegraph Works Company, Silvertown, and has been erected by them at the Leamington New Water-Works, where it is stated to be giving every satisfaction. The reservoirs from which the supply of water is distributed to the town are situated some half-mile from the pumping-station, and it was therefore found necessary to have some kind of indicator placed at the engine-house, in order to enable the man in charge of the engines to see at a glance the exact height at which the water stood in the reservoir, so that he might be able to regulate the rate of pumping accordingly. The indicator that has been placed at the engine-houses resembles somewhat, in outward appearance an ordinary round metal case clock; the dial, instead of being divided into hours, minutes, &c., is divided into twenty equal divisions representing feet, and corresponding to the rise and fall that is required to be registered. A hand on the dial points to one of the divisions, which at any particular instant corresponds to the height at which the water in the reservoir stands. This hand, for every foot rise in the level of the water, moves an equal number of divisions round the dial; whilst, as the water falls, the hand turns back in the other direction, so that it always points to the exact height at which the water stands in the reservoir. A single line of ordinary telegraph wire communicates between the indicator and the apparatus at the reservoir. This apparatus is so constructed that at every foot rise of the water one pole of a battery is brought into connection with the line for a certain space of time, and the current from the the battery actuating the indicator at the engine-house, causes the hand to move the requisite distance round the dial. On, however, the water falling, the opposite pole of the battery is brought into connection with the line, and this is made to cause the indicator hand to move in a contrary direction. The apparatus at the reservoir is actuated by an ordinary float and weight in the water, and is arranged in such a manner that the battery contacts are always of the same duration, irrespective of the rate at which the water may be either rising or falling. A variety of uses will at once suggest themselves to which this class of electric indicator might be advantageously applied, as it can be arranged, if required, to give a diagram on paper of the water level at stated intervals of times, instead of using a hand to point to the divisions on the dial, as in the present instance; also it is evident that it can quite as readily be made to give variations in inches as in feet. As a tide indicator it might be made very serviceable on many of our large rivers, and probably ere long we shall hear of some further uses to which this novel application of electricity has been applied.

A SHOCK of earthquake was felt at Hetzdorf and Oederan, Saxony, in the night from March 12 to 13. The shock had a north-westerly direction, and a violent storm was raging at the time. At Hall, in the Tyrol, a violent shock was felt on March 13, at 11.15 P.M., in the direction from west to east.

THE *Report* of the Glasgow Industrial Museum for 1878 is satisfactory, showing as it does that the institution, under the care of Mr. Paton, is in a fair way of developing into something worthy of a city of the first commercial importance.

WE have received a copy of the Hunterian Oration delivered at the Royal College of Surgeons on February 14 last, by Prof. Humphry. It is published by Macmillan and Co.

THE Spanish *Crónica Cientifica* of March 25 contains among other interesting papers a Catalogue of the Terrestrial Testaceous Molluscs of the plain of Barcelona.

PART I. has been sent us of "A Universal Dictionary for Architects, Civil Engineers, Surveyors, Sculptors, Archæologists," &c., &c., by Mr. W. J. Christy. The London publishers are Griffith and Farran.

PROF. VIRCHOW has left Berlin for Troy in acceptance of an invitation from Dr. Schliemann.

BULLETIN NO. 1 of vol. v. of the United States Geological Survey of the Territories, contains the following papers:—Notes on the Aphididæ of the United States, with descriptions of species occurring west of the Mississippi, by Chas. V. Riley and J. Monell; The relations of the horizons of extinct vertebrata of Europe and North America, by E. D. Cope; Observations on the faunæ of the miocene tertiaries of Oregon, by E. D. Cope; Notes on the birds of Fort Sisseton, Dakota Territory, by Chas. E. McChesney, Acting Assistant Surgeon, U.S.A.; Palæontological papers, No. 9: Fossils of the Jura-Trias of South-Eastern Idaho, by C. A. White, M.D.; Jura-Trias Section of South-Eastern Idaho and Western Wyoming, by A. C. Peale, M.D.; Fossil forests of the volcanic tertiary formations of the Yellowstone National Park, by W. H. Holmes; Palæontological papers, No. 10: Conditions of preservation of invertebrate fossils, by C. A. White; Supplement to the bibliography of North American invertebrate palæontology, by C. A. White and H. Alleyne Nicholson.

DR. C. V. RILEY has reprinted in a separate form the entomological papers contributed by him to the last meeting of the American Association for the Advancement of Science. They are: "The Philosophy of the Movements of the Rocky Mountain Locust," "A New Source of Wealth to the United States," "Notes on the Life-History of the Blister Beetles and on the Structure and Development of Hornia," "On the Larval Characteristics of Corydalus and Chauliodes, and on the Development of *Corydalus cornutus*," Biological Notes on the Gall-making Pemphigmæ."

THE additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. J. N. T. Martheze; a Globose Curassow (*Crax globicera*) from South America, presented by the Rev. Ralph Cooper; a Grey-breasted Parrakeet (*Bolborhynchus monachus*) from Paraguay, presented by Miss Maria Hilhouse; a Common Peafowl (*Pavo muticus*) from India, presented by Mr. F. B. Hopkinson; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. F.

Belcher; five European Geckos (*Phyllodactylus europæus*) from Italy, presented by Prof. H. H. Giglioli, C.M.Z.S.; a Cape Ant-bear (*Orycheropus capensis*) from South Africa, purchased.

INTELLECT IN BRUTES

WE have another batch of letters on this subject, the essential points of which we shall endeavour to give in brief space:—

Mr. Wm. Hogg tells us of an incident he witnessed when calling on Mr. W. H. Michael, a gentleman well known at the parliamentary bar, who resides at Queen Anne's Gate, St. James's Park. While they were sitting in the study, the French window of which communicates with a garden at the back of the house, and had a crank latch by which it could be opened on either side, a cat presented herself outside the window, pleading for admission. She continued to plead for some time, and finding no help from within she resolved to trust to her own powers. Eying the latch, which was four feet above her, she made a spring, caught hold of the crank with her fore feet, and putting her hind feet against the other half of the window as a fulcrum she pressed open the window. This she would do several times in succession. Mr. Michael informed Mr. Hogg that the cat had never been taught to do this.

D. R. S. sends the story of a little terrier that left her puppies only once a day to be fed, gulping down hurriedly a great quantity of porridge. Returning quickly to her family she would put up all the porridge in order that she and her puppies might together enjoy a hearty meal. When the terrier was scolded for a fault it rushed away to a little distance and catching up anything it could get hold of at once—a bit of stick, a straw, a slipper or anything at hand—it would come coweringly and lay it down at our feet, with an expression of utter submission. If we were not propitiated it would run off a second time and bring another peace-offering, often in its distress catching things it would not at any other time have dared to touch.

M. W. T. writes:—A farmer, in Somersetshire, was going to a neighbouring village some three miles distant, and, not wishing to take his dog he ordered him home. The dog reluctantly obeyed. When the man arrived at a spot, about half way on his road, where the short cut he had taken across the fields joined the more circuitous road, he found the dog waiting for him. Evidently the animal had taken the longer route, which he doubtless knew, calculating on meeting his master at that point, and thus gaining his end without hindrance.

Mr. John Harmer, of Wick, Arundel, possessed a few years ago a very fine and intelligent tom-cat which was much addicted to plundering a rabbit-warren about a mile from his home. After a time it was noticed that before he proceeded on one of his expeditions "Sam" completed his toilet by wallowing in the filth turned out of the tame rabbits' hutches, he taking particular care that his neck and breast should be in as disgusting a condition as possible by smearing them up and down till both were saturated and the fur all matted together.

Mr. J. J. Cole of Mayland, Sutton, Surrey, writes:—It has been my custom to have—not a letter-box in a door in the usual way but the plate and flap in the bottom of a window sash near. I had a cat which often saw a servant go to the window on hearing the flap moved by the postman, and which, when shut out used to jump on to the window sill and rattle the flap and when the servant was seen through the glass jump down to be let in at the door. I knew a horse which during week days went round and round to the left, grinding in the cellar of a snuff maker in London. On Sundays his owner turned him out in a field at his place in the country where the horse went round and round all day long unwinding himself the other way. Why?

Mr. B. G. Jenkins describes a scene he witnessed between the large insect known as "daddy long-legs" and a small spider. The former got caught by one of its hind-legs by a pendant thread of cobweb about eight inches long, at the other end of which was the small spider. The spider cautiously descended on the thread, doubling it as he came, and secured the insect's leg more firmly. He then ascended about three inches, and drew the insect up about half an inch; but a violent resistance on the part of the latter induced him to give up the attempt. He, however, went up the thread, strengthening it as he went, and coming down again to the same place, evidently attempted once more to raise his prey, but without success, for the insect resisted so stoutly that it appeared to me to stretch the thread. The spider, Mr. Jenkins writes, saw clearly that the insect was too strong for him, that he would never be able to draw him up to the centre of his web, and that if he did not take very summary measures he would lose him altogether; so, on the principle that half a loaf is better than no bread, he set to work to secure a portion of it. The hind-leg of the insect, to which he had his web fastened, was composed of four jointed portions. Round three of these he busied himself weaving a web. Mr. Jenkins noticed particularly that he did not go up to the last jointed portion, that attached to the body. Having well secured these three, he moved up to the joint, and for a few moments appeared perfectly still. Suddenly the insect darted away, leaving three-quarters of its leg behind. What other explanation is there than that the spider disconnected it at the joint? Quietly ascending the thread, which he carried with him, and of course the leg as well, he properly placed the latter, settled down at the union of the two uppermost portions, gorged himself with juices from above and below, and then retired for the night.

Several correspondents express surprise at Mr. Henslow's position with regard to "abstract" and "practical" reasoning. They think that several of the instances adduced render that position untenable, and prove that in their degree the animals referred to showed themselves possessed of powers of "abstract" reasoning. With regard to the dog and bell story, Dr. Rae writes:—It was never intended to be understood that the dog associated the bell with "a *particular* maid," as Mr. Henslow puts it; any of the other servants would have done equally well. The dog could only show his reasoning powers by declining to ring the bell; for had he rung it, Mr. Henslow or any one else would naturally have said that the "brute" had shown no reasoning powers at all. Mr. Henslow has passed over without notice the fox and gun story, which, by his own definition, was as clearly a case of abstract reasoning as could be adduced, differing only in form of carrying into effect from what he would have recommended, which, if adopted by the fox, would have led to its destruction.

Dr. G. Frost sends the following good story:—

In answer to Mr. Henslow's request for an example of "abstract reasoning" in the lower animals (NATURE, vol. xix. p. 433), I beg to subjoin the following:—Our servants have been accustomed during the late frost to throw the crumbs remaining from the breakfast table to the birds, and I have several times noticed that our cat used to wait there in ambush in the expectation of obtaining a hearty meal from one or two of the assembled birds. Now, so far, this circumstance in itself is not an "example of abstract reasoning." But to continue: For the last few days this practice of feeding the birds has been left off. The cat, however, with an almost incredible amount of forethought, was observed by myself, together with two other members of the household, to scatter crumbs on the grass, with the obvious intention of enticing the birds. I think Mr. Henslow might now be convinced that animals also possess in an inferior degree that boasted reasoning power which is generally supposed to belong to man alone.

THE PLANE OF POLARISATION ELECTRO-MAGNETICALLY ROTATED IN A VAPOUR

IT is known that Faraday did not succeed in proving electro-magnetic rotation of the plane of polarisation of light in gases, nor have others succeeded. Considering the interest attaching to this question, Herr Kundt and Herr Röntgen lately thought to repeat the attempt with very strong currents and under the most favourable conditions. The result is that they have been able to prove the rotation, at least in the case of sulphide of carbon vapour. (Their researches have been communicated to the Munich Academy.)

Sulphide of carbon was chosen because, on the one hand, it shows a strong electro-magnetic rotation in the liquid state, and on the other, its vapour has a considerable tension, even at low temperatures. An iron tube was used for inclosure and heating of the substance; it was closed at the two ends with glass plates 1 ctm. thick, and itself inclosed in a tin-plate tube; so that steam could be led between the tubes to heat the inner tube throughout to 100°. The outer tube was surrounded by six large wire-coils, each having 400 windings of wire 3mm. thick,

through which was passed the current from sixty-four large Bunsen elements. A little sulphide of carbon was introduced into the inner tube, and the air having been driven out by vapour forming at ordinary temperature, the tube was closed and fixed in position, and steam was sent through the space round it.

When the whole tube had taken the temperature of boiling water the glass plates and the sulphide of carbon vapour within became quite transparent. A beam of light rectilinearly polarised by a Nicol was now sent through, and a Nicol at the other end extinguished it. The current of the sixty-four elements being now allowed to flow, a distinct brightening of the field was observed. The brightening became still greater when, after closing the circuit, the foremost Nicol was turned to darkness, and the current then reversed with a commutator. The rotation of the plane of polarisation occurred, as was to be expected, in the direction in which the positive current passed through the wire coils.

To test whether the rotation might not be due wholly or in part to the glass plates closing the inner tube, the experiment was made without any sulphide of carbon in this tube. A weak rotation, due to the glass, was indeed observed, much smaller than in the other case. To avoid this, however, as much as possible, the wire coils next the glass plates were shut out from the circuit. The four coils now traversed by the current were so far from the plates that their influence must have been very small, indeed the plates then gave no perceptible rotation. Sulphide of carbon having been again admitted, and the experiment repeated, there was a well-marked brightening as before, when the current passed. The amount was roughly estimated at half a degree.

It is thus proved that saturated sulphide of carbon vapour at about 100°, *in the magnetic field, rotates the plane of polarisation of light.*

Sulphuric ether was tried in the same way, but gave no effect.

The authors consider it can hardly be doubted that, with suitable arrangements, the rotation may be demonstrated in the case of unsaturated vapours and gases. They are engaged in making an apparatus which will enable them to examine permanent gases at very high pressures in the magnetic field, in order to prove the rotation in their case, and, if possible, to measure the phenomenon. "It would be specially interesting," they remark, "to ascertain whether oxygen rotates the plane of polarisation in the same direction as other gases."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

A BROAD and liberal scheme has been published by the Cambridge Syndicate on the affiliation of local Colleges to the University as suggested in various Memorials. They have taken a large amount of evidence and have had interviews with deputations from Nottingham and Sheffield. They have also held conferences with a Committee of the Hebdomadal Council of the University of Oxford, with whom they find themselves in general agreement. The Syndicate recommend that application be made to the University of Cambridge Commissioners for the powers required to enable the University to affiliate local Colleges, and that the following conditions of affiliation be established by grace of the Senate. Any educational institution within the British dominions, in which the majority of the students are over seventeen years of age, may be admitted on condition that it be incorporated by Royal Charter, or established on a permanent and efficient footing; that the University shall be represented on its Governing Body, and undertake the general conduct of its Examinations; and that the connection between the University and an affiliated College shall be established and shall be terminable by a grace of the Senate, or by a resolution of the Governing Body of the College. Persons who have completed an approved course of three years at an affiliated College, passing satisfactorily the Examinations connected with that course, will be entitled to receive a University Certificate, and if they obtain honours in the final Examination connected with that course, shall be excused the previous Examination; and provided they obtain a degree by one of the Tripos Examinations will be permitted to take their degree after only six terms' residence at Cambridge. In each College there are to be three examinations yearly, the Annual College Examination, the First and the Second Examinations, the Annual College Examination is to be held in subjects taught with the sanction of the University, in the College, and be open to those students only who have satisfactorily attended the teaching in these subjects. To pass the First Examination every candidate will be required to satisfy the Examiners in (1) Arithmetic; (2) Euclid, Books I., II., and III.; and Algebra, to Quadratic Equations inclusive; (3) One of the following languages: Latin, Greek, French, Italian, German. Candidates will be at liberty to take up more than one language, and one or more additional subjects, including Heat, Experimental Mechanics, Chemistry, Botany, and Mathematics. 4. The Second Examination shall include four groups: (1) Ancient and Modern Languages, two to be taken. (2) Mathematics, one higher subject, pure or applied, being required. (3) Natural Science. Candidates pass in Elementary Chemistry and Physics, and also in one of the following:—Higher Chemistry, Higher Physics, Animal and Vegetable Physiology, Comparative Anatomy with selected portions of Zoology, Vegetable Anatomy and Physiology with Classificatory Botany, Geology and Physiography, Mineralogy. Candidates to pass in (1) English Constitutional History and (2) Political Economy or Logic, and subjects connected with History, Literature, and Philosophy. A pass in one group will give a pass in this second examination, and honours may be obtained on the minimum number of subjects. The Syndicate think it desirable to avoid if possible increasing seriously the severe strain caused by the outside work of the University. The sections and groups of the senior and higher local examinations are in general correspondence with the scheme, and the lectures at the centres are under the superintendence of the Syndicate for conducting local examinations and lectures. Thus there is machinery in existence which may, with some modifications, be conveniently and properly used. It is thought desirable that the Universities of Oxford and Cambridge should, as far as practicable, act in concert in conducting this great scheme of affiliation. It is recommended that the scheme be so administered as to be self-supporting.

THE Cambridge Council of the Senate propose to repeal entirely the few unrepealed provisions in the will of Dr. Woodward relating to the Professorship of Geology, and to frame a new statute on a plan already approved for the Professorship of Chemistry. The same plan is likely to be followed with regard to the chairs of Anatomy, Botany, and Mineralogy, the nomination of half the Electoral Board in the case of anatomy falling to the Board of Medical Studies and in the other professorships named, to the Board of Natural Science Studies.

IN consequence of the greater importance to be given in future to the first part of the Cambridge Natural Sciences Tripos, held in June, a practical and oral examination is to be held then, two extra days being allowed tor this.

THE Higher Senior Class of Mathematics in University College, London, which had been conducted by the late Prof. Clifford, has been intrusted during the summer term of the present Session to Mr. M. J. M. Hill, M.A., Fellow of the College, and fourth wrangler and bracketed equal Smith's prizeman this year at Cambridge.

DR. WITTROCK, the well-known algologist of Upsala, has been appointed lecturer on Botany, and curator of the botanical section of the museum at Stockholm.

THE commission of the Chamber of Deputies proposes to establish a compulsory system of education in France. Parents neglecting to comply with the provisions of the law are to be fined, and in certain cases to be sent to prison for a certain period. The expenses required for enlarging school accommodation and adding to the number of teachers are to be supported by the National Exchequer.

SCIENTIFIC SERIALS

American Journal of Science and Arts, March.—In the opening paper Prof. Norton contends that under varying conditions the ultimate molecules of bodies are subject to changes in the intensity of their attraction or repulsion, at a given distance of neighbouring molecules (temperature and chemical constitution remaining constant). Evidence of this is found in the phenomena of permanent distortion of materials after temporary subjection to a force of stress; in observed changes in the mechanical properties of materials, through tension, pressure, heat, &c.; change of mechanical properties of a body through presence of minute quantities of other substances; and certain facts in chemical physics (phenomena of solution, allotropy, the nascent

state, &c.). The hypothesis is advanced, that the ethereal atmosphere condensed round an atom by its attractive action consists of an atmosphere of luminiferous ether, and an envelope of electric ether immersed within this for a certain depth, an ethereo-electric atmosphere, in fact.—Some observations on flocculation of small particles (or their tendency to form, under moderate agitation, granular aggregates or compound particles of larger size), are described by Prof. Hilgard, and have important physical and technical bearings, especially on points in agriculture.—Prof. Dawson points out what he considers defects and errors in the method of investigation pursued by Prof. Möbius recently with regard to *Eozoon canadense*, leading to a decision adverse to the organic character of that object.—Mr. White offers some remarks on the Jura-trias of Western North America; Mr. Fontaine continues his notes on the mesozoic strata of Virginia, and Mr. Bannister contends for the hypothesis of the transition character of the Rocky Mountain lignite series, or Laramie group.—Some new species of anthozoa and cephalopoda added to the marine fauna of the eastern coast of North America, are described by Prof. Verrill; the cephalopoda have some specially interesting features.—Mr. Penfield gives analyses of triphylite.

Annalen der Physik und Chemie, No. 2.—In this number Herr Thoss communicates an interesting paper on artificial dichroism. He experimented (to produce it) in the three directions of making a coloured isotropous medium doubly refractive, colouring a doubly-refractive medium, and giving a colourless isotropous medium both colour and double refraction. The last series were negative in results. In the first series, plates of gutta-percha, indigo, and chrysamminate of potash gave convincing proof that there is no difference between double refraction produced mechanically and double refraction in crystals. It was found impossible to produce dichroism with pressure in coloured glass. Colouring matter in crystals is considered the real producing cause of dichroism. The subject of quickly alternating electric currents is treated by Herr Oberbeck, who notes as an important fact the diminution of the resistance of liquids by increase in the number of alternations of the transmitted current in unit time; this occurs only when the number becomes high, and the average time of passage of one constituent molecule to its neighbour in the direction of the current can no longer be regarded as infinitely small in comparison to the duration of the current. The author describes experiments on alternating currents in two induction coils, variously connected, and finds in the phenomena certain analogies to vibrations of the nature of sound and light.—Herr Lubarsch endeavours to show that the faultiness of past experiments on fluorescence has arisen only from the first of three causes assigned by Prof. Lommel, viz., absorptive action of the fluorescent liquid on the fluorescent light, in observation of the liquid mirror. He finds evidence of the generality of this law: in all fluorescent substances the more refrangible limit of the derived spectrum coincides with the place of strongest absorption in the absorption spectrum, or (where this is not distinctly perceptible) with the place of strongest fluorescence in the fluorescent spectrum. Substances with double fluorescence, as chlorophyll (the phenomena of which he describes), are not excepted from the law.—Herr Rudorff describes a simple and convenient apparatus for determining the specific gravity of powdered substances; Herr Wiedemann and Herr Schulze, an arrangement with which can be proved the dissociation of hydrate of chloral at 100°; Herr Wiedemann, experiments yielding the result that by passage of electricity a gas may become luminous far under 100°, &c.—A large part of the number is occupied with the concluding part of Kohlrausch's paper on electric conductivity, &c., already referred to.

Journal de Physique, February, 1879.—The opening paper by M. Jamin, on complements to the theory of dew is followed by one in which M. Lippmann shows that the depolarising property of a metallic solution is limited to the same metal as it contains; and that this electric reaction may be applied, in several cases, in testing for a metal, as a convenient auxiliary of chemical analysis. The electric work expended to produce polarisation is stored (he contends) not in the form of chemical energy, but in that of electrical, as in a condenser.—M. van der Mensbrugghe offers some remarks on measurement of the superficial tension of liquids, *apropos* of recent experiments by M. Terquem.—M. Gernez describes a method of observing the rotatory power of quartz at different temperatures, and which seems to meet the difficulties of the case better than that of M. Joubert and other physicists. Two quartzes of contrary rotation are fixed at the two ends of a tube, and only one is heated. A universal support or electro-diapason for inscribing and showing in projection vibratory movements is described by M. Duboscq.

SOCIETIES AND ACADEMIES

London

Royal Society, March 27.—"On the Organisation of the Fossil Plants of the Coal Measures. Part X." By W. C. Williamson, F.R.S., Professor of Natural History in Owens College, Manchester.

The still existing differences of opinion respecting the botanical affinities of the Sigillariæ give value to every new fact calculated to throw light upon the question. In 1865 Mr. Edward Wunsch, of Glasgow, made a discovery, which proves to have an important bearing upon it. He found, at Laggan Bay, in Arran, a series of rather thin carboniferous strata, separated by thick beds of volcanic ash, and in one of the carboniferous shales especially, he discovered the bases of the stems of numerous very large trees standing perpendicularly to the shales. These trees have been referred to by several authors as Sigillarian. In the summer of 1877 Mr. Wunsch and I employed quarrymen to make extensive excavations amongst these strata, for the purpose of adding to the extensive series of specimens which he had obtained, and the whole of which he kindly placed in my hands. The aggregate result of these explorations was to show that the conclusion previously arrived at, viz., that the stems had belonged to a grove of Sigillarian trees, was unsupported by a solitary fact. These stems were of very large size, showing that they had belonged to fully grown trees. None of them displayed any traces of leaf-scars, having outgrown the stages at which such scars would remain visible. Their outer surfaces were scored with deep irregular longitudinal fissures, resulting from internal growth and consequent expansion, and which appear to have been mistaken for the longitudinal grooves and ridges of a Sigillarian bark. Such, however, they certainly were not, since, in every instance, the surface bark had been entirely thrown off, and the fissures entered deeply into the subjacent bark layer. In most of the stems this comparatively thin bark layer was the only one that remained, the greater portion of the inner bark and the central vascular axis having disappeared, leaving a large cylindrical cavity, which was filled up with volcanic ash. These stems failed to display a single feature justifying the conclusion that they were Sigillarian.

In two of them the central cavity, instead of being filled with ash, was filled with miscellaneous heaps of vegetable matter, amongst which were large fragments of the vascular axes of various plants, such as Lepidodendra and Stigmariæ, but in one of the largest stems were five or six decorticated vascular cylinders of Diploxyloid stems, of the largest size, and which, though arranged parallel to the long axis of the cylinder which inclosed them, obviously did not belong to them, but had been floated in from without. The supposition that these had been young stems that had grown within the hollow protecting cylinders, from spores, accidentally introduced, is wholly untenable, since each one of these several vascular axes has been the centre of a stem fully as large as that within which we found them aggregated. Of course, these Diploxyloid vascular axes had the organisation which Brongniart and the younger school of French botanists which still upholds his views on this point, believe to be characteristic of true Sigillariæ—a conclusion from which I have long dissented.

The only fragments we found, that threw any light upon the character of the leaf-scars that had indented the surfaces of these fully-grown stems, was a well-defined example of the Lepidodendroid type.

We directed careful attention to the nature of the smaller fragments of branches and foliage which abounded in the volcanic ash with which the large stems were overlaid. These consisted of Lepidodendroid branches and twigs of all sizes and ages, and no doubt was left upon my mind that they were really the *disjecta membra* of the stems around which they were so profusely scattered. The only fruits that have been obtained from the same locality are Lepidostrobi, most of which contain macrospores and microspores. Unless we are prepared to believe that this Arran deposit contained, on the one hand,

numerous stems without branches, and, on the other, yet more numerous branches without stems, we must recognise in these specimens the complementary elements of a grove of Lepidodendroid trees.

One specimen found is a very important one. It has a mean diameter of six inches, and is either a small stem or a very large branch. Internally it exhibits the same structure as all the smaller Lepidodendroid branches, except so far as it is modified by size and age. But in addition to its other features, it exhibits *a very narrow* exogenous ring surrounding the ordinary Lepidodendroid one, thus giving some clue to the size attained by such branches before the internal organisation passed from the Lepidodendroid to the Sigillarian type.

The important discovery by Mr. D'Arcy Thompson, of Edinburgh, of young branches of Ulodendron with reproductive cones actually attached to the scars characteristic of the genus, finally settles the nature and functions of these scars, showing that they mark the positions from which bilaterally arranged deciduous organs of fructification have fallen.

The structure of *Calamostachys Binneyana* has had further light thrown upon it, sustaining my previously expressed convictions that it had a triquetrous axis, and that consequently its affinities were with Asterophyllites and Sphenophyllum, and not with Calamites. A specimen demonstrates that the six vascular bundles going to the six fertile sporangiophores were given off in pairs from the three truncated angles of a triangular vascular axis—an orientation absolutely indentical with that represented in similar sections of stems of Sphenophyllum, published by M. Renault. The recent discovery by Herr Stur, of Vienna, of a plant in which Sphenophylloid and Asterophyllitean leaves are found upon a common stem, establishes the correctness of my previous conclusions, as to the very close affinities of these two genera.

A large series of specimens from Oldham and Halifax has enabled me to investigate in detail the very curious objects to which Mr. Carruthers gave the name of Traquairia, and which that observer believes to be a form of Radiolarian life. Their very elaborate organisation can scarcely be made intelligible without the aid of plates. In a previous memoir (*Phil. Trans.* 1874, p. 56), I ventured to doubt the correctness of Mr. Carruthers' conclusions, and expressed my conviction that these objects resembled spores rather than protozoan skeletons. Further study of their details of structure has only strengthened this opinion which has also received the important support of Professors Hæckel and Strasburger, of Jena, both of whom have carefully studied my collection of specimens. These objects are small spheres—the sphere-wall of which is prolonged into a series of long radiating tubes not unlike the muricated species of a Cidaris. In their young state each murication gives off a delicate thread or threads, which ramified freely in an apparently mucilaginous or gelatinous, structureless, investing magma. In older specimens these threads developed into branching and radiating cylindrical tubes which, like the primary ones, had very thin walls. Within the outer sphere-wall, which consists of the coalesced bases of these branching tubes, were at least two other thin layers of membrane, and in several of the specimens the interior of the capsule is filled with cells, exactly like those seen in the corresponding cavities of Lycopodiaceous macrospores found in the Halifax deposits from which the finest Traquairiæ have been obtained. These objects differ considerably from all known reproductive structures; but I agree with Prof. Hæckel in his very decided rejection of them from the Radiolarian group of organisms, and with his conclusion that they are vegetable and not animal structures. Prof. Strasburger thinks it most probable that their affinities are with the macrospores of the Rhizocarpæ.

Myriads of the vegetable fragments both from Oldham and Halifax are drilled in all directions with rounded insect or worm borings, and further traces of these zylophagous animals are seen in innumerable clusters of small Coprolites of various sizes, the size of those composing each cluster being uniform.

Desirous of verifying Count Castracane's alleged discovery of Diatoms in coal, specimens of twenty-two examples of coal from various localities in Yorkshire, Lancashire, and Australia were reduced, after the Count's method, to a small residue of ash. This work was done for me in the chemical laboratory of Owens College through the kindness of Prof. Roscoe. Like Mr. F. Kitten, of Norwich, the Rev. E. O'Meara, of Dublin, and the Rev. G. Davidson, of Logie Coldstone, I have failed to discover the slightest trace of these organisms in coal.

The last objects described are some minute organisms from the carboniferous limestones of Rhydmwyn, in Flintshire, and which were supposed by Prof. Judd to have been siliceous Radiolarians from which the silica had disappeared and been replaced by carbonate of lime. I fail to find any confirmation of this conclusion. The objects appear to me to constitute an altogether new group of calcareous spherical organisms that may either have been allied to the Foraminifera or have had some affinities with the Rhabdoliths and Coccoliths. I have proposed for several species of the organisms the generic name of Calcisphæra. Myriads of objects of similar character, but of larger size, constitute the greater portion of a Corniferous limestone from the Devonian beds of Kelly's Island, U.S.A.

Additional light is thrown upon some Lycopodiaceous Strobili, fern-petioles, *Sporocarpous* or cryptogamic conceptacles, and other spore-like bodies, Gymnospermous seeds and stems.

Chemical Society, March 20.—Dr. Gladstone, president, in the chair.—The following papers were read:—On plumbic tetrethide, by E. Frankland and A. Lawrance. The authors prepared this compound by adding plumbic chloride to zinc-ethyl, and distilling the product in a current of steam. Ammonia, carbonic anhydride, carbonic oxide, cyanogen, nitric oxide, oxygen, and sulphuretted hydrogen, do not act on this substance at ordinary temperatures; sulphurous anhydride converts it into a white amorphous mass, consisting of diethylsulphone and plumbic ethylsulphinate.—Prof. W. Foster gave a verbal communication on the production of the higher oxides of iron, chromium, manganese, and bismuth. When the salts of the above metals are treated with an alkaline solution of sodic hypobromite, ferrates, chromates, permanganates, &c., are formed, oxygen being evolved. Copper sulphate solution, when mixed with the hypobromite solution, evolves oxygen at ordinary temperatures.—On the decomposition of water by certain metalloids, by C. F. Cross and A. Higgin. The authors conclude that pure sulphur decomposes water, uniting both with its oxygen and hydrogen; the decomposition is independent of atmospheric oxygen. Amorphous phosphorus decomposes lead acetate solution, but is without action on water at 100°. Vitreous phosphorus does not decompose water at 100° when air is excluded.—On the volumetric determination of chromium, by W. J. Sell. To the boiling solution containing chromium, acidified with sulphuric acid, permanganate is added until a pink tint remains after boiling for three minutes; the manganese is precipitated by the addition of sodium carbonate and alcohol, and filtered off; the chromic acid in the filtrate is then determined by iodine and hyposulphite. The author also gives details of a method of fusing chrome iron ore, by means of which an estimation of the chromium can be made in an hour and a quarter.

Geological Society, March 12.—Henry Clifton Sorby, F.R.S., president, in the chair.—Lazarus Fletcher, Arthur Samuel Hamand, William J. Pope, and George W. Slatter, were elected Fellows of the Society.—The following communications were read:—On perlitic and spherulitic structures in the lavas of the Glyder Fawr, North Wales, by Frank Rutley, F.G.S. He mentioned the fact that the lavas of Bala age in Wales were generally vitreous, and instanced some remarkable cases of spherulitic structure from that district. Prof. Judd stated that among the most ancient rocks of the north-west of Scotland were lavas showing spherulitic and fluidal structure. These were also common in the old red sandstone lavas. He thought that as the spherulitic, perlitic, and fluidal structures were in rocks of modern date confined to vitreous varieties, the inference was safe, when applied to ancient rocks, that they were once glass. Dr. Sheibner asked if an analysis of the rock had been made. If the rock was a true perlite, there should be about 80 per cent. of silica. If the rock was altered, one might expect a large excess of magnesia. Prof. Ramsay said that the character of these lava-flows was evident even without microscopic examination. He recapitulated the evidence which had persuaded him of this when surveying the district, and expressed doubt as to the rocks at the base of the Cambrian in North Wales being true lava-flows. Dr. Hicks said he thought there was no reason why a perlitic structure should not occur in rocks of Bala age. He thought the first spherulitic rocks recognised in this country had come from rocks of Arvonian age at St. David's. Mr. Bauerman said that modern lava-flows often cover very large areas, as in North America and India; so the mere distance of the Wrekin from Wales would be no difficulty. Mr. Rutley doubted whether spherulitic structure was

always connected with vitreous. He did not see that the presence of magnesia would prove or disprove alteration. He did not think a rock could be vitreous if solidified at a great depth, since it would hardly be able to cool with sufficient rapidity.—The gold-leads of Nova Scotia, by Henry S. Poole, F.G.S., Government Inspector of Mines. The author remarked upon the peculiarity that the gold-leads of Nova Scotia are generally conformable with the beds in which they occur, whence Dr. Sterry Hunt and others have come to the conclusion that these auriferous quartz veins are interstratified with the argillaceous rocks of the district. With this view he does not agree. He classified the leads in these groups according to their relations to the containing rocks, and detailed the results of mining-experience in the district, as showing the leads to be true veins by the following characters:—(1) Irregularity of planes of contact between slate and quartz; (2) The crushed state of the slate on some foot-walls; (3) Irregularity of mineral contents; (4) The termination of the leads; (5) The effects of contemporary dislocations; (6) The influence of strings and offshoots on the richness of leads. The author further treated of the relative age of the leads and granite, and combated the view that the granites are of metamorphic origin, which he stated to be disproved by a study of the lines of contact. He also noticed the effects of glaciation on the leads, and the occurrence of gold in carboniferous conglomerate.—On conodonts from the Chazy and Cincinnati groups of the cambro-silurian, and from the Hamilton and Genesee-shale divisions of the devonian, in Canada and the United States, by G. Jennings Hinde, F.G.S. After a sketch of the bibliography of the subject, the author described the occurrence of conodonts. In the Chazy beds they are associated with numerous *Leperditiæ*, some trilobites, and gasteropods; in the Cincinnati group with various fossils; and in the devonian strata principally with fish-remains; but there is no clue to their nature from these associated fossils. They possess the same microscopic lamellar structure as the Russian conodonts described by Pander. The various affinities exhibited by the fossil conodonts were discussed; and the author is of opinion that though they most resemble the teeth of myxinoid fishes, their true zoological relationship is very uncertain. The paper concluded with a classification of the conodonts from the above deposits.—On annelid jaws from the cambro-silurian, silurian, and devonian formations in Canada, and from the lower carboniferous in Scotland, by G. Jennings Hinde, F.G.S. After referring to the very few recorded instances of the discovery of any portions of the organisms of errant annelids as distinct from their trails and impressions in the rocks, the author noticed the characters of the strata, principally shallow-water deposits, in which the annelid jaws described by him are imbedded. A description was given of the principal varieties of form and of the structure of the jaws. They were classified from their resemblance to existing forms under seven genera, five of which are included in the family Eunicea, one in the family Lycoridea, and one among the Glycerea. The author enumerated fifty-five different forms, the greater proportion of which are from the Cincinnati group.

Meteorological Society, March 19.—Mr. C. Greaves, F.G.S., president, in the chair.—The following were elected Fellows of the Society:—R. Burniston, W. H. Crawford, J. Davies, The Earl of Derby, H. Downs, S. Egar, J. S. Hodgson, S. Hollins, T. M. Hopkins, H. Horncastle, C. W. Johnson, E. M. Nelson, and F. Wilkin.—The papers read were:—Dew, mist, and fog, by George Dines, F.M.S. The author has during the last two years made a number of experiments to determine the amount of dew that is deposited on the surface of the earth. The plan adopted was as follows:—Glasses similar to ordinary watch-glasses were procured; the surface area and the weight of each was ascertained. These glasses were exposed to the open air in the evening, being placed on different substances, viz., on grass, on slate, and on a deal board, the two latter being raised a few inches above the grass. A minimum thermometer was generally placed by the side of each glass. It is only on rare occasions that an amount of dew exceeding the 0·010 inch in depth has been deposited upon the measuring glasses, and out of 198 observations, in only 3 has that amount been exceeded. Fifty-eight observations give the amount from 0·010 to 0·005 inch: 107 from 0·005 to 0·001 inch; 22 less than 0·001 inch; and 8 observations no dew at all. The author thinks it may be fairly assumed that the average annual deposit of dew upon the surface of the earth falls short of 1·5 inch. There are two kinds of mist, the morning and evening; the morning mist is caused by the evaporation from the water and the moist ground taking place faster than the vapour is taken away; the air becomes saturated, but this does not stop the evaporation; the vapour continues to rise into the air, is there condensed, and forms mist, which gradually spreads over a wider surface. The evening mist is produced as follows:—The cold on the grass caused by radiation lowers the temperature of the air above it; the invisible vapour of water previously existing in the air is in excess of that which the air can retain when the temperature is lowered; the surplus is condensed, becomes a mist-cloud, and floats in the air just above the surface of the grass. Taken either separately or combined, the mists appear to the author totally and altogether inadequate to account for those dense fogs which at times overspread large tracts of country. Dense fogs near the earth are often accompanied by a clear sky above, when the sun may be seen reflected from the gilded vanes of our public buildings. After long consideration the author is inclined to attribute these fogs to some cause at present unknown to us, by which the whole body of the air to some distance above the surface of the earth is cooled down, and, as a consequence, part of the vapour in that air is condensed and forms what has been called an "earth-cloud."—On the inclination of the axes of cyclones, by the Rev. W. Clement Ley, M.A., F.M.S. The object of this paper is to call attention to the evidences recently afforded by the results of mountain observations to the theory that "the axis of a cyclone inclines backwards." The author first reviews the state of the question up to the present time, and details his own investigations, chiefly founded upon the movement of cirrus clouds; he then refers to Prof. Loomis's recent "Contributions to Meteorology," in which is discussed the observations at the summits and bases of several high mountains, the results of which fully confirm the theory that the axis of a cyclone inclines backwards. The discussion on this paper was adjourned till the next meeting.—Contributions to the meteorology of the Pacific. No. III. Samoan or Navigator Islands, by Robert H. Scott, F.R.S.

Physical Society, March 22.—Prof. W. G. Adams, president, in the chair.—New Member, Capt. Hastings R. Lees, R.N.—Capt. Abney, R.E., F.R.S., read a paper on obtaining photographic records of absorption spectra. Absorption spectra have hitherto been recorded by the difficult method of hand-copying; but the discovery by Capt Abney of a silver salt sensitive to all rays in different degrees renders the photographic method available. The records thus obtained are photographs of the spectrum of the naked light of the source and of that of the same light reduced by insertion of the absorbing material in its track, and these are taken parallel, so that the dark absorption lines can be readily compared. Examples of these were thrown by him on the screen. This method can be used as a new weapon in attacking solar physics and determining whether or not compound bodies exist in the sun. Absorption spectra to compare with the sun can be got for compound bodies by burning the matter in question in a flame in front of the slit and passing a bright light through the flame.—Prof. Guthrie, F.R.S., then read a paper on the fracture of colloids, as illustrated by experiments on the breakage of glass plates either by pressure or heating at the centre or round the circumference. Circular plates of glass, pressed at centre or circumference, break in radial lines. However supported, a plate breaks in the same fashion if heated in the same way. If heated in the middle the crack is peak-shaped, like an obelisk on a double pedestal, two cracks forming the outline, with sometimes a third down the middle. The two cracks unite before they reach the edge on one side, and (as afterwards pointed out by Prof. W. G. Adams) the three extremities of the two cracks all meet at right angles to the edge. The crackage varies with the size and shape of the plates, the flame, and kind of glass; but the type is the same for all. Cracks cross each other. Prof. Guthrie defined a crack as the line where the ratio of cohesion to strain is least, and likened it to the lightning flash. Mr. W. Chandler Roberts, F.R.S., said that he had observed once a volute spiral crack in dried hydrated silicic acid, and recommended Prof. Guthrie to study cracks in agate, which is the most perfect colloid known.

Geneva

Society of Physics and Natural History, November 6, 1878.—M. Raoul Pictet read a paper on temperature and on the general synthesis of all calorific phenomena. The purpose of this research is to prove the absence of rigorous definition of the word "temperature," the *petitio principii* on which the construc-

tion of all thermometers rests, and the confusion which exists in most of the treatises on physics between the terms: heat, sensible heat, latent heat, and temperature.—M. Arthur Achard described certain considerations relative to the useful effect of magneto-electric machines, and spoke especially on some experiments made by Prof. Hagenbach of Bâle with a Gramme machine (see *Archiv des Sc. Phys. et Nat.*, vol. lxiv. p. 332).—Prof. Schiff spoke of the electric currents observed in animals. Most of them proceed from the glands of the skin, which are veritable electrical apparatus. If we destroy the skin or cauterise it, currents are produced which are attributed to the muscles, but which proceed from the movements of the animal. The observations were made on the protics.

November 21, 1878.—Prof. Soret communicated the results of his researches on absorption spectra by means of the eye, especially with respect to ultra-violet radiations. He operated on the eyes of oxen and calves, and he found that the ultra-violet radiations were transmitted as far as the S line. The aqueous humour allows them to pass as far as V. The vitreous substance has a transparency much greater than the aqueous humour.—M. Raoul Pictet presented several considerations on the passage to a liquid state of compressed gases, and in that state the limit which is produced for each gas at a certain pressure and a certain temperature.

December 5, 1878.—M. J. M. Crafts showed a new thermoscope, a description of which he recently published, and the air-reservoir of which is only $\frac{3}{10}$th of a cubic centimetre. From experiments made with this instrument he concludes that the physical characteristics of different bodies are much less dissimilar at high than at low temperatures.—Prof. Schiff experimented before the Society on a seismograph of M. Ziegler, consisting of a tube of gold-beater's skin, with which he envelopes the member whose variation of volume he wishes to measure, corresponding to the beatings of the pulse. This large tube communicates its impressions to a registering apparatus by means of a very narrow tube.—Prof. Joh. Müller gave an account of very delicate microscopic observations made by him and M. Minks on lichens, and overthrowing definitely the theory of Schwendler (See *Arch. des Sc.* Jan. 1879, p. 49).

PARIS

Academy of Sciences, March 24.—M. Daubrée in the chair.—The following papers were read:—On the slow changes undergone by wine during its conservation, by M. Berthelot. These observations relate to bottles of port 100 and 45 years old respectively, which the author analysed. *Inter alia*, the cane-sugar in the older wine had disappeared almost completely; and there was very little in the other. The wines must have lost more than a fourth of their acidity through etherification. The amount of cream of tartar was much under the normal solubility. The alcohol was in like proportion to that of recent port, there had been little change in it, therefore, probably. As to gases, one litre of the wine contained 12·4 cc. oxygen, and 32·3 cc. nitrogen, without carbonic acid; corresponding to normal saturation of the wine by the gases of air. In saturation with oxygen this old wine contrasts with recent Burgundy wines, which have no trace of it in solution, but which contain CO_2, while the old ports have parted with this through diffusion.—Remarks on some points of crystallogy, by M. Lecoq De Boisbaudran. He makes inferences from the unequal resistances of different faces of a crystal to change of state. The solubility of the crystal must vary with its exterior form. The peculiar mode of regeneration of mutilated crystals is easily explained; also the influence of rapidity of growth on relative development of the different parts of a crystal, &c.—Communications on several geographical questions, by M. de Lesseps. These relate to inter-oceanic canals and a conference on the subject, to be held in Paris on May 15; news from M. Soleillet on the banks of the Niger, and from Serpa Pinto, announcing important discoveries on the course of the Zambesi; news also from M. Roudaire at Gabes.—Addition to a previous note on damming the Tiber at Rome, by M. Dausse.—Observations of Brorsen's periodic comet, by M. Tempel.—Formulæ relating to perturbations of planets, by M. de Gasparis.—On resolving equations, by M. Pellet.—On the solution in whole numbers of the equation $(1) aX^4 + bY^4 + dX^2Y^2 + fX^3Y + gXY^3 = cZ^2$, by M. Desboves.—Molecular vibrations in magnetic metals during passage of undulatory currents in these metals, by M. Ader. Such vibrations are thus had in all the magnetic metals, and give articulate sounds. To have them in all their intensity, a mechanical action must be opposed to the wires or bars, especially the inertia of two heavy masses at their extremities. The effects of these electrodynamic vibrations, and the conditions of mechanical actions to be opposed to the bars are quite the same as those the author has indicated for electro-magnetic molecular vibrations.—On ytterbine, the new earth of M. Marignac, by M. Nilson.—On *scandium*, a new element, by M. Nilson. The preparation of ytterbine (which he describes) furnished a substance with molecular weight 127·6 instead of 131, indicated by M. Marignac; and M. Nilson was led to suspect the presence of another earth, of lower molecular weight, mixed with the product examined. The spectroscope favoured this idea, and M. Thalen indicates the lines proper to the spectrum of the new earth. The name given to the new element is meant to recall its presence in gadolinite or euxenite minerals, found only in the Scandinavian peninsula hitherto. The atomic weight is under 90. M. Nilson remarks on some of its chemical properties.—On the cyanosulphite of potassium, by M. Etard.—Thermochemical study of alkalino-earthy sulphides, by M. Sabatier.—On various alcoholic iodides and bromides, by MM. De Montgolfier and Giraud.—On the formation of aurine, by MM. De Clermont and Frommel.—On the presence of lithine in rocks and in the waters of seas; consequences relative to saliferous strata and to certain classes of mineral waters, by M. Dieulafait. Lithine is as widely distributed as soda and potash, and accompanies these two bases in all rocks of primordial formation. It exists in the Mediterranean and other seas in such quantity that it can be recognised in the residue of evaporation of even one cubic centimetre. It concentrates notably in the sediments of salt marshes. Marls in small quantity give an intense spectrum of lithine. All waters mineralised in the primordial formation contain lithine, and all waters distinctly saline contain it in exceptional proportion.—Resistance of germs of certain organisms to the temperature of 100°; conditions of their development, by M. Chamberland. He describes two forms of *Bacillus*, whose germs have this resistance; boiling water several minutes or even an hour will not kill them.—On the presence in the blood and tissues, under spheroidal form, of certain liquids not miscible in water and which have penetrated through the lungs, by M. Poincaré. Spirit of turpentine and nitrobenzine are among the substances observed to have this effect. The fact bears on respiration of noxious vapours by workmen.—Anatomical and physiological study of nectaries, by M. Bonnier. He rejects Darwin's theory of the *rôle* of nectaries. Nectariferous tissues, floral or extra floral, emitting a liquid or not, form special nutritive reserves in direct relation with the life of the plant.—Experimental researches on the conditions of growth of root hairs, by M. Mer.—On a new disease of the Rubiaceæ of hothouses, by M. Cornu.—On halos and parhelias seen in the park of Saint Maur, by M. Renou.—On the unity of forces in geology, by M. Hermite.

CONTENTS

THURSDAY, APRIL 10, 1879

JOHANNES MUELLER'S CLASSIFICATION OF PASSERES

Johannes Müller on Certain Variations in the Vocal Organs of the Passeres that have hitherto escaped Notice. The Translation by F. Jeffrey Bell, B.A. Edited, with an Appendix, by A. H. Garrod, M.A., F.R.S. (Oxford, at the Clarendon Press, 1878.) 4to. Plates.

MORE than thirty years ago was this celebrated treatise, now translated by Mr. Bell, published, but without attracting any notice in this country. It is true that some twelve months after the author's investigations were first communicated to the Academy of Sciences of Berlin (June 26, 1845), a brief abstract of them appeared in what was then, as it still is, our leading biological magazine (*Ann. Nat. Hist.*, xvii. p. 499), but no one here seems to have thought them worthy of further attention. Indeed, the principal British ornithologists had so long gone astray in pursuit of that will-o'-the-wisp—the "Quinary System," which seemed to have been revealed to the obscure vision of Vigors, and had so completely mystified themselves with hazy speculations concerning circles, types, affinities, and all the jargon of what was so loudly proclaimed to be *the* "Natural Arrangement," that it would have been hopeless to expect them to return to the paths of common sense. Their successors had to make the best of what was before them, and that best was obviously to leave it alone: for they doubtless found, even as we find to day, that all which had been written by the Quinarians was hopelessly unintelligible.[1] Preached, however, as this doctrine constantly was, amid terrific maledictions on all who hesitated to receive the "Circular System" as the orthodox faith, they were content to let its results pass unquestioned, and thus the "Natural" Orders and other groups, which were the invention of Vigors and some of his followers, were silently accepted, and they continued to be adopted by most British ornithologists until very recently, if indeed they have now gone wholly to their rest. There is nothing extraordinary in all this. No disputant is so difficult to overthrow as a mystic, and a mystic your Quinarian certainly was. He could, moreover—and the fact is worthy of note, since mystics are seldom so highly accomplished—write long and smooth sentences, irreproachable as to style or grammar, generally not deficient (allowance being made for certain false premisses) in logical arrangement, sometimes distinctly marked by wit, and always abounding in metaphor. They only lacked a plain meaning. If you pleaded that it was not easy to distinguish the boundaries of the metaphorical and the real, he politely intimated in reply that you were an ass, and deluged you with another torrent of mystic verbiage of the same kind. On raising further objection, your Quinarian began to lose his temper, and, metaphorically shaking "a bunch of fives" (namely, his fist) in your face, discharged at you a volley of well-assorted epithets, about the reality of which there could be no doubt. This is absolutely no exaggeration of some of the characteristics of the Quinarian controversy which may be found in certain publications since 1823, when Vigors unhappily began to apply to Ornithology the senseless fantasies which Macleay had a short time before evolved from the depths of his own imagination. Good work, very good work, was no doubt being done in the meanwhile by some British ornithologists, but the good work was wholly of a limited and special kind. Generalised or broad views were either not taken at all, or, if attempted, were propounded by men of comparatively poor ability, men who were unable to see their way through the baleful fogs that the Quinarian magicians had conjured up around them. It is not too much to say that for some forty years British ornithologists were wandering in a wilderness of words. Temminck's "Manuel d'Ornithologie," the second edition of which was published in 1820, and speedily became well known in England, it is true, kept some, who regarded it as a kind of gospel, from being utterly bewildered by the cloudy dreams of the Quinarians, for Temminck was a simple-minded Dutchman, who had no philosophical or pseudo-philosophical theories to support, no circular visions to relate, and no metaphorical phrases wherewith to encumber his statements. He wrote in French, and if his language appeared to Vieillot not to be the pure French of the Académie Française, it was easily understood by most Englishmen, and he consequently exercised an enormous influence on their mind—an influence which in time produced evil effects, though that is at present no business of ours to show.

During this period of darkness in England there were, however investigators in other lands pursuing what is now obvious to all to have been the right road. Unfortunately their investigations were published so as to be practically inaccessible to our countrymen, and the results at which they arrived were utterly unknown to British ornithologists. Thus we find Strickland, by far the best-informed man of his calling and time, saying, in 1844, that the labours of Wagler[1] and Nitzsch "have not fallen under my inspection." Accustomed as we are in these days to the rapid exchange of publications with our continental brethren, we might regard this at first sight to be a grave shortcoming, but commercial and postal facilities of intercourse with fellow-workers in foreign countries did not exist, and we are prepared to maintain that no very great blame is to be ascribed to British ornithologists of that epoch for not knowing what was being done abroad. The fault lay beyond them. There was first the heavy import duty on foreign books, which pre-

[1] The Quinarian system has so completely dropped out of use, that readers of this generation may be at a loss to find out what it really was. We therefore present them with the following "Symbolum Fidei," drawn up by a very orthodox Quinarian (Mr. Neville Wood), in 1836, in the hope that it will convey clear ideas to them:—"The first and fundamental principle inculcated by Macleay and his disciples is, that all nature moves in a circle, and that the series of beings is unbroken; and, secondly, that each group and each species has a double affinity. Every one of the higher groups has a binary division, viz. the normal or typical, and the aberrant, the former containing two, and the latter three, of the five subdivisions of which each of the higher groups is composed. We cannot here explain the doctrine of *analogy*—which is wholly distinct from affinity—but we can give an instance of it:—the Hedge Dunnock in the *Sylviadæ* represents the House Sparrow in the *Fringillidæ*; that is, the one bears the same relation to the *Sylviadæ* that the other does to the *Fringillidæ*, and *hence* they are said to bear an analogy to each other. The whole zoological series, before arranged in a simple chain, according to this system revolves in an almost infinite number of circles around man, from whom they may be said to degrade on all sides." It is pleasing to observe that a little further on the author states that "no one who supposes the Quinary System, or any part of it, to lead to Atheism, can rightly understand its principles."—*Ornithologist's Text-book*, pp. 30, 31.

[1] Referring probably to his "Natürliches System" of 1830, for his "Systema Avium" of 1827 had long before been reviewed in England (*Zool. Journ.* iii. p. 465).

vented international booksellers from existing, and made it far more difficult to obtain in London a work published in Paris or Leipzig than it now is to get one that has been printed in Chili or Japan. Next—perhaps it should have been placed first—there was the lamentable fact that so defective was English education, that few boys were taught to read a book in any modern language but their own. Enormous time was wasted at school over Greek and Latin, which, owing to the senseless method of teaching, were, as now, scarcely ever taught to any useful purpose. A smattering of French was sometimes picked up by boys when at home for the holidays, but that was all. German was an utterly unknown tongue. All this is of course notorious. To the ordinary English gentleman it mattered nothing, nor did it signify very much to the literary man, but on the man of science its effect was disastrous, and especially was it so to the naturalist. Most of Cuvier's works had been, it is true, translated, so they were open to all, but this was a very exceptional case. Probably no British ornithologist had ever heard of Merrem, Tiedemann, or Meckel; assuredly no British ornithologist was acquainted with their writings. Yarrell, Macgillivray, and Blyth had each made some advance in certain directions, and the last two were unquestionably not fettered by Quinarian bonds, but their advance was rather that of scouts than that of permanent occupiers. Later came Nitzsch, Dr. Cabanis, and the illustrious author of the work under review—in Germany, and—in Sweden, Sundevall; but still no effect was produced on our insular mind.

It was Nitzsch who first began the great work of critically examining the Linnæan Orders, *Passeres* and *Picæ*—the very names of which had passed out of use and were well nigh forgotten in this country, being superseded by the term *Insessores*. In his anatomical contributions to Naumann's excellent "Vögel Deutschland's," a work still far from being appreciated in England, in his treatise on the Carotid Artery of Birds—which unfortunately yet remains in the obscurity of its original Latin, and much more completely in his "Pterylographie"—edited after his death by Dr. Burmeister, and only translated into English for the Ray Society in 1867—the most important structural differences and affinities between the various forms so classed by Linnæus were clearly shown. The Order *Passeres* (or *Passerinæ* as Nitzsch called it) was revised and reconstructed, some genera being added and others excluded, while a majority of the Linnæan *Picæ* became the *Picariæ* of Nitzsch—a very heterogeneous assemblage it must be allowed—the old name being unsuitable, since the genus *Pica* was found to be truly Passerine. But Nitzsch had the opportunity of dissecting but few if any of the New-World forms, and consequently he did not know that many American *Passeres* differed essentially from those of the Old World in the structure of their vocal organs. This fact it seems was first ascertained by Macgillivray,[1] but he did not see its importance, which was really recognised by Müller, and the latter's discovery was the cause of the treatise now translated for us after so many years by Mr. Bell, and edited by Mr. Garrod.

[1] Müller in the work under review (Transl., pp. 5 and 6) makes the mistake, which his translator or editor might, we think, have corrected, of attributing the anatomical portions of the "Ornithological Biography" to Audubon! They are admittedly by Macgillivray, who is known to have also helped largely in the composition of that work.

Though ornithologists have by no means followed up Müller's investigations as they deserved, the period that has elapsed since their publication has not been altogether idly passed, and Mr. Garrod has enhanced the value of his coadjutor's translation by adding thereto an appendix bringing the knowledge of the subject almost "up to date," and incorporating the results of his own labours thereon. Müller, however, was no more free from error than his predecessors had been. He divided his "*Passerinen*"—to which he applied the Vigorsian title of *Insessores*—into three tribes:—(1) the *Oscines* or *Polymyodi*, "having the lower larynx formed partly by the trachea and partly by the bronchi, and possessing five or six pairs of muscles attached to the end of certain of the bronchial rings"; (2) the *Tracheophones*, "with the lower larynx formed exclusively by a modification of the lower part of the trachea"; and (3) the *Picarii*, "with the larynx either partly tracheal and partly bronchial, or wholly bronchial and with not more than three pairs of muscles."[1] The *Picarii* of Müller, however, form a group not quite commensurate with the *Picariæ* of Nitszch, and this is a point to which attention should be directed, as, owing to the very slight difference between the two names, one has been frequently written for the other, and the two groups deemed to be identical. Nitzsch very properly excluded what are now known as the *Tyrannidæ* from his *Picariæ*, while Müller, as improperly, included them among his *Picarii*. Both authors also erred in their conception of the family *Ampelidæ*, which, in the sense in which it is used by them comprises two very distinct groups, the *Ampelidæ* proper and the *Cotingidæ*. Nitzsch, whose experience had lain with the single European representative of the former, placed the family among his *Passeres*, while Müller, judging it would appear from the New-World genera, which are now more rightly held to compose the *Cotingidæ*, referred the family to his *Picarii*. It is nowadays abundantly clear that the true *Ampelidæ* are very normal *Passeres*, while the *Cotingidæ* are not *Passeres* in the most restricted sense. But it is impossible for us here to go into details. Mr. Garrod's appendix will show how and to what good purpose he, with the abundant opportunities he has enjoyed, has followed Müller's line of research, and has greatly extended it. We certainly wish he had more explicitly set forth, in a tabular form for instance, the general results of his continuous investigations. The want of some such summary is the only serious complaint we have to make against this book; and, regretting as we do its absence, we think we can perceive what has possibly been the motive of his abstention—his consciousness that there is yet much more to be done, that few conclusions drawn at present can be otherwise than general, and that fewer still can be final. On one important point, however, he corroborates what we imagine to have been a singularly interesting discovery of Prof. Huxley's, namely, the divergence of *Menura* (the Lyre-bird) from almost all the other *Passeres*, its only relative (and the relationship can hardly be very close) being *Atrichia*.

Our sincere thanks, and those of every English-speaking ornithologist, are due to all concerned in this book—to Mr. Sclater, whose influence with the Clarendon Press

[1] These definitions are taken from Prof. Huxley (*Proc. Zool. Soc.*, 1867, p. 471), being expressed with his usual admirable terseness.

Delegacy caused it to be undertaken; to Mr. Bell, who seems to have very efficiently performed the actual task of translation;[1] to Mr. Garrod for the Appendix already mentioned; and last, though not least, to Prof. Peters for supplying the use of the very plates which illustrated Müller's work.

OUR BOOK SHELF

Proceedings of the London Mathematical Society, vol. ix. (November, 1877, to November, 1878.) 279 pp. (Hodgson and Son, Gough Square, 1879.)

WE have, in previous notices, indicated the character of the papers contained in former volumes, and the same remarks apply equally well to the volume before us. We shall content ourselves, in our present notice, with giving the titles and author's names only of the more important papers.

Prof. Cayley, not so large a contributor as usual, furnishes a short paper "On the Geometrical Representation of Imaginary Quantities, and the Real (m, n) Correspondence of Two Planes," and another equally short, "On the Theory of Groups." There are brief notes "On a Generalised Form of Certain Series," by Mr. Glaisher; "On the Transformation of Elliptic Functions," by Dr. Klein, of Munich; "On Certain Extensions of Frullani's Theorem," by Mr. C. Leudesdorf; "The Flexure of Spaces," by Mr. C. J. Monro; "On the Relation between the Functions of Laplace and Bessel," by Lord Rayleigh; "Notes on Normals," and "The Decomposition of Certain Numbers into Sums of Two Square Integers by Continued Fractions," by Mr. S. Roberts. Longer papers are: "On the Singularities of the Modular Equations and Curves," by Prof. H. J. S. Smith; "On Partial Differential Equations with Several Dependent Variables," and "On a General Method of Solving Partial Differential Equations," by Prof. Lloyd Tanner; "A Method in the Analysis of Plane Curves," by Mr. J. J. Walker; "On Conjugate Four-piece Linkages," by Mr. A. B. Kempe; and "A New Method of finding Differential Resolvents of Algebraical Equations," by Mr. R. Rawson. M. Halphen contributes a long and valuable paper on "The Characteristics of Systems of Conics."

Physical papers are "On the Electrical Capacity of a Long Narrow Cylinder, and of a Disk of Sensible Thickness," by Prof. J. Clerk Maxwell; "On the Conditions for Steady Motion of a Fluid," by Prof. Lamb; "Notes on the Solution of Statical Problems connected with Linkages and other Plane Mechanisms," by Prof. A. B. W. Kennedy; "On the Astatic Conditions of a Body acted on by given Forces," by Prof. Minchin; and "Progressive Waves," by Lord Rayleigh. Mr. H. McColl contributes a paper in two parts bearing on logic and probabilities, viz., "The Calculus of Equivalent Statements."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Brorsen's Comet

I OBSERVED Brorsen's comet, about 8h. on March 29, through a whitish haze that extinguished the small stars near it. It appeared about the seventh magnitude, by estimation 3′ in diameter, its light very much condensed in the centre and approximately circular.

On April 4 I obtained the following places, although the moonlight much diminished its brilliancy:—

1874.		G.M.T. h. m. s.		App. R.A. h. m. s.		App. decl. ° ′ ″
April 4	...	8 29 59	...	2 56 53·7	...	+ 25 42 36
,,	...	8 38 31	...	2 56 55·3	...	+ 25 42 56

At the first observation the comet was compared with the star Arg. + 25°, No. 485; at the second with Arg. + 25°, No. 496 (Bonn Obs., vol. vi.). The observations admit of more accurate reduction.

The correction to the ephemeris of Herr Schulze (reproduced in NATURE, vol. xix. p. 510) is—

In R.A. ... ... − 5ˢ·0 | In decl. ... ... − 31′ 20″

On both evenings when the equatorial was set to the position given in the ephemeris (with Strasser's correction, A.N. 2250) the comet was not in the field of a low power. The above correction to the declination may prevent loss of time in finding the comet.

G. L. TUPMAN

1, Vanbrugh Park, Blackheath, April 6

Madagascar Forms in Africa

A PARAGRAPH in a recent number of NATURE (p. 470) mentions the discovery of a new species of *Ouvirandra* in Eastern Africa, the genus being hitherto supposed to be peculiar to Madagascar. The plant in question, which was collected by Dr. Hildebrandt, is, however, as has been pointed out by Dr. Trimen and myself (*Gardeners' Chronicle*, February 1, p. 149), not a species of *Ouvirandra*, being destitute of the fenestrated leaves, which is the only distinguishing character of that not very sound genus. It is, in fact, a well-known and widely-distributed African plant, *Aponogeton leptostachyus*, E. Mey. Dr. Hildebrandt, when lately in this country, fully assented to this identification.

A more novel fact in the same connection is the discovery by my colleague, Prof. Oliver, of a new monimiaceous plant amongst the collections of Gustav Mann in East Tropical Africa. The order itself, though represented in Tropical America and Asia, has hitherto been unknown in Africa, although the Mascarene Archipelago is well supplied with species, and one at least is known from the Comoro Islands, whence its remarkable fruit was sent to the Kew Museum by Dr. Kirk.

W. T. THISELTON DYER

Transportation of Seeds

IN NATURE, vol. xvii. p. 390, which through the carelessness of my agent has only just reached my hands (together with the numbers for August, September, October, and part of November), I see Mr. Francis Darwin notices the penetration of certain grass seeds through the skin of sheep. It may interest him and your readers to know that I can corroborate this from what I have witnessed here and at the Cape of Good Hope.

In passing a butcher's shop in Noumea, lately, I was struck with the appearance of a fore-quarter of mutton. On a closer examination I found it so full of grass-seeds that it resembled a ham just unpacked from its bag of chaff. Many of the seeds had still their long thin tails drawn through the flesh like threads interlacing each other in every direction. On questioning the butcher, he said they rarely killed a sheep that was not more or less punctured.

All our sheep are imported for slaughter from Australia or Norfolk Island. This particular one came from the former place.

At the Cape of Good Hope I have skinned "spring-bucks," in which the shanks were pierced through and through with these "awms" and small thorns. My wonder has been how the animals could endure the pain of moving, but I suppose they do not suffer as we do.

E. L. LAYARD

British Consulate, Noumea, New Caledonia, February 1

Rayons de Crépuscule

WHAT does Mr. Abbay call (vol. xviii. p. 540), the "low country" in Ceylon? If he means the sea-board generally, I

[1] We might take exception, perhaps, to his rendering of the title, which, we think, might have been more literally and better expressed by "On the Hitherto Unknown Diversities of Type in the Singing Organs," &c.

can assure him that, while magistrate at Point Pedro, five-and-twenty years ago, I used almost nightly to see "rayons de crépuscule" in the most glorious perfection for months together. Point Pedro is the extreme northern point of the island, with a splendid sea horizon. I shall never forget the beauty of the tints.

E. L. LAYARD

British Consulate, Noumea, New Caledonia, February 1

Salmon in Rivers of the Pacific Slope

IN a notice of the Report of the U.S. Commissioner of Fish and Fisheries, in NATURE, vol. xix. p. 430, the reviewer refers to the statement in a "memorandum respecting the American salmon and white fish recently introduced in New Zealand by Dr. James Hector," that "so far as yet observed, the adult fish all die after spawning and never return to the sea." The reviewer writes: "We shall be glad to have some authoritative statement with regard to the above fact, as without some explanation it seems too extraordinary for belief."

Dr. Hector in the above remark refers to the so-called "Californian Salmon" (*Salmo quinnat*) when in its native waters. There must be some exceptions to the rule as above stated, for Mr. Livingstone Stone, in his evidence in the same volume (p. 806), testifies that in the Columbia full-grown fish of this species are caught in considerable numbers, nearly exhausted, on the *back* of the drift-nets, in July and August, but it is nevertheless almost strictly true.

In the Fraser River, British Columbia, the general opinion is that the salmon never return to the sea except accidentally in a dying state. The Indians, who are generally well informed on such points, affirm this. The late J. K. Lord, in his "Naturalist in British Columbia," (vol. i. p. 40, *et seq.*), is very clear on the same point. It is, moreover, almost certain from the tumultuously rapid character of the Fraser, that the salmon hatched in its upper waters—in some instances 600 miles from the sea—never return there till mature and ready to spawn, and that this act is their last. They show no disposition to attempt to go back to the sea. I have seen them in great numbers in small streams tributary to the North Thompson, in August, spent, their silvery colour turned to a livid red (with the exception of the fins and tail, which are darker) but still heading persistently up stream, and continuing to do so, till from sheer weakness the current carried them away. In fording the brooks, the disturbance of the water causes those possessing sufficient vitality to scatter in all directions, but interrupts only for a moment their dogged struggle. At this season, in most years, dead salmon in great numbers are found floating down the stream, or stranded on the bars and banks of the river.

In Okanagan, Shuswap, and other lakes, there is a smaller fish, which may be a "land-locked" salmon, but of which I was not able to preserve specimens. The Indians say that it does not come from the sea, but lives in the deep waters of the lake, till in August it enters certain streams to spawn. Like the salmon it becomes, when spent, first blotched with pale red, and eventually altogether of that colour and without silvery lustre, the flesh at the same time losing its pink tint. It possesses the same instinct of struggling against the stream till it dies. I have seen them in brooks within a stone's throw of the lake, endeavouring with their remaining strength to keep themselves from being carried back into it.

Lord makes an exception of the "fall" or "dog-tooth" salmon (*S. lycaodon*), of which he supposes some go back to the sea, and return to the rivers in following years. It remains, however, an undoubted fact, that by far the greater part of the prodigious number of salmon entering the Fraser every year, perish. The fish appears to refuse food, and is not caught in the river by bait or fly, though frequently taken by trolling with fish or spoon-bait in the salt water.

It is much to be desired that a systematic investigation of the species of salmon frequenting the Fraser and other rivers of British Columbia should be made, embracing their habits and the course of their migrations. The subject is an interesting, but very intricate one.

GEORGE M. DAWSON

Geological Survey of Canada, Montreal, March 27

The Marsupials of Australia

THE peculiarities of the structure of the marsupials of Australia are so remarkable and their habits are so unlike those of the placentals of the Old World, that probably no apology is needed for venturing to lay before your readers a short account of one of these peculiarities possessed by certain genera, which I believe has escaped the observation of most naturalists, and may prove interesting to some of your subscribers.

The inferior maxillary or lower jaw-bone of almost all known mammals consists of two bones united together with more or less rigidity by a strong cartilage, which allows no play or independent movement whatever, and which practically firmly unites them into one bone.

The formation of the inferior maxillary of the Macropidæ, or kangaroos, is an exception, however, to this rule; instead of being united by a cartilage, the two rami of the lower jaw are jointed at their point of contact with a hinge somewhat resembling that upon which the two shells of a bivalve move, that is, upon corrugations which project from the two edges and fit accurately into one another.

These two rami extend a short distance beyond the point of contact, and into their terminations are fixed two long procumbent incisor teeth, the only two incisors possessed by this family in the lower jaw. Immediately in front of this joint, that is, at the root of the procumbent incisors, a circular muscle embraces the two rami of the jaw, the contraction of which has the effect of bringing the inner edges of the procumbent teeth together; upon its relaxation or the contraction of another set of muscles, placed probably at the extremities of the rami, where they hinge upon the facial bones, the incisors are separated the extreme distance allowed them by the ligaments around the joint. The action of separating the teeth is probably connected in some measure with the action of opening the jaws, as I not unfrequently found that when the mouth was widely opened, the teeth themselves became separated.

The muscular action of uniting the incisors may be said to be exemplified in the case of a pair of shears when the blades are closed by a grasp of the hand, and the force is applied between the fulcrum and the point of resistance.

In Prof. Owen's work upon the "Anatomy of the Vertebrates," the following passage appears, showing that he was aware of a certain looseness of connection of the two rami, but probably not aware of the completeness of the construction with its separate functions. After certain references to the wombat he says, "In other marsupials the rami of the lower jaw are less firmly united at the symphysis; they permit independent movements of the right and left incisors in the kangaroos, and in the opossum both the rami of the lower jaw and all the bones of the face are remarkable for the loose nature of their connections."

In the work upon "Odontography" by the same distinguished writer, various references are made to the lower incisors of the macropidæ, but his readers are in every instance led to believe that their trenchant margin is their outer edge, and I believe it has escaped his observation altogether, that the inner margin where the two teeth come in contact has the principal cutting edge.

Mr. G. R. Waterhouse was aware of the inner trenchant margin as in his "Natural History of the Mammalia," he refers to these incisors as having "cutting external and internal margins." Further on he says—"In *Macropus major* (and, perhaps, in some nearly allied species), the rami of the lower jaw are loosely attached at the chin, and at the apex they are free, and the animal has the power of slightly separating the lower incisors, so that their outer cutting edges are brought more closely in contact with the upper incisors than they otherwise would be." Were this, however, the only utility of the loose attachment at the symphysis, what function has the cutting inner margin to perform?

An examination of those incisors will disclose the following facts:—

If the jaw of one of the macropidæ is examined immediately after death, when the muscles are relaxed, it is found that the smallest pressure upon the base of the rami suffices to open the lower incisors to the extent of about one-fourth of an inch in larger specimens, and about one-eighth in the smaller Pademelons or Halmaturi. The inner edges of the procumbent teeth will then be seen to be sharp, but strongly supported by a considerable thickness of enamel immediately in rear of the edge, and when the teeth are united by the contraction of the muscles, they fit so perfectly throughout their whole length that they will grasp a hair at any point between the base and the apex. On the other hand, the outer margins of these teeth are blunt and somewhat rounded, and when the jaw is closed and at rest, instead of fitting on to the teeth of the upper jaw, as represented in diagrams in Owen's "Odontography," the

two procumbent teeth rest upon a pad or projecting palate which rises from the inner base of the upper incisors, and whose surface is nearly upon the level of the edges of the upper teeth themselves; the lower incisors, therefore, are only brought into contact with the upper incisors by protruding the jaw forward.

I have, moreover, examined many specimens of the *Macropus major*, or kangaroo, and of varieties of the Halmaturus known as wallabies and pademelons, when they have been mortally wounded and under the influence of the spasmodic muscular contractions which occur at the point of death, and I have repeatedly found that they will alternately open the two incisors to their full extent, and unite them again with the energy which characterises all the muscular movements of an animal in its death-struggle.

If a small object, for instance the blade of a knife, is inserted between the teeth when fully extended, the animal will immediately grasp it with its incisors, which he will do without closing the jaw, showing that the movement is not absolutely dependent upon the action of closing the jaws, although, as I have said above, I believe it usually accompanies it.

The Phalangists or Australian opossums closely resemble the macropidæ in their dental formation, but they possess partially-developed canines in the upper jaw, whilst the latter have none in either jaw except in very early life; but although these opossums have their two procumbent incisors similarly situated, they probably do not possess the power of utilising them in the same manner; I have examined some specimens, but have failed so far to find more than the looseness of connection at the symphysis referred to by Prof. Owen.

In the genus which is represented by the Phascolarctos or native bear of Australia, which possesses the same lower incisors but distinct canines in the upper jaw, this arrangement is certainly wanting, as the rami of the lower jaw are firmly united.

This remarkable formation of the lower jaw of these kangaroos and wallabies is possibly an interesting instance of the retention of a construction, and of a set of muscles in a class of animals which have constantly required their aid to sustain life, which in other families of the animal kingdom have become rigid by ossification and cartilaginous formations, and by atrophy of the muscles in consequence of disuse.

The great plains and deserts over which these marsupials wander in search of food afford an exceedingly precarious supply of pasture in consequence of droughts and bushfires, which not unfrequently follow a superabundance of herbage. These animals, by means of their procumbent teeth which they make use of as shears, are thus enabled to cut off any green shoots or half-buried remains spared by a scorching sun, and obtain nourishment where any grass-feeding placental would certainly starve.

It is in consequence, I believe, of the power which is by this means given to these marsupials of eating scanty pasturage closer to the ground than any other animal, that in the great pastoral districts of New South Wales and Queensland it has been found that they are far more destructive of food than any stock that can be put upon the land, and in places where wallabies and pademelons are exceedingly numerous, it is noticeable that the native grasses in the particular localities which they frequent become completely destroyed, and that such places remain ungrassed until fresh seed is scattered over them by the winds.

HENRY WELD BLUNDELL

Gordon Downs, Queensland, December 5, 1878

Measuring the Velocity of Sound in Air

THE following simple way of arriving at the velocity of sound in air occurred to me lately:—Standing on a straight staircase between two blank walls (brick, and papered), which I find to be $32\frac{1}{2}$ inches apart, I clap my hands. The effect from each clap is a brief musical sound, metallic in character, and of quite appreciable pitch. It arises, doubtless, from the disturbance travelling to and fro between the walls. The pitch I find to be, as nearly as possible, G sharp (in the fourth space). Now, the number of *complete* vibrations per second, corresponding to this note, seems to be about 205 (see Deschanel's "Natural Philosophy," p. 820). This implies that the disturbance, when I clapped my hands, made 410 excursions across the space per second. Consequently, $410 \times 32\frac{1}{2} = 13,325$ inches = 1,100 feet. This is exactly the number Deschanel gives as the velocity of sound in air at 50° (approximately our mean annual temperature).

M.

Snow Flakes

WHILST walking home on March 26, about one in the morning, snow began to fall very gently; but instead of the usual powdery or feathery appearance, each flake consisted of a distinct plate, in some cases perfect six-pointed crystals. I measured some of them, and the largest were as much as five-eighths of an inch across. On taking up a handful the appearance was still more remarkable; instead of the white opaque body one usually sees, the mass was pearly and semi-transparent, and so strongly resembling boracic acid, that I should have had some difficulty in distinguishing a handful of each substance by sight alone.

Near the lamps the effect was very beautiful, more especially when the road became covered, luminous points appearing in all directions, which scintillated like stars as one walked along, whilst many of the falling crystals reflected iridescent hues on nearing the ground.

When out of the town I ignited a piece of magnesium wire, and the effect was most brilliant.

It was a cold, dull night, barometer falling.

Burton-on-Trent FRANK E. LOTT

Rats and Water Casks

IN 1840, in a voyage from Sydney, *viâ* Madras, to London, about three weeks after leaving the latter, it was found that a number of water-butts, on their heads in the between-decks, were leaking. On examining them we ascertained that as many as ten or twelve butts had been perforated by rats; three or four were entirely empty from the leakage so caused, while the remainder contained ullages from about half to a few gallons. In every case the stave had been eaten through just above the chime hoop, and those which had been apparently most recently operated on had only been perforated so as to cause a slight weeping, while the empty ones showed an opening as large as an ordinary vent-peg hole. The rest of the voyage a tub placed in the square of the main hatchway was kept constantly supplied with water, besides one or more square tins of water on the main deck.

In the above voyage we stayed a week in Madras, and in loosing the foretop-gallant-sail on leaving, a rat and five or six young ones fell to the deck; and the sail was found to be so much eaten and full of holes, made to form and line the nest, that the sail had to be unbent and replaced.

Gurnet Bay, March 31 E. J. A'COURT SMITH

P.S.—The ship was the *Cornwall*, East Indiaman, Capt. Cow.

HEINRICH WILHELM DOVE

PROF. HEINRICH WILHELM DOVE was born at Liegnitz, Silesia, on October 6, 1803, and at the age of eighteen passed from the schools of that town to the Universities of Breslau and Berlin, where for the next three years he devoted himself assiduously to the study of mathematics and physics. In 1826 he took his degree of Doctor of Philosophy, his thesis on the occasion being an inquiry regarding barometric changes; and it is further significant of his future life-work that his first published memoir was a paper on certain meteorological inquiries relative to winds, these two subjects holding a first place in the great problem of weather-changes.

Dove began his public life as tutor and Professor at Königsberg, where he remained till 1829, being then invited to Berlin as supplementary Professor of Physics. His strikingly clear-sighted, bold, and original intellect turned instinctively to that intricate group of questions in the domain of physics which comprise the science of meteorology, and his success in these fields as an original explorer was so marked and rapid that he soon achieved for himself a seat in the Royal Academy of Sciences, and some time thereafter was raised to the distinguished position of the Chair of Physics in the University of Berlin.

Among the scientific and fashionable circles of Berlin he took first rank as a lecturer, the combined qualities of accurate science, fine imagination, lucidity of style, com-

manding presence, and the extent over which his utterances were heard, marking him out as the Arago and Brewster of Germany. Germany showered on him in profusion those honours and offices which it gracefully and gratefully bestows on learning and science; and perhaps there is no learned or scientific society of any note that has not the name of Dove enrolled among its honorary members. After a protracted and hopeless illness he died on Sunday last, April 6, in the seventy-sixth year of his age.

In the Royal Society's Catalogue of scientific papers, the lists under Dove specify 234 memoirs written between the years 1827-73. These show him to have been a successful worker and investigator in electricity, optics, crystallography, and in such practical matters as measures and the art of measuring, or the metric system of civilised nations. But it was to meteorological inquiries he devoted his full strength and all the powers of his mind, and, by his herculean but well-directed labours he has written his name in large imperishable characters on the records of science.

His fame rests on the successful inquiries he carried out with a view to the discovery of the laws regulating atmospheric phenomena which apparently are under no law whatever. The work he will be long best known by is his isothermals and isabnormals of temperature for the globe, in which work one cannot sufficiently admire the breadth of view which sustained and animated him as an explorer during the long toilsome years spent in its preparation. Equally characterised by breadth of view, and what really seemed a love for the drudgery of detail even to profuseness when such drudgery appeared necessary or desirable in attaining his object, are his various works on winds, the manner of their veering and their relations to atmospheric pressure, temperature, humidity, and rainfall, and the important bearings of the results on the climatologies of the globe; on storms and their connections with the general circulation of the atmosphere; the influence of the variations of temperature on the development of plants; and the cold weather of May—to which may be added the valuable system of meteorological observations he gradually organised for Germany, and the many full discussions of these which he published from year to year.

It is no small praise to pass on his work to say that those views he propounded, which subsequent researches are likely to modify materially, are those he arrived at by methods of investigation necessarily defective at the time. Thus, for instance, in inquiring into the law of storms, it was not in his power to work from isobaric charts, seeing that the errors of the barometer and their heights above the sea were known in but few cases. When we consider the condition in which he found man's knowledge of weather and the large accessions and developments it received from his hand, the breadth of his views on all matters connected with the science and the well-directed patience, rising into high genius, with which his meteorological researches were pursued, there can be only one opinion, that these give Dove claims, which no other meteorologist can compete with, to be styled "the Father of Meteorology."

THE INSTITUTION OF NAVAL ARCHITECTS

THE twentieth session of the Institution of Naval Architects has now been brought to a close. The meeting, with Lord Hampton in the chair, was held at the house of the Society of Arts, John Street, Adelphi, and was well attended throughout. One of the latest developments in ship-building is shown in the paper on "The Structural Arrangements and Proportions of H.M.S. *Iris*," by W. H. White, Assistant-Constructor of the Navy. The construction of the *Iris* marks a new era in the British Navy, being the first vessel built wholly of steel; she is an unarmoured dispatch vessel, specially designed for high speed and great coal endurance. Her principal dimensions are: length between perpendiculars, 300 feet, breadth, extreme, 46 feet, mean load draught, 19 feet 9 inches, displacement, 3,735 tons.

Special attention has been paid to resistance to torpedo attack by constructing the hold in twenty-one separate compartments and the double bottom and bunkers in forty; with the additional weight thus introduced, it is still found that there is a saving of weight in the hull by the use throughout of steel amounting to 12 per cent., or 175 tons. The engines take 28 per cent of the displacement, and 20 per cent. is available for coal, which is estimated to be sufficient for steaming 7,000 knots at a speed of 10 knots per hour. The speed attained by the *Iris* on the measured mile was 18·6 knots, with an expenditure of 2·3 indicated horse-power per ton of displacement as compared with 14 indicated horse-power, required by a torpedo vessel.

In striking contrast with the *Iris* we have the monster proposed by Rear-Admiral J. H. Selwyn in his paper "On the most Powerful Ironclad." The author of the paper has long advocated some modification of the circular ironclad first proposed by Mr. Elder some years ago, and carried out with some alterations by Admiral Popoff. The vessel here proposed is 370 feet in length 220 feet in breadth, with a draught forward of 18 feet and aft 13 feet. Her armament is to consist of twenty 80-ton guns, or eight 100-ton and eight 80-ton; these are to be mounted in two gun-pits on the Moncrieff hydro-pneumatic principle. The guns are carried on a turn-table of the full size of each gun-pit, the floors of which are composed of steel bars set on edge to provide for ventilation, but to keep out shell fragments; the breast-work round each will consist of 30 inches of armour-plating. The guns would be raised by hydraulic power to fire over the breast-work, recoiling automatically under cover for re-loading. The vessel would be protected with a belt of 30-inch armour round the water-line, and a thickness of 25 feet of coals stowed inside it. There would be two Perkins hydraulic engines of 21,000 horse-power for propulsion and steering, and these would be at once available for keeping the vessel afloat in case of a leak. The author estimates that if a hole 10 feet square were made by a torpedo, the engines would be able to keep the water under, while danger of sinking by such damage is much lessened, if the engines are partially disabled, by the large number of water-tight compartments. It cannot be denied that the Russian Popoffkas have been far from successful, especially in facility of steerage, which was one of the main advantages claimed for them, but it can only be determined by an actual experiment whether our naval authorities can overcome the difficulties in speed and steering which have baffled the Russian Admiralty. Even if a vessel as here proposed could not be made sea-going, or to attain a 16-knots speed as claimed, she would at least be more valuable as a harbour defence than a Spithead or Plymouth breakwater fort, and could be adapted to some sites at a less cost in proportion to the weight of armament.

"Armour for ships" by Mr. Barnaby, C.B., Director of Naval Construction, consists of a general review of the progress of armour-plating from its introduction in 1854 down to the present time. The description of the steel turret-plates manufactured by Messrs. Schneider at Creuzot, 32 inches thick, and weighing 65 tons, is not without significance in the present state of depression in the iron trade of this country, but some consolation is to be obtained from the account given of the steel-faced plates of Messrs. Brown and Cammell which shows that some progress is still being made nearer home. The paper by Admiral Sir R. Spencer Robinson, K.C.B.,

F.R.S., gives an exhaustive statement of the experiments that have been made on various targets at Shoeburyness, representing the armour of different ships. A table is given showing the displacement, thickness of armour, and proportion of the former to the latter in ships of different types; this ratio varies from 6·38 in the *Warrior*, 4·00 in the *Alexandra*, to 2·95 in the *Dreadnought*, and 2·50 in the *Glatton;* thus the last may be considered the most heavily-armoured vessel in proportion to size in the navy. The penetration of shot of different diameters and weights with various velocities is given, and the experiments show that it is proportional to the energy of the shot on impact whether due more to velocity or weight, and inversely proportional to diameter of shot; also that the resistance of solid plates is proportional to the square of their thickness. The resistance of composite targets is treated at some length, and a comparison drawn between the various forms adopted in existing ships and the Millwall shield designed by Mr. Hughes, in which the latter is shown to be preferable; but the questions of steel and steel-faced armour which are now attracting the attention of artillerists are not gone into, and are only referred to with the evident feeling that the end of the battle between guns and armour has not yet come.

The paper "On the Resistance given to Screw-Ships by the Action of the Screw-Propeller, and how to Remedy it," by Robert Griffiths, points out an important difficulty in screw-propulsion which has only recently been recognised. A screw-propeller obtains the resistance to drive the ship forward by accelerating the velocity of the currents of water flowing past the stern of the vessel; as in different parts of the screw's disk these currents are encountered at different velocities, the resistance to a blade is not uniform throughout a revolution. In experiments made at Devonport by towing a screw-pinnace, it was found that the water flowed through the lower half of the screw disk nearly at the speed at which the boat was towed, but in the upper half it was so dragged by the boat as to flow past the screw at only half that speed. In dynamometer diagrams, taken with H.M.S. *Rattler*, it was shown that the thrust of the screw varied from 2·9 to 4·1 tons in each revolution. The increase in the resistance of the ship, due to the working of the screw above that due to the ship herself when towed at the same speed, and which Mr. Froude has shown to be 40 or 50 per cent., is considerably greater when the upper currents are more accelerated than it would be if the acceleration were uniformly given to the whole column of water passed through by the screw disk. The author proposes a screw-propeller so constructed that the blades always meet with equal resistance. The blades are so made that more than half their surface is aft of the centre line, so that the pressure on their surface tends to lessen the pitch; they are also made movable in the boss, but so connected that by decreasing the pitch of one, that of the other is increased; when, therefore, one blade meets with more resistance than the other, the increased pressure causes it to turn and throw some of the work on the other.

In his paper on naval guns, Mr. C. W. Merrifield vigorously attacks the Woolwich type of gun, pointing out the disadvantages and absolute futility of the increasing twist in rifling at present adopted. It is now four or five years since this was first done by Prof. Osborne Reynolds, and, aided by the *Thunderer* explosion, it is to be hoped that the time is drawing near when the subject will receive the consideration of the War Department. The author also lays great stress on the advantages of breech-loaders over muzzle-loaders, regarding the latter now, with its complication of gear and fittings, as inferior to the former, even in the simplicity always claimed for it.

Amongst other papers read at the meeting are the following:—"On Sir William Thomson's Navigational Sounding Machine," by P. M. Swan, in which the accuracy of this now well-known apparatus is amply testified by a large number of observations; and a paper by Mr. J. Scott Russell, F.R.S., "On the true Nature of the Wave of Translation, and the Part it plays in Removing the Water out of the Way of a Ship with least Resistance."

OUR ASTRONOMICAL COLUMN

NOTE ON 72 OPHIUCHI (O. Σ. 342).—The publication of the entire series of observations of this suspected double star, made at Pulkowa to 1876, does not lessen the difficulty of arriving at a definite conclusion as to its duplicity or otherwise. On November 1, 1841, it was noted double magnitudes 4 and 7 on Struve's scale, and, no doubt attached to the observation; on May 14, 1842, it appeared single, but at the epoch 1842·72 it was again double, the measures giving for position, 156°·6, and distance, 1″·3. Subsequent observations gave the following results:—

1844·85	...	Single, or with only a suspicion of elongation at 63°; images excellent.
1845·62	...	With very good images; no companion seen.
1846·49	...	Single, or perhaps slightly wedged at 87°.
1847·50	...	Pos. 162°·4, dist. 1″·61, but there was a doubt if the object observed was not an optical illusion.
1847·70	...	Pos. 168°·1, dist. 1″·6. M. Struve says: "I feel sure of the duplicity, but the images are not very good."
1848·79, 1850·50, 1851·51	...	Single.
1851·67	...	Pos. 166°·3, dist. 1″·49. After the observation a note was added—"This is only an optical deception."
1852·63	...	Single; under excellent atmospheric conditions.

This last observation appearing decisive, M. Struve considered that 72 Ophiuchi should be omitted from the list of double-stars, and in the following years only examined it once (1859·66), when it was again single under very favourable conditions. But in 1876 he found reason to modify his view: at 1876·67 the satellite was seen very distinctly, with position 156°·0, distance 1″·60; a fortnight later there were only very slight impressions of a satellite, and M. Struve remarked that the principal star of 70 Ophiuchi presented an analogous phenomenon, though less distinctly. Hence arose the suspicion that the said impressions were caused by accidental conditions of the air and the instrument. Nevertheless, on considering the preceding observations and the fact of their being made without the least recollection of anterior ones, M. Struve thinks their approximate agreement cannot be attributed to chance, and that we are necessarily led to infer that the star is really double, but the companion undergoes considerable and rapid variation of brightness. It is worthy of note that only three weeks before the Pulkowa observation of 1859, when the star was pronounced single, Secchi had recorded of it: "Certainly double, and well separated," his measures giving the position 3°·75, distance 0″·61.

THE VARIABLE STAR χ CYGNI.—According to the later observations of Dr. Julius Schmidt at Athens, it is probable that the next maximum may occur on or about April 25, and the next minimum about December 14. At the last observed maximum on March 14, 1878, the star was hardly a fifth magnitude, which is about the mean brightness in that phase, the extreme limits of variation being two magnitudes or 4m.—6m. according to Prof. Schönfeld; at minimum it descends to 13m. No formula has yet been deduced which will represent satisfactorily the totality of the observations, commencing with those of Kirch the discoverer in 1686; considerable

irregularities following no law so far discovered occasionally presenting themselves. This is particularly evident if we compare Argelander's last formula in vol. vii. of the Bonn observations with the observed times of maxima during the last fifteen years. The place of the true χ Cygni of Bayer, which is the variable, is, for 1880·0, in R A. 19h. 45m. 57·3s., N.P.D. 57° 23′ 18″; it therefore follows the star to which Flamsteed attached this letter, 4m. 4s., and is south of it 50′·6; Flamsteed's star ought to be called by his number, 17 Cygni. At the times when he was looking for Bayer's χ, as Argelander has remarked, the variable would be near a minimum; hence his observing the nearest star of similar brightness.

THE MINOR PLANETS IN 1879.—Advanced sheets of the *Berliner astronomisches Jahrbuch* for 1881, containing places of the small planets during the present year have been circulated amongst observers, the ephemerides for the planets coming into opposition early in the year, some time since. There are positions of the first 187 members of this group, with the exception of Nos. 99 and 155, for which sufficient data are not available. Only two out of the number approach the earth at opposition, within her mean distance from the sun: *Isis*, on June 20, is distant 0·995, with a south declination of 25°, and *Hertha*, on September 12, 0·988, just upon the equator. No. 154 travels as far south as 50½° about July 14.

BRORSEN'S COMET.—The observations of this comet made at Arcetri and Kremsmunster from March 10 to 19 with Dr. Schulze's other elements, fix the time of perihelion passage to about March 30·5716 G.M.T., which is nearly twelve hours later than that assigned by calculation. The following ephemeris is founded upon this corrected epoch for arrival at perihelion:—

For 12*h. Greenwich M.T.*

1879.	Right Ascension. h. m. s.	Declination, North. ° ′	Log. distance from Earth.	Log. distance from Sun.
April 14	3 39 10	38 44·7	9·9202	9·8199
,, 15	3 43 59	40 3·9		
,, 16	3 49 0	41 23·0	9·9092	9·8317
,, 17	3 54 12	42 42·2		
,, 18	3 59 37	44 1·2	9·8986	9·8442
,, 19	4 5 18	45 20·0		
,, 20	4 11 16	46 38·4	9·8887	9·8571
,, 21	4 17 32	47 56·3		
,, 22	4 24 9	49 13·6	9·8794	9·8703
,, 23	4 31 8	50 30·0		
,, 24	4 38 33	51 45·4	9·8709	9·8836
,, 25	4 46 25	52 59·5		
,, 26	4 54 46	54 12·1	9·8633	9·8969
,, 27	5 3 39	55 22·9		
,, 28	5 13 7	56 31·6	9·8565	9·9103
,, 29	5 23 12	57 38·0		
,, 30	5 33 57	58 41·7	9·8507	9·9235

EDISON'S LAMP

A COMMUNICATION in yesterday's *Daily News*, from a New York correspondent of that paper, gives a glowing, and to all appearance justifiably so, account of Mr. Edison's success in attaining a form of electric lighting that seems to be in all respects much superior to anything hitherto produced. The first impression made on the correspondent was the mild effect of the light on the eyes, its steadiness, and the absence of that ghastly hue which seems to be an invariable accompaniment of the carbon. This new form of light has only been attained after many disappointments on the part of Mr. Edison, who, however, has all along been confident of success.

During the past two months the progress towards its present perfection has been very rapid. Chiefly contributing to this result has been the discovery of a new alloy, the fusing-point of which is much higher than either platinum or iridium, in fact, than any known metal. This discovery is spoken of by some of Mr. Edison's chief *employés* as the greatest achievement of his life. This alloy also reduces the cost of the valuable metals used in each lamp to such a point as to do away entirely with Prof. Tyndall's criticism. It is said to possess properties heretofore unknown, or at least undefined by scientific men. Not only has it cheapened the cost, but the union of the metals has increased the illuminating power to such a degree that six lights are now obtained per horse power where only four were possible with the pure platinum coil. Six lights per horse-power is the number authoritatively stated, but Mr. Edison's chief assistant does not hesitate to predict that eleven lights will eventually be obtained for each horse-power. This is not expected from the Gramme machine, however, which is now used; but is hoped for after the completion of the new generator, which a dozen of the most skilled workmen at Menlo Park are now engaged in constructing.

The lamp itself takes many forms. In some instances it is attached to the wall, like a gas bracket, and in many others it hangs from the ceiling and takes the external form of a glass globe, capped by brass or nickel attachments. There is none of the hissing, sputtering, and flickering observable in the carbon lamps. The lamp which attracts most attention is, in appearance, a St. Germain student lamp, without the reservoir for the oil. It stands in the middle of a small table, and two fine covered copper wires alone connect it with the main conducting cables from the Gramme machine. In this the *Daily News* correspondent tells us, are embodied all the latest improvements. He also tells us that there cannot possibly be any mistake, as Mr. Edison has taken crucial precautions in all directions. There is nothing in the lamp itself that gives any idea of its construction. The cunning device for rendering the flame steady is in reality the idea of the quadruplex telegraph applied to heat instead of electricity. Now that the new alloy has been discovered, its twofold purpose of preventing fusion and steadying the light is no longer served. The expansion of the tiny key, or switch, breaks the current for the fraction of a second, and permits the actual, though imperceptible, cooling of the incandescent coil. This connection is made and broken many times during each second, so that to human eyes the light is constant as the sun. The movement of a finger and thumb converts the glowing meteor before us into a night lamp for a sick-room. Again, it is seen at one-candle power, then at two, and so on. It is as manageable as a tallow dip, and much more satisfactory. It will not go out of itself, and needs no care. The little coil of wire is hermetically sealed in the glass chamber. It is not in a vacuum, but the chamber is filled with air. There is a sensitive spot on the metal cap in which the glass tube sits, and the expansion of the air manipulates the switch. The heat of the metal itself, therefore, is no longer relied on. The inventor explains that after all manner of severe tests this has been found the easiest and the least easily deranged manner of controlling the light. The difficulty of making thin plates of metal of equal density and weight rendered the previous method impracticable for small lights, although it will probably be the best form in which to secure the desired result where the lamps are to show lights of great intensity.

As there seems no reason to distrust the evidence of the *Daily News* correspondent, it may be accepted that Mr. Edison has succeeded in going a long way to solve some of the difficulties connected with the practical adoption of electric lighting. It is stated that in a few months the Edison Company will be prepared to supply the light to such private consumers as may desire it at at least one-third or one-fourth the cost of gas.

EXPERIMENTAL RESEARCHES ON THE REPULSION RESULTING FROM RADIATION[1]

II.

HAVING completed the experimental investigation of the amount of repulsion produced by radiation on disks of various kinds, and coated with different substances, I turned my attention to the amount of repulsion produced when polarised light is allowed to fall on a plate of tourmaline suspended in vacuo in a torsion balance. It was originally thought that a slice of tourmaline, being black to a ray of light polarised in one plane, and white to a ray polarised in the other plane, would be repelled when the incident light was quenched by it, and not affected when the incident light passed through it. Experiments, however, prove that this action does not exist in any

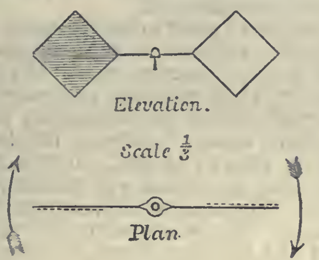


FIG. 4.

appreciable degree, the repulsion resulting from radiation being almost entirely a surface action, whilst the action of a tourmaline on a ray of polarised light is one in which thickness is necessary.

I next examined the effect of shape in influencing the amount and direction of repulsion. These experiments were for the most part tried with the apparatus shown in Fig. 3 (p. 513, part I.). Through the open top access can readily be obtained, and disks, plates, &c., can be quickly tested by being fixed to the extremities of a pair of aluminium arms, with a glass cap in the centre, rotating on the needle-point. Plates, 12 millims. square, cut from thin aluminium foil, were mounted diamond-wise on arms, and supported on the needle-point inside the bulb. The plates were lampblacked on sides facing

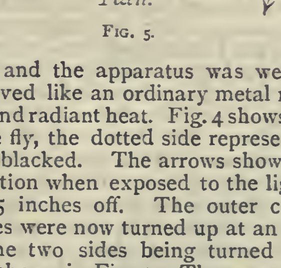


FIG. 5.

opposite ways, and the apparatus was well exhausted. The vanes behaved like an ordinary metal radiometer in respect to light and radiant heat. Fig. 4 shows the elevation and plan of the fly, the dotted side representing the one which was lampblacked. The arrows show the direction of positive rotation when exposed to the light of a standard candle 3·5 inches off. The outer corners of the aluminium plates were now turned up at an angle of 45°, 4 millims. of the two sides being turned up, leaving 8 millims. flat, as shown in Fig. 5. They were lampblacked on the inside, as shown in the figure by dots. A lighted candle 3·5 inches off caused very slow and feeble positive rotation. On shading the light from the black side, the bright side was repelled, causing positive rotation; and on shading the light from the bright side the black was repelled, causing negative rotation.[1] The positive repulsion was, however, rather stronger than the negative repulsion, so that, when both sides were illuminated, the force was only that due to the difference of these repulsions.

A hot glass shade is a convenient means of heating the bulb, by immersing it in a hot-air bath, without the liability of introducing action of rays other than those emitted by hot glass. On inverting a hot glass shade over the bulb in the above experiment, negative rotation was produced which changed to positive on cooling. Both these rotations were stronger than that given by the candle. The experiment was varied (1) by 6 millims. of the sides being turned up instead of 4; (2) by folding the plates across the vertical diagonal and then across their horizontal diagonal; (3) by attaching flat

FIG. 6.

plates to the arms at an angle of 45°, blacking them on the insides away from the bulb, and repeating the experiment with plates blacked on the outsides. The results obtained show that when flat plates are taken blacked on alternate sides, the rotation is normal or positive, *i.e.*, the black side is repelled. When the outer corners of each plate is turned up so as to keep the blacked surface on the concave side, the positive rotation is either diminished, stopped, or converted into negative rotation, according to the amount of surface of the plate which has been turned up. The favourable presentation of the surface of the vanes to the inside of the bulb has more influence on the movement than has the colour of the surface. Radiometers constructed with silver flake vanes set at an angle

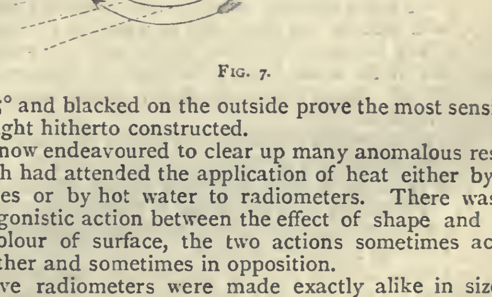


FIG. 7.

of 45° and blacked on the outside prove the most sensitive for light hitherto constructed.

I now endeavoured to clear up many anomalous results which had attended the application of heat either by hot shades or by hot water to radiometers. There was an antagonistic action between the effect of shape and that of colour of surface, the two actions sometimes acting together and sometimes in opposition.

Five radiometers were made exactly alike in size of bulb, shape of vanes, and degree of exhaustion, only differing in the material of which the vanes were composed. No. 1 was made of mica, 0·003 inch in thickness; No. 2, of mica, 0·0005 inch in thickness; No. 3, of pith,

[1] Continued from p. 514.

[1] I call the rotation *positive* when the black or driving side is repelled, and *negative* when the side which under ordinary circumstances would be the driving side, moves towards the light.

0·05 inch in thickness; No. 4, of aluminium, 0·002 inch in thickness. These four radiometers were plain on each side, no lampblack being applied. Their appearance is shown in Fig. 6. No. 5 was made of aluminium, identical with No. 4, but the vanes were lampblacked on each side instead of being bright. Had the vanes pointed radially there could have been no tendency for any one of the flies to move either way, but being inclined, the normal movement, on exposure to radiation, should be in the direction of the arrows—a direction which I called the *positive* direction.

In Fig. 7 the candle is represented shining on the bulb of the mica-vaned radiometer. The rays of light pass through the first wall without action. They then meet the mica, and that also being transparent, the rays pass through it likewise, and then escape through the opposite side of the bulb as is shown by dotted lines, without absorption and consequently without doing work. But in addition to light the candle is radiating ultra-red dark heat-rays, which in great measure are arrested by the glass, and raise its temperature. The inner surface of the bulb then becomes the surface on which molecular pressure is generated, which may be called the *driving* surface; this is shown by the shading next the candle. This molecular disturbance presses on the mica-vane which is in front of it, and drives it round in the direction of the arrows as if it were subjected to a bombardment of small shot. The vanes, in fact, may be said to be blown round by what may be likened to a wind, which however is not *molar* but *molecular*, inasmuch as there is no wind

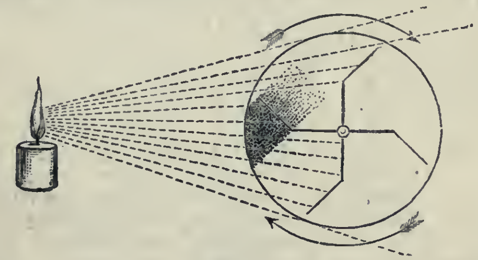


Fig. 8.

in the sense of an actual transference of gas from one part of the bulb to the other.

In Fig. 8 I have endeavoured to represent part of the action which takes place when the candle shines on the aluminium radiometer. The light passing through the bulb falls on the aluminium plate, and raising its temperature, causes pressure to be exerted on all sides. The molecules rebounding from the face next the glass, cause increased molecular pressure on that side, and produce movement in the direction of the arrows, or positive rotation. As each vane passes the candle it takes up heat, and acquires extra *driving* energy. As it swings round, the opposite side of the glass acts as a *cooler*, and by the time the vane has completed the circle, and has radiated away some of its extra heat, it is ready to recommence the cycle of transformation—light, heat, molecular pressure, motion.

Unlike mica, which generates very little pressure on its surface, the aluminium fly carries sufficient driving power to enable it easily to pass the dead centre opposite the candle. Therefore, as soon as the candle has shown on the aluminium radiometer long enough to warm the vanes a little, rotation readily continues.

The action of the pith radiometer is similar to the aluminium, except that the dissipation of pressure from the back surface of the pith will be almost *nil*. The pith, moreover, being sensitive to the heat-rays, and being a non-conductor, moves quicker than the aluminium, which requires time to get warm throughout.

The agreement between theory and observation, so far, seemed exact. I now tried numerous experiments with dark heat applied in various ways to these five radiometers. The results I obtained led me to think that the kind of dark heat might vary in refrangibility according to its source, and that the rays from hot water, hot glass, and hot metal, might affect the materials composing the vanes in a different manner, and being absorbed by one body and transmitted by another, might cause the positive or negative rotation which I obtained. I immersed the five radiometers in boiling water, and after cooling again immersed them in water only a few degrees above the temperature of the room; the results were similar to those I had previously obtained with water of 70° C. The radiometers were covered successively with hot shades of English, French, and German glass of different thicknesses, and at different degrees of temperature. The bulbs were also heated with a gas or spirit flame, but no uniform results were obtained.

A funnel was then heated in boiling water, and allowed to rest on the five radiometers in succession. They all moved in the *positive* direction, except the bright aluminium radiometer, which remained stationary. When the funnel was removed, the two aluminium and the thick mica radiometers rotated positively till they were cold. The funnel was allowed to cool. It was then inverted over a radiometer, and steam was passed through for a second or two. The same experiment was repeated with each radiometer. The results were now equally uniform with those of the last experiment, but the rotation was

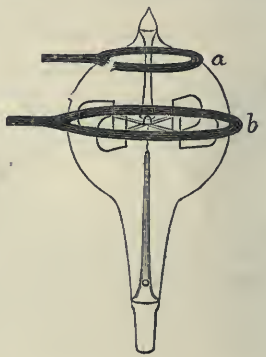


Fig. 9.

negative, the bright aluminium fly moving the best o all, and the pith fly the least.

I repeated the experiment with a thick brass ring, the internal diameter of which was about half that of the bulb (Fig. 9, *a*) and then with another brass ring a little larger in diameter than the bulb, *b*, Fig. 9. These rings were each heated to about 400°. With the first the rotation was *negative*, while in the second all the flies revolved in the *positive* direction. The two brass rings were made red hot, and held in position till the flies were in rapid movement, when the rings were removed and the hot part of the bulb dipped into cold water, so as to chill the glass quickly, and still keep the fly warm. These experiments proved that when heat is applied round an equatorial ring of the bulbs the rotation is always in the positive direction. The hot ring of glass generates molecular disturbance, which presses towards the centre and strikes the sloping vanes, driving them round as if the wind were blowing on them. In Fig. 10 I have tried to represent this action. The positive movement is independent of the material of which the fly is made, and is only slightly increased or diminished according to the conducting power of the fly for heat. The lighter the weight of the fly to be driven round, the easier it moves, and the heavier the fly the longer it keeps in motion after it is once started.

When heat is applied to either pole of the bulb *negative* rotation takes place. The molecular pressure proceeding

from a hot pole of the bulb will strike the *inner* surface of the sloping vanes. and driving them before it, will cause a rotation which appears *negative* to an observer, although it is really *positive* to the direction of pressure. Fig. 11 sufficiently illustrates this mode of action. The heat is supposed to be applied near the centre, and the molecular pressure, radiating on all sides, presses the vanes chiefly on the inner surfaces. The anomalous results obtained when the radiometers were heated with hot glass shades or hot water are thus accounted for. Polar heating gives *negative*, and equatorial *positive*, rotation, and when both are applied together by immersion in hot water, the direction of motion is governed by the stronger of these two forces.

In my description of Fig. 7 (p. 534) I showed that the glass heated by the ultra-red rays became hot, and acted on the driving surface, generating molecular pressure, and causing the sloping vanes to turn in the positive direction. At the same time the vanes get warm and become themselves sources of molecular pressure. The amount of molecular pressure thus generated depends on the capacity of the material of the vanes to absorb heat. Thin mica will hold very little, thick mica will hold more, and aluminium will hold most. This extra capacity for heat causes more molecular pressure to proceed from the aluminium and thick mica, and generates a proportionate amount of driving power on the surfaces of the vanes, turning them in the positive direction, and supplementing the action of the equatorial ring of hot glass.

The next subject of investigation was the action of radiation on cones, cylinders, and cup-shaped vanes. A pair of

FIG. 10.

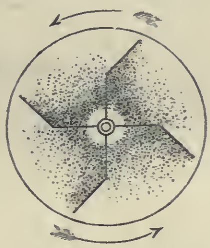


FIG. 11.

thin aluminium disks, cut half across the diameter, were bent into cones and mounted on two arms as a radiometer, the cones facing opposite ways. Several experiments were tried and repeated with cones of different material. The movement which appeared most anomalous was the attraction observed when a candle was allowed to shine on the hollow side of a cone or cup-shaped radiometer, the light being screened off the retreating side. Further experiments, however, showed that the effect of bending the plates, or of making cones of them, is to produce a more favourable presentation to the inner surface of the glass bulb. Radiation falls from the candle on the aluminium; some is reflected and lost, but a portion is absorbed, to be converted into thermometric heat or heat of temperature. Aluminium being a good conductor of heat, and the thickness of metal being insignificant, it becomes equally warm throughout, and a layer of molecular disturbance is formed on each surface of the metal. At a low exhaustion the thickness of this layer is not sufficient to reach from the metal cone to the side of the glass bulb; as the exhaustion increases, this layer extends further from the generating surface, until at a sufficiently high exhaustion the space between the side of the glass bulb and the adjacent portion of the metallic cone is bridged over, and pressure is exerted between the two surfaces. Fig. 12 shows how this pressure will act. The direction of pressure is indicated by dotted lines issuing from the metal cone. The more favourable presentation offered by the cone causes the pressure to be greatest between the glass bulb and the outside of the cone; the pressure from the inside of the cone and from the outside, away from the side of the glass, is dissipated without acting, but the pressure between the glass bulb and the side of the cone nearest to it is active; the cones, therefore, are pressed round in the direction of the arrows, and the motion has the appearance of *attraction*.

Cones being inconvenient in shape, I employed portions of cylinders wherewith to shape the vanes, and I ultimately found that cups were more easily affected by radiation than portions of cylinders, whilst they are more easily fashioned. I found that a four-armed cup-shaped aluminium radiometer, the cups being bright and 10 millims. in diameter, and the radius of the curvature being 6 millims., rotates in the light as well as a flat vaned instrument. I sealed one of these instruments on to the mercury pump. During exhaustion accurate observations were taken of the number of revolutions per minute caused by one or more standard candles 3 inches from the centre of the bulb. I also took observations of pressure, and the exhaustion was carried to a very high point. Fig. 13 shows the curve plotted from these observations, taking the rarefaction of the air in millionths of an atmosphere as abscissæ, and the number of revolutions a minute as ordinates. The curve traced through the dots representing observations illustrates the gradual increase of sensitiveness up to a certain point of rarefaction, and the sudden drop after that point is reached.

To still further investigate the action of dark heat on the vanes, I contrived an apparatus to which I could apply a very intense source of heat always ready in the

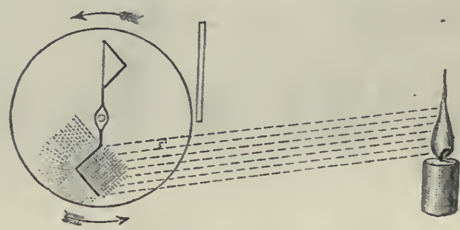


FIG. 12.

same place, the heat not having to pass through glass, and being completely under control as to intensity and time of action. The instrument with which I performed the great number of these experiments is shown in Fig. 14. The cylinder is sealed at the top so as to permit of the highest possible exhaustion. It is drawn off narrow at the end, and a stem is sealed in to hold a needle-point. To the narrow end a fine tube is attached to connect the apparatus to the mercury-pump. Round the needle is placed a ring of fine platinum wire, *a a*, the ends of which are joined to thicker platinum wires passing through the glass. The fly consists of four square vanes of clear mica, *b b*, inclined at an angle of 45° to the horizontal plane and supported on light aluminium arms. Above the vanes is a flat disk of clear mica, *c c*, having a glass cap in its centre, and easily rotating on a needle-point. The vanes and the mica disk are supported independently of each other on separate needle points, which are held in glass rods, *d*, *d*, *d*. A current of electricity from two Grove's cells, turned on or off by a contact key, gives the power of making the wire ring, *a a*, red hot when desired.

The normal or positive movement of the disk is in the opposite direction to that of the vanes; thus, if the positive movement of the vanes is in the direction of the hands of a watch, the positive movement of the disk is in the opposite direction. With the apparatus full of air at the ordinary pressure (bar. = 761 millims.) the direction of rotation, both of the vanes and disk, is *positive* when the platinum wire is ignited. The speed of the vanes is 13·3 revolutions a minute, and that of the disk 1 a minute

At a pressure of 80 millims. the disk does not rotate. The vanes rotate *positively* but slowly.

At 19 millims. no movement whatever takes place. The disk and vanes are as still when the wire is heated as when it is cold.

At 14 millims. the disk remains stationary. The vanes move slowly in the *negative* direction.

At 1 millim. the disk rotates in the *positive* direction slowly, whilst the vanes rotate *negatively* rather fast; the disk commences to rotate in the same direction as the vanes at a speed of three revolutions a minute.

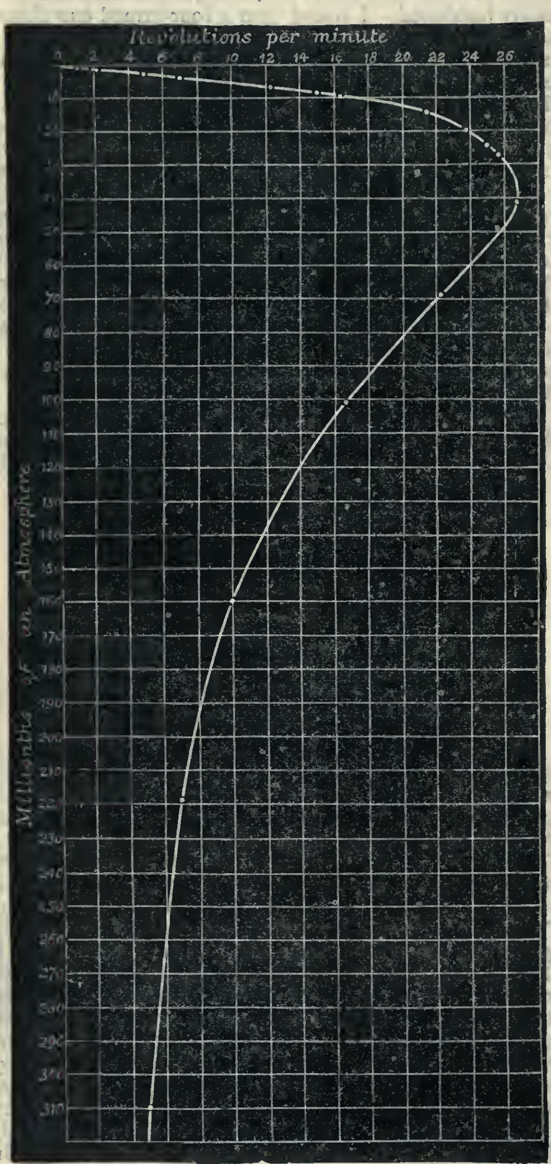


FIG. 13.

(At low exhaustions I speak of millimetres of pressure, but at high exhaustions I prefer to count in millionths of an atmosphere.)

At a pressure of 706 millionths of an atmosphere the direction keeps the same as at 1 millim. in each case, but the disk makes ten revolutions and the vanes forty revolutions a minute.

At 294 millionths, the speed of the disk and vanes is exactly alike, both rotating together in the same direction. Up to this pressure and at some distance beyond, the vanes have been gradually diminishing whilst the disk has been increasing in speed. At a pressure of 141 millionths the disk rotates rapidly, *positively*, but the vanes do not rotate at all. At a little higher exhaustion than the last, viz., at 129 millionths, a great change is observed. The vanes which were still now rotate in the *positive* direction at a speed of 100 revolutions a minute, whilst the disk rotates as before, but with a little diminished velocity. I have previously shown, in a paper to the Royal Society, that the viscosity of air at a rarefaction of 129 millionths of an atmosphere is only a little less than its viscosity at the normal density, and hence it is certain that the vanes at a speed of 100 revolutions a minute exerts a considerable drag upon the disk when it rotates in the opposite direction.

As the rarefaction increases above this point, the speed of both the disk and vanes increases till those of the latter exceed 600 revolutions a minute.

To carry these experiments to a much higher exhaustion it was necessary to modify the apparatus. The complex apparatus I now employed is shown at Fig. 15. Only the upper part of the pump *a b* is shown. It has five fall tubes and is fitted with a small radiometer, *c*, and a McLeod measuring apparatus, *d e*, to enable the degree of exhaustion in the apparatus to be ascertained. The phosphoric anhydride, for absorbing aqueous vapour, is contained in the horizontal tube *f*. In order as far as possible to prevent the passage of mercury vapour, three long narrow tubes *g g* are introduced between the pump and the apparatus to be exhausted; the one nearest the pump is filled with precipitated sulphur, the centre tube contains metallic copper reduced from its oxide, and the third tube phosphoric anhydride. At *h* is a vacuum-tube containing aluminium wires, and having a capillary bore for examining the spectra of the residual gas. An induction coil and battery are connected with the tube by wires. From the tube *h* two tubes branch off, one of them, *i*, leads to the "viscosity" apparatus contained in the case *k*, and the other, *j*, goes to the apparatus to be exhausted.

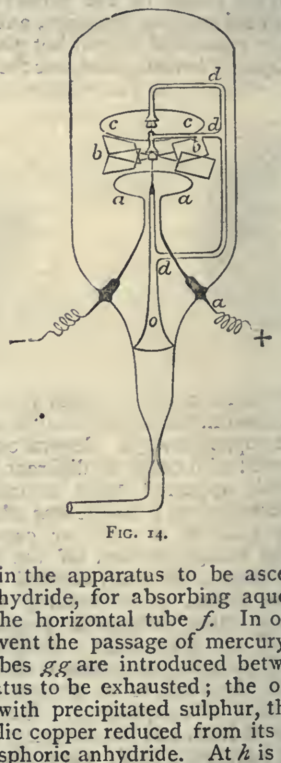


FIG. 14.

The apparatus *s*, containing the rotating disk and vanes, is sealed to the tube *j*. The platinum ring is ignited by the battery *t*. On the top of the ring rests a disk of mica, H, lampblacked on the upper surface; this cuts off direct radiation from the hot ring, and diffuses the heat somewhat over the surface of the black mica. Instead, therefore, of the molecular pressure starting from

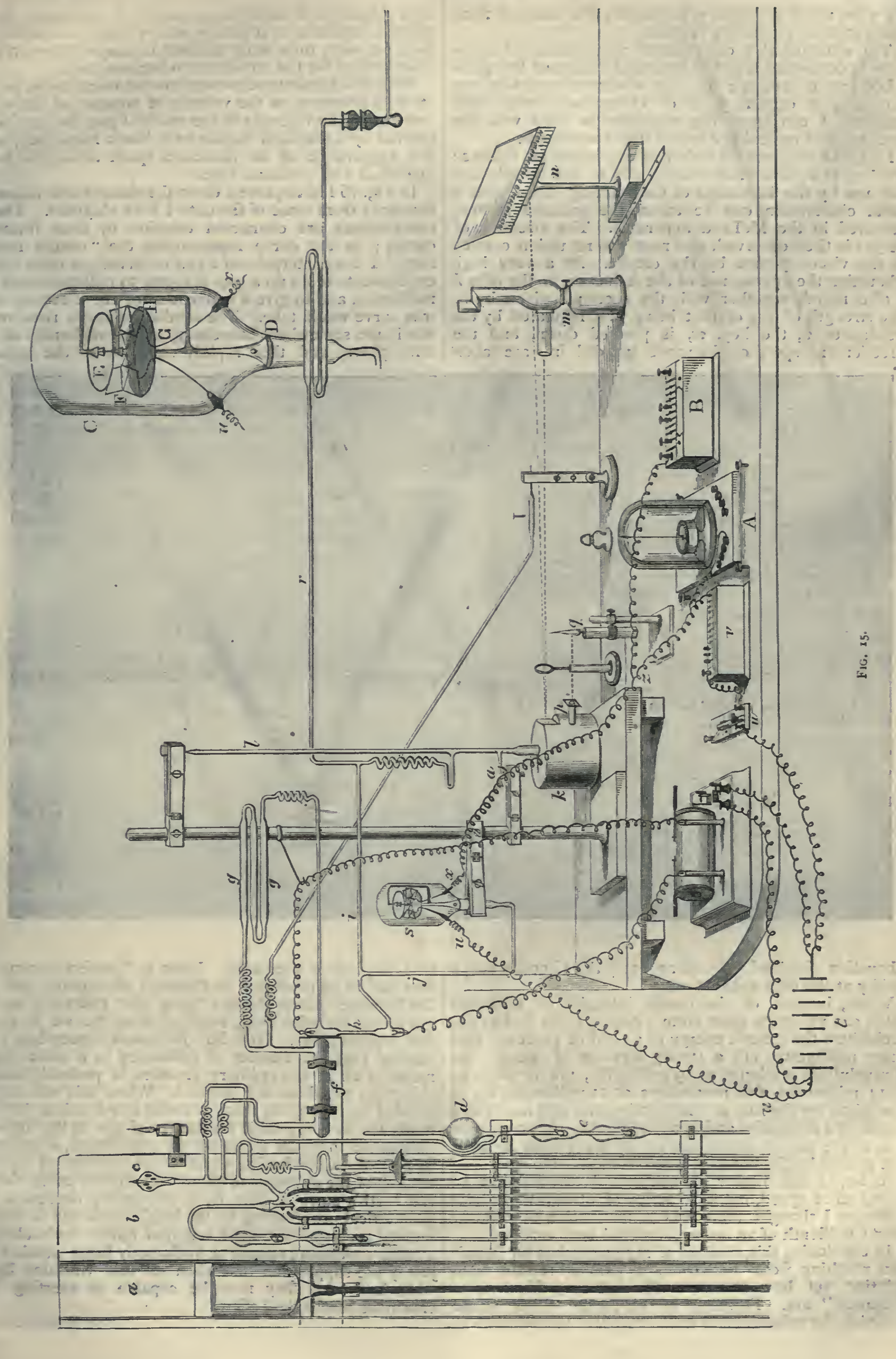


Fig. 15.

the wire, as in previous experiments, the blacked mica now becomes the driving surface.

The whole of this complicated arrangement of apparatus is connected together by actual fusion of the glass tubes one to another; no joint whatever occurs in any part, and a certain point of exhaustion being once attained, I can leave the apparatus to itself with the certainty that no leakage from without can occur.

I take an observation with this apparatus as follows: Arriving at a point when a depression of the contact-key tells me by the behaviour of the rotating disks that a useful observation can be taken, the pressure is first measured in the McLeod apparatus. The viscosity of the gas is then observed, and next the repulsion exerted on the viscosity-plate by the candle. At a very high exhaustion the appearance of the induction in the tube *h* is also noted, together with the spectrum given by it. The strength of the current being first regulated by the resistances *v*, the key, *w*, is pressed down, and the direction and speed of the vanes and disk in *s* are taken by a chronograph recording to tenths of a second. Frequently duplicate or triplicate observations are taken at each pressure, time being allowed to elapse between the observations for the apparatus to become cool.

With this apparatus observations can therefore be taken at each pressure, on the velocity of rotation of the disk and vanes, the viscosity of the residual gas, the repulsion exerted by a standard candle on a black mica plate, and the appearance of an inductive spark through a tube furnished with platinum wire.

In Fig. 16 I have plotted down the observations taken in air-vacua from some of the data I have obtained. These observations are connected together by lines forming curves; in the curve representing the "candle repulsion," I have interpolated a few observations from other experiments to fill up a gap between 59 millionths and 14 millionths, and to give a better idea of the direction the true curve would take. The candle repulsion rises to a maximum somewhere between 59 and 14 millionths of an atmosphere, and then rapidly sinks up to the highest

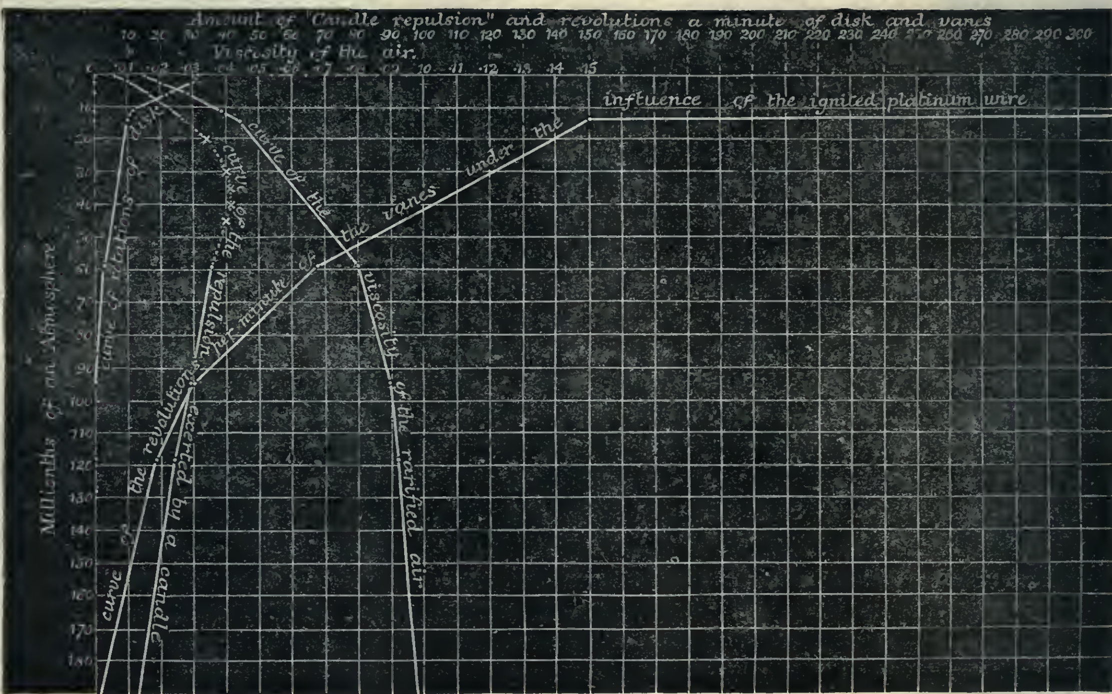


Fig. 16.

exhaustion obtained. Simultaneously the viscosity drops rapidly at the high exhaustions.

When, instead of the feeble intensity of radiation which can penetrate glass from a candle some inches off, I substitute the intense energy of a red-hot platinum wire a few millimetres off, a steady increase of speed from 67 revolutions a minute at 59 millionths, 150 revolutions at 14 millionths, 600 revolutions at 11 millionths, up to over 1,000 revolutions at 6 millionths, and still increasing speeds at 2 millionths and at 0·4 millionth. At an exhaustion, where the repulsion set up by the candle is least, that caused by the hot wire is greatest.

In air, at still higher exhaustions, I could detect no falling off of speed, but in a series of observations with hydrogen I thought there was a diminution of velocity after 1 millionth of an atmosphere had been reached.

In concluding this abstract of my researches on Repulsion resulting from Radiation, I cannot refrain from pointing out how erroneous the ordinary ideas of a "vacuum" are. Formerly an air-pump which would diminish the volume of air in the receiver 1,000 times was said to produce a vacuum. Later a "perfect vacuum" was said to be produced by chemical absorption and by the Sprengel pump, the test being that electricity would not pass, this point being reached, when the air is rarefied 100,000 times. Now Mr. Johnstone Stoney has calculated that the number of molecules in a cubic centimetre of air at the ordinary pressure is probably something like one thousand trillions. When this number is divided by 2,500,000, there are still four hundred billion molecules in every cubic centimetre of gas at the highest exhaustion to which I carried the experiment, illustrated in Fig. 16—a rarefaction which would correspond to the density of the atmosphere about seventy-five miles above the earth's surface, that is, if its density decreases in geometrical progression, as its height increases in arithmetical progression. Four hundred billion molecules in a cubic centimetre appear a sufficiently large number to justify the supposition that when set into vibration by a white-hot wire they may be capable of exerting an enormous mechanical effect.

W. Crookes

THE MIRROR OF JAPAN, AND ITS MAGIC QUALITY[1]

THE lecturer commenced by referring to the vast differences between the Chinese and the Japanese nations, of which the English people, as a rule, do not seem to be aware. He instanced various points of contrast; one of the most important being the intensely oriental secluded character of the private life of the Chinese on the one hand, and the Japanese dwelling in houses unfurnished and left wide open to the public gaze on the other. But why, he asked, in this comparative absence of nearly all that we should call furniture, does one article pertaining to the ladies' toilet—the bronze mirror with its stand—hold so prominent a position?

This mirror of the Far East is usually circular, from three to twelve inches in diameter, made of bronze, and with a bronze handle covered with bamboo. The reflecting face is generally more or less convex, polished with a mercury amalgam; the back is gracefully ornamented with a well-executed raised design representing birds, flowers, dragons, a geometrical pattern, or some scene in Japanese mythical history. Occasionally there are also one or more Chinese characters (signifying long life, happiness, or some similar idea) of polished metal, in bold relief. The general appearance of the back of the mirror, therefore, is something like that seen in the accompanying figure.

It might at first sight be surmised that the elaborate head-dresses of the ladies in Japan, combined with the painting of their faces, furnished an explanation of the prominence given to the metal mirror. But that this is not the case is easily seen from the fact that it is in the Imperial Palace, where the court ladies, still preserving the simple fashion of ancient days, merely comb back their long black tresses, and so have least need of a looking-glass, that the Japanese mirror receives the highest respect. A foreigner meets the mirror in the temples, in the hands of the street-conjuror, in pictures of the infernal regions, and in the regalia of the Japanese sovereigns, and for some time after his arrival in Japan, feels as an Oriental ignorant of biblical history might when unable to understand the constant repetition of the cross in Roman Catholic countries. But at length he hears that the mirror is part of the Japanese religion and mixed up with the "divine right of kings"; that it is the most precious of the possessions of a Japanese woman, and constitutes the most important part of the *trousseau* of a bride, and "the two Great Divine Palaces" at Isé, in which was deposited the first made mirror, have in the eyes of the Japanese the same importance as has the Holy Sepulchre for the Greeks and Armenians, and Mecca for the Mohammedans.

And to realise the reason of this, the stranger must learn that there is a famous ancient myth in Japan, which was recounted by the lecturer, detailing how the sun-goddess in a rage shut herself up in a rocky cave, and how the other gods to dispel the darkness thus caused, used various artifices to entice her forth, the most successful ruse being the manufacture of the first historical mirror, in which, seeing her face, she was drawn forth by her curiosity and jealousy. He will also learn how in the supposed creation of the Japanese Empire, the sun-goddess is reputed to have handed this mirror (with the two other "god's treasures," which, together with a mirror, at present constitute the regalia of the Emperor) to her grandson with these words, "Look upon this mirror as my spirit, keep it in the same house and on the same floor with yourself, and worship it as if you were worshipping my actual presence."

After describing many interesting points in connection with this strange mirror-worship of the Japanese, as seen in the palace and in the cottage, the lecturer went on to say that to the majority of those present the investigation of the so-called *magic* properties of the Japanese mirror would probably prove of yet more interest.

This magic property, which is possessed by a few rare specimens coming from the East, is as follows: If the polished surface is looked at directly, it acts like an ordinary mirror reflecting the objects in front of it, but giving, of course, no indication whatever of the raised patterns on the back; if, however, a bright light be reflected by the smooth face of the mirror on to a screen, there is seen on this screen an image formed of bright lines on a dark ground more or less perfectly representing the pattern on the back of the mirror, which is altogether hidden from the light.

When this appearance is seen for the first time it is perfectly startling even to an educated mind, and if the source of light is sufficiently bright, as for instance a tropical sun, it is difficult for the observer to divest himself of the idea that the screen is not perforated with cuts, corresponding with the pattern on the back of the mirror, and illuminated from behind.

This strange phenomenon was known to both Sir David Brewster and Sir Charles Wheatstone, both of whom were of opinion that it was produced by trickery on the part of the maker. Sir David Brewster, for example, says in the *Philosophical Magazine* for December, 1832: "Like all other conjurors, the artist has tried to make the observer deceive himself. The stamped figures on the back (of the mirror) are used for this purpose. The spectrum in the luminous area *is not an image of the figures on the back*. The figures are a copy of the picture which the artist *has drawn on the face of the mirror*, and so concealed by polishing that it is invisible in the ordinary lights, and can be brought out only in the sun's rays."

Prof. Ayrton then related how he had been quite unable to find for sale in any of the shops of Japan one of these magic mirrors, which was supposed in Europe to be a standard Japanese trick, and he explained how he had at length ascertained that with regard to this so-called magic mirror, the Japanese were the people who knew least about the subject.

But these magic mirrors were known to the Chinese from the earliest times, and one of their writers spoke about them in the ninth century of the Christian era. They call them *theou-kooang-kièn*, which means literally, "mirrors that let the light pass through them," this name,

[1] The Friday evening discourse at the Royal Institution, January 24, by Prof. W. E. Ayrton.

of course arising from a popular error on the subject. The Roman writer Aulus Gellius, who lived seventeen centuries ago, referred to mirrors that sometimes reflected their backs and sometimes did not. From the great antiquity of these Chinese magic mirrors, the German writer Herr Sterne has concluded that it is probable that the mirrors with secret signs and figures of imps on the back, which formed a portion of the stock-in-trade of the witches of the middle ages, were of Eastern manufacture. The Italian historian Muratori gives an account of the magic mirror found under the pillow of the Bishop of Verona, who was afterwards condemned to death by Martin Della Scala, as well as of the one discovered in the house of Colla da Rienzi, and on the back of which was the word "Fiòrone." But of these magic mirrors, which have played so important a part, not only in the priestcraft of China, but also in the oracles of the Greeks and Etruscans, and in the witchcraft of the middle ages, inquiry has shown that Japanese literature makes absolutely no mention.

Is it, then, that such mirrors cannot be found in Japan? Undoubtedly they cannot be bought on inquiry at the shops, but Prof. Ayrton's investigations have shown that if a careful examination with properly arranged light be made of a large number of the ordinary Japanese bronze mirrors, a few, perhaps 2 or 3 per cent., will be found showing the magic property clearly.

The lecturer then referred to the extracts he had made from a large portion of that which had been written in various languages regarding the explanation of the phenomenon. He mentioned that the earliest explanation was given by a Chinaman, Ou-tseu-hing, who lived between 1260 and 1341, and who also had the impression that the magic property of the mirror was produced by an artifice; for he wrote: "When we turn one of the mirrors with its face to the sun, and allow it to throw a reflection on a wall close by, we see the ornaments or the characters which exist in relief on the back, clearly. Now the cause of this phenomenon arises from the employment of two kinds of copper of unequal density. If on the back of the mirror a dragon has been produced while casting it in the mould, then an exactly similar dragon is deeply engraved on the face of the disk. Afterwards the deep chisel cuts are filled up with denser copper, which is incorporated with the body of the mirror, which ought to be of finer copper, by submitting the whole to the action of fire; then the face is planed and prepared, and a thin layer of lead or of tin spread over it.[1]

"When a beam of sunlight is allowed to fall on a polished mirror prepared in this way, and the image is reflected on a wall, bright and dark tints are distinctly seen, the former produced by the purer copper, and the latter by the parts in which the denser copper is inlaid."

Ou-tseu-hing adds that he has seen a mirror of this kind broken into pieces, and that he has thus ascertained for himself the truth of this explanation.

In a paper communicated some years ago to the French Academy of Sciences, the well-known French writer on China, M. Stanislaus Julien, says: "Many famous philosophers have for a long time, but without success, endeavoured to find out the true cause of the phenomenon which has caused certain metallic mirrors constructed in China to have acquired the name of magic mirrors. Even in the country itself where they are made, no European has up to the present time been able to obtain either from the manufacturers, or from men of letters, the information which is so full of interest to us, because the former keep it a secret when by chance they possess it, and the latter generally ignore the subject altogether. I had found many times in Chinese books details regarding this kind of mirrors, but it was not of a nature to satisfy the very proper curiosity of the philosopher, because sometimes the author gave on his own responsibility, an explanation that he had guessed at, and sometimes he confessed in good faith that this curious property is the result of an artifice in the manufacture, the monopoly of which certain skilled workmen reserve to themselves. One can easily understand this prudent reticence when we remember that the rare mirrors which show this phenomenon sell from ten to twenty times as dear as the rest."

The prevalent idea has been that the phenomenon of the magic mirror was caused by a difference of density in the various parts of the surface, either produced intentionally or accidentally; and this, the lecturer explained, arose from two causes, first, from the common belief that the patterns on Japanese and Chinese mirrors were, like those on ordinary coins, produced by stamping; the other, because the distinguished European philosophers who had examined into the question had investigated with considerable success, experimentally, how such mirrors might be made, but they had not, the lecturer thought, directed their attention to the examination of the question—How was the phenomenon in these rare eastern mirrors actually produced?—obviously a very different question.

Prof. Ayrton mentioned that he and Prof. Perry were led to take up the investigation from a very remarkable fact pointed out by Prof. Atkinson, of Japan, viz., that a scratch with a blunt iron nail on the back of one of these magic mirrors, although it produced no mark on the face of the mirror which could be seen by direct vision, nevertheless became visible as a bright line on the screen when a beam of sunlight was reflected from the polished face of the mirror. The lecturer mentioned that after trying several experiments with polarised light, &c., Prof. Perry and himself availed themselves of a very simple method of investigation, but one which had apparently not suggested itself to previous observers. On one occasion, when some of their students were using lenses to endeavour to make the exhibition of the phenomenon more striking, it occurred to them that the employment of beams of light of different degrees of convergence or divergence would furnish a test for deciding the cause of the whole action. For while, if the phenomenon were due to the molecular differences in the surface—the commonly received opinion—the effect would be practically independent of the amount of convergence of the beam of light; on the other hand, if it by any chance were due to portions of the reflecting surface being less convex than the remainder, a complex *inversion* of the phenomenon might be expected to occur, if the experiment, instead of being tried in ordinary sunlight, were made under certain conditions in a converging beam—that is, the thicker portions of the mirror might be expected to appear darker instead of lighter than the remainder.

[Experiments were then shown of the image cast on the screen: (1) when a divergent beam of light fell on the mirror, (2) when the beam was parallel, (3) when the beam was convergent; and it was seen (1) the pattern appeared as bright on a dark ground, (2) the pattern was invisible, (3) the pattern appeared as dark on a light ground.]

Again, by allowing a parallel beam of light to fall on it, and interposing a double convex lens between the mirror and the screen, we can make the image show the pattern either as a bright on a dark ground, or as dark on a bright ground, or not at all, merely by causing the screen to be: 1st, nearer the lens than the conjugate focus of the mirror; 2nd, farther than the conjugate focus; 3rd, at the conjugate focus. [This experiment was here shown.] Now it can easily be proved by simple geometrical optics that each of these effects would be produced if the thicker parts of the mirror were a little less convex than the remainder. [This was explained by various geometrical

[1] This probably refers to the mercury-amalgam which is used in polishing, and which Ou-tseu-hing mistook for lead or tin.

diagrams.] And lastly, if the phenomenon was, as the previous experiment would lead us to conclude, due not to unequal reflecting power of the different portions of the surface of the mirror, but to minute inequalities on the surface, in consequence of which there is more scattering power of the rays of light falling on one portion than on another, then, since rays of light making very small angles with one another do not separate perceptibly until they have gone some distance, it follows that if the screen be held *very near* to the mirror, the apparent reflection of the back, the magical property, in fact, ought to become invisible. And this also, it was shown, was exactly what happened when the screen was made almost to touch the polished surface.

The lecturer then proceeded to explain why a *divergent* beam emitted by a bright luminous *point* at some fifteen feet distance from the mirror gave the best effects.

We have therefore strong reasons for favouring the "inequality of curvature" theory. In order, however, to make the explanation quite certain, the lecturer said he had made a small concavity and a small convexity on the face of one of the mirrors, by hammering with a blunt tool, carefully protected with a soft cushion to avoid scratching the polished surface, and he showed by experiment that the concavity reflected a bright image and the convexity a dark one, when the pattern on the back appeared bright, but when the light was so arranged that the pattern appeared as dark on a bright ground, it was the convexity which appeared as the bright spot and the concavity as the dark one.

Guided by all that precedes, we are led to the undoubted conclusion that the whole action of the magic mirror arises from the thicker portions being flatter than the remaining convex surface, and even being sometimes actually concave. But, in spite of this irresistible conclusion forced on us by the experiments previously mentioned, it must be admitted that it seems extraordinary how such small inequalities in the surface of the mirror, so small in fact that the eye quite fails to detect them, can, even with a proper arrangement of the light, produce on the screen an image of the pattern on the back as sharp and clear as is seen with a good specimen of the magic mirror.

The next question arises, why is there this difference in the curvature of the different portions of the surface? The experience that Prof. Ayrton had gained from an examination of a large number of Japanese mirrors supplied, in part at any rate, the answer to the question. No thick mirror reflects the pattern on the back, not one of the many beautiful mirrors exhibited at the National Exhibition of Japan in 1877, and which the lecturer was so fortunate as to be able to experiment with in a darkened room with a bright luminous point at some twelve feet distance, showed the phenomenon in the slightest degree; some good old mirrors in the museum of the Imperial College of Engineering, and which belonged to the family of the late Emperor, the Shogun, of Japan, failed to reflect any trace of a design, and some old round mirrors without handles, which he had also tried, were (with the exception of one which was immensely prized, and brought to him wrapped in five distinct silk cases, and the heirloom of the family of a nobleman) equally unsuccessful.

Again, it is not that the pattern is less clearly executed on the backs of these choice mirrors, since the better the mirror the finer and bolder is the pattern, but what is especially noticeable is that every one of these mirrors is, as a whole, far thicker than an ordinary Japanese mirror, and its surface is much less convex.

This naturally led him to inquire how are Japanese mirrors made convex? Are they cast so, or do they acquire this shape from some subsequent process? His search through all the literature at his disposal—European, Japanese, Chinese—on the subject of mirrors failed to elicit the slightest hint; he was therefore compelled to perform the somewhat difficult task of obtaining information from the Japanese workmen themselves. Eventually he ascertained that while practically all Japanese mirrors were convex, the surface of each half of the mould was flat, and that the curvature was given to the mirror after casting in the following way: the rough mirror is first scraped approximately smooth with a hand-scraping tool, and as this would remove any small amount of convexity had such been imparted to it in casting, it is useless to make the mould slightly convex. If, however, a convex or concave mirror of small radius is required, then the surface of the mould is made concave or convex. On the other hand, to produce the small amount of convexity which is possessed by ordinary Japanese mirrors the following method is employed, if the mirror is thin, and it is with thin mirrors we have especially to deal, since it is only in these mirrors that the apparent reflection of the back is observed. The mirror is placed face uppermost on a wooden board, and then scraped or rather scratched with a rounded iron rod about a third of an inch in diameter, and a foot long, called a *megebo*, "distorting rod," so that a series of small parallel scratches is produced, which causes the face of the mirror to become convex in the direction at right angles to the scratches, but to remain straight parallel to the scratches, in fact it becomes very slightly cylindrical, the axis of the cylinder being parallel to the scratches. This effect is very clearly seen by applying a straight-edge in different ways to the face of an unpolished mirror which has received a single set of scratches only. A series of scratches is next made with the *megebo* in a direction of right angles to the former, a third set intermediate between the two former, and so on, the mirror each time becoming slightly cylindrical, the axis of the cylinder in each case being parallel to the line of scratches, so that eventually the mirror becomes generally convex. Some workmen prefer to make the scratches with the *megebo* in the form of small spirals, others in the form of large spirals, but the general principle of the method employed with their mirrors appears to be always the same—the face of the mirror is scratched with a blunted piece of iron, and becomes slightly convex, the back, therefore, becoming concave.

[Mirrors were here exhibited, one with its surface flat, although somewhat rough, just as it came from the mould after casting; a second that had received one set of parallel scratches with the *megebo*, and which by means of a straight edge was shown to be slightly cylindrical; and a third on the face of which the operation of scratching had been completed, and which was therefore slightly convex.]

After the operation with the "distorting-rod" the mirror is very slightly scraped with a hand-scraping tool to remove the scratches, and to cause the face to present a smooth surface for the subsequent polishing.

In the case of thick mirrors the convexity is first made by cutting with a knife, and the distorting-rod applied afterwards. But in connection with this cutting process of thick mirrors, there is one very interesting point. If the maker finds, on applying from time to time the face of the mirror to a hard clay concave pattern, and turning it round under a little pressure, that a portion of the surface has not been in contact with the pattern, in other words, that he has cut away this portion too much, then he rubs this spot round and round with the *megebo* until he has restored the required degree of convexity. Here again, then, scratching on the surface produces convexity.

Now, why does the scraping of the "distorting rod" across the face of the mirror leave it convex? During the operation it is visibly concave. The metal must receive, then, a kind of "buckle," and spring back again so as to become convex when the pressure of the rod is removed. It might in such a case reasonably be expected that the thicker parts of the mirror would yield less to

the pressure of the rod than the thinner, and so would be made less convex, or even they might not spring back, on the withdrawal of the rod and so remain actually concave. Again, since we find that scraping the face of the mirror is the way in which it is made convex, and the back therefore concave, we might conclude that a deep scratch on the back would made the back convex and the face slightly concave. Such a concavity would, as we have proved, explain the phenomenon of the bright line appearing in the reflection of sunlight on the screen, which was observed by Prof. Atkinson to correspond with the scratch on the back.

After the scratches produced by the *megebo* are removed the mirror is polished with whetstones and then with charcoal. The face now becomes fairly smooth, but it still generally contains some few cavities; these the maker fills up from a stock of copper balls of various sizes which he has at hand. (It was probably the presence of these bits of copper that led Ou-tseu-hing to believe that the explanation of the cause of the magic mirrors was the inlaying of different metals.) The face of the mirror is finally rubbed over with a mercury amalgam containing 50 per cent of tin, by means of a small straw brush or the hand.

The lecturer then referred to the various metal mixtures employed by the Japanese in making their mirrors, the best being composed of 75 per cent. of copper, 23 of tin, and 2 per cent. of a natural sulphide of lead and antimony.

Although the Japanese have paid no attention to the magic mirror, which has created such interest in Europe, they have, in connection with their priestcraft, employed mirrors on the surface of which, if looked at obliquely, could be seen the faces of saints, but which were not in any way connected with the pattern on the back of the mirror. [Photographs of such mirrors were projected on to the screen.] The lecturer also exhibited two mirrors of this kind which he had had made in consequence of the belief expressed by one of the Japanese mirror-makers that the phenomenon of the so-called magic mirrors was produced by chemical action on the surface. But the result of the experiment had been that if the face of a mirror which had been chemically acted on was polished until every trace of the marks disappeared for direct or oblique vision, then they also disappeared in the image produced by reflecting a beam of light on to a screen, and consequently that it did not seem possible, as far as his experiments had gone, to produce, by means of chemical action on the surface, a mirror fulfilling all the conditions of the magic mirror.

He concluded by saying: "It appears, then, contrary to what is commonly believed, that the magic of the Eastern mirror results from no subtle trick on the part of the maker, from no inlaying of other metals or hardening of portions by stamping, but merely arises from the natural property possessed by certain thin bronze of buckling under a bending stress, so as to remain strained in the opposite directions after the stress is removed. And this stress is applied partly by the distorting rod, and partly by the subsequent polishing, which, in an exactly similar way, tends to make the thinner parts more convex than the thicker."

GEOGRAPHICAL NOTES

THE April number of Petermann's *Mittheilungen* contains a paper of considerable geographical and geological value by Dr. Edmund Naumann, on the Plain of Yedo, Japan; it is accompanied by several illustrations and a fine map. Lieut. Onatzewich's account of his cruise in the clipper *Wssadnik* to the north of Behring Strait in 1876, is well timed in connection with the *Vega's* sojourn in that region. Dr. von Scherzer contributes an interesting paper on anthropometry, and Capt. Johannesen describes the voyage of the *Lena* from the mouth of the River Lena to Jakutsk. The monthly summary contains, as usual, many interesting items.

THE February number of the *Bulletin* of the Paris Society gives M. Savorgnan de Brazza's paper on his Ogowé expedition, the results of which we have already stated. A valuable and detailed notice of Dalmatia is contributed by M. de Sainte-Marie, and the number contains the recent correspondence of Dr. Crevaux from Guiana, referred to last week. The *Nouvelles* are continued.

AS we stated last week, though M. Soleillet has got so far on his journey to Timbuktu, it must not be forgotten that some fifteen years ago Lieut. Mage and Dr. Quintin reached the same place, and that after being detained as prisoners for eighteen months by the present Sultan Ahmadu, they were refused permission to embark on the Niger, and compelled to return to the coast *re infectâ*. Indeed, just as we go to press we learn that a telegram sent to the French Société de Géographie announces that the progress of M. Soleillet has been cut short. He was stopped by Ahmadu, the Emperor of Segou under the pretence that the roads are insecure in the north of his government. M. Soleillet has returned to St. Louis.

WITH reference to our note last week (p. 516) on the Conference in connection with the Inter-oceanic Canal, M. Maunoir, Secretary of the Paris Geographical Society, writes us that the Conference is to be held on May 19 under the auspices of that Society and not of the Society of Commercial Geography. The Congress, M. Maunoir writes, will not have any particular project in view, but will give an impartial hearing to the various projects brought before it. The various solutions of the questions proposed will be propounded by their authors, and, if deemed advisable, the Congress may in the end give its approval of one or other of the projects.

THE April number of the *Church Missionary Intelligencer* states that letters have been received from the missionary reinforcements who are travelling up the Nile to join the Nyanza expedition. They only reached Lado opposite Gondokoro, on October 10; after three weeks stay at that Egyptian station they reached Regiaf on November 7. The cause of delay, we are told, has been that the Nile has been unusually high, and the immense quantity of water loosened great masses of reeds and papyrus which formed floating islands and blocked up the river, besides which, owing to a deficiency of fuel between Khartoum and Lado, they remained fast bound during the whole month of September some distance south of Sobat, and the missionaries saw no living beings (besides the men on board) but "pelicans, fishes, and a white-headed eagle or two."

ANNEXED to the recently issued report of Her Majesty's Consul on the newly-opened port of Wênchow, is a very interesting map of the three Chinese provinces of Chekiang, Fukien, and Kiangsi, showing the means of communication by land and water between the principal cities and districts. The object of the map is more especially to point out the position of Wênchow as regards some of the most important districts in these provinces, the names of which with their products are given.

GARIBALDI has snubbed the Italian New Guinea expedition, so that it may now be regarded as nipped in the bud.

THE collection of funds instituted by the Dutch Central Committee for Arctic Exploration for the renewed outfitting of the *Willem Barentz*, which vessel is shortly to start on another expedition of some eighteen months' duration, is progressing so favourably that it may be reasonably expected that the 50,000 florins which are required for the expedition will soon be completely subscribed.

NOTES

In the interests of British science we have refrained now for some time from referring to the evil days which have fallen upon one of the most reputable of our learned societies. The time, however, has now come when silence is impossible. At the meeting of the Royal Astronomical Council yesterday, the Astronomer-Royal, in consequence of the recent action of the Council—an action inevitable when the present constitution of that body is considered—resigned his seat at the board. We cannot too much regret that this Society, the traditions of which are second to none in Europe, should have been utilised for some years past by an advertising clique who have everything to gain by their connection with a body of honourable students of science. The withdrawal of men long known for their astronomical work from the Council commenced some time since. It has now culminated in the resignation of the Astronomer-Royal, and we are informed that other resignations are to follow; indeed, a man of scientific repute risks somewhat in being found amongst the Councillors. Surely the Fellows of the Royal Astronomical Society of London are strong enough to remedy such a state of things as this.

At the meeting of the Council of the Royal Society, held last Thursday, the following fifteen candidates were selected to be recommended for election. The day fixed for the election is Thursday, June 12:—J. Anderson, M.D., Rev. M. J. Berkeley, H. Bessemer, Prof. A. Crum-Brown, W. L. Buller, Sc.D., G. H. Darwin, Prof. J. D. Everett, Prof. F. S. B. François de Chaumont, Prof. G. D. Liveing, G. Matthey, G. J. Romanes, A. Schuster, Ph.D., Prof. H. G. Seeley, B. Williamson, and T. Wright, M.D. The following have been elected Foreign Members of the Society:—Arthur Auwers, Berlin; Luigi Cremona, Rome; Jean Louis Armand de Quatrefages, Paris; Georg Hermann Quincke, Heidelberg; Theodor Schwann, Liége; Jean Servais Stas, Brussels.

Macmillan and Co. will publish shortly the following literary and scientific remains of the late Prof. W. K. Clifford. (1) A volume of mathematical papers which have been read before the Royal Society or contributed to scientific journals; Mr. Wm. Spottiswoode, F.R.S., will probably see this collection through the press. (2) Two volumes of collected essays and lectures, edited by Mr. Leslie Stephen and Mr. Frederick Pollock; Mr. Pollock will also contribute a biographical introduction to this work. (3) A small volume containing three popular lectures on "Seeing and Thinking."

We regret to see by the Civil Service Estimates that the amount to be devoted to "Purchases and Acquisitions" in the Department of Zoology of the British Museum during the present financial year has been reduced by one-fourth, *i.e.*, from 1,200*l.* to 900*l.* It seems rather absurd that a Government which has shown its anxiety to meet the claims of science by giving 4,000*l.* a year to be distributed in "aid of research" by the Royal Society, should have taken such a step as this to save a miserable 300*l.*, especially when it was the universal complaint of naturalists that the sum previously granted was wholly inadequate to the purpose. We cannot help thinking that the Trustees and their Secretary are in fault in this matter.

A further circular, in addition to that referred to on p. 472, has been issued by the Meteorological Office, with regard to the conditions on which weather information will be supplied. These conditions are too detailed to be noticed here, but they show a desire on the part of the Office to give every facility both to residents in London and in the country to obtain descriptions of the actual state of the weather and forecasts for not more than one day in advance. No doubt a copy of the circular may be obtained on application at the Office, 116, Victoria Street, S.W.

M. Renan was elected a few months ago a member of the Académie Française to fill the place vacated by the demise of M. Claude Bernard, the celebrated physiologist. On Monday, last week, he pronounced, before an immense audience, the *éloge* of his predecessor, in which he mentions that Claude Bernard must be considered as being the real founder of physiology in France. No public course of lectures was given before 1845, when M. Bernard established a laboratory, in the rue Saint Jacques, near the Panthéon. It was in this institution that the illustrious Academician conceived the idea of the great experiments which rendered his fame universal. But the establishment failed, M. Bernard having collected not more than five or six pupils.

The Council of the Society of Arts offers one gold and three silver medals for the best suggestions founded upon evidence already published, for dividing England and Wales into districts for the supply of pure water to the towns and villages of each district.

"A Citizen and Fishmonger" very pertinently asks in Tuesday's *Times* why it is that while we hear so much from time to time of the City Guilds' Technical College, and of the Society for University Extension in London, we hear nothing of any proposal for the utilisation of Gresham College?

The *Times* Geneva correspondent writes that the Lake of Neuchâtel is just now lower than has ever before been known, and continues to yield rich rewards to the researches of antiquarian explorers. Prof. Forel found, a few days since, at the lacustrine station of Corcelet, an earthenware vase dating from the age of bronze. On the bottom of the vase are plainly visible the impressions made by the fingers of the prehistoric potter in the plastic clay. Of these fingers—or, rather, of the thumb and forefinger, for the other digits are unfortunately lacking—the professor has taken a plaster of Paris cast and submitted them to a minute examination. He pronounces the maker of the vase to have been a woman. There are two impressions of the thumb and three of the forefinger. The prints left by the nails are perfect—that of the thumb, which must have been regular, well-shaped, and of an elegant convexity, measures in length twelve millimetres, in breadth eleven millimetres; the length and breadth of the finger-nail, equally well modelled, are eleven and nine millimetres respectively, the transverse convexity representing a curve or rise (*flèche*) of two millimetres. These nails, considers M. Forel, can only have belonged to a female hand. The vase has been placed in the cabinet of antiquities in the Vaudois Cantonal Museum at Lausanne. Another investigator, who has been cutting trenches in ground left bare by the abatement of the waters of the lake, has arrived, after careful examination of the *débris* and relics which his explorations have brought to light, at some interesting conclusions concerning the way in which certain of these lake-dwellings were destroyed, the time of the year when they disappeared, and the level of the lake at the epoch of their extinction. He believes they were destroyed by fire. This opinion he bases on the fact that, in all his explorations, he finds the same mixture of gravel and sand blackened and interspersed with charcoal and partly burnt seeds and bits of wood. This *débris* has evidently been carried to its present position by the waters of the lake, and varies in thickness according to the inclination of the slope on which it has been deposited. In other places besides those where the trenches have been cut, similar indications are observable—for example, at Bied, where, in sinking for the foundations of a house, a lacustrine cemetery was some time ago discovered; and at Colombier, where a stream running over the dry bed of the lake near the shore has laid bare *débris* identical with that brought to light by the excavations in question. From the quality and quantity of the winter stores, such as nuts, seeds, and berries,

found among these remains, the burning of the lake-dwellings probably occurred in spring or early summer. In one place a vessel was found filled with acorns, which, not being a favourite food, would naturally, it is presumed, be left to the last, and only used in default of something more palatable. From these facts and considerations it is inferred that at the time when many, if not all, the lake villages of Neuchâtel fell a prey to the flames, its waters were at the height usual with them in spring before their level had been artificially lowered by the engineering operations recently undertaken for confining within their channels the streams of that part of the Jurassic range which dominates the valleys of Neuchâtel and Morat.

THE death is announced, on Tuesday, at the age of eighty-two years, of Sir Anthony Panizzi, formerly Principal Librarian of the British Museum.

WE regret to announce the death of Herr Ludwig Reichenbach, an eminent German botanist and zoologist. He died at Dresden on the 18th ult., aged 86 years.

THE German scientific world is much gratified at the distinction conferred upon Dr. Julius Schmidt, of Athens, by the French Academy of Sciences, which recently honoured Dr. Schmidt by presenting him with the Prix Volz in recognition of his work on the moon. This prize is only given for important astronomical work, and has not been awarded since 1870.

THE various German societies for the protection of animals are now keenly discussing the vivisection question and have arranged a congress, which will shortly meet at Gotha, and which is to fix the final resolutions.

DR. J. E. TAYLOR has concluded the seventh winter series of scientific discourses at the Ipswich Museum. These have been free, and the average attendance has been 500 per night. Dr. Taylor regards the local collection of the Ipswich Museum as probably one of the best geological museums in Europe. He deserves credit for his exertions in spreading a knowledge of science in the district in which he lives.

ON Easter Monday and following day, the Geologists' Association will have an excursion to Weymouth and Portland.

WE have received a small pamphlet of eighty pages, containing the numerous opinions which have been expressed on Prof. Church's position with reference to the Agricultural College at Cirencester. The result of the conduct of the College authorities is likely to be the establishment of a rival institution, to be ready in October next. If it be the case, as is so generally alleged, that the Cirencester institution is a failure so far as agricultural education is concerned, we cannot be sorry that steps should be at once taken to supply the want.

WE have received the first two parts of a very fine "Atlas of Histology," by Dr. Klein and Mr. E. Noble Smith. The work is intended to be a pictorial and literal representation of the structure of the tissues of man and other vertebrates, its chief aim being to teach not so much the history of histology as histology itself in its modern aspect. The delicate illustrations are executed with wonderful care and beauty.

THERE was a slight shock of earthquake on the 3rd inst. at Cadiz.

IN the year 1877 Mr. Clark Mills, of Washington, the well-known sculptor, visited St. Augustine, Florida, in the interest of the Smithsonian Institution and of the Peabody Museum in Cambridge, for the purpose of taking casts of the heads of certain Indian prisoners in Fort Marion. These prisoners had been captured some years before, and sent to Florida for safe keeping, and were in charge of Capt. R. H. Pratt, of the Army. Most of them had been guilty of grave offences against life and property. A visit to and interviews with these Indians have constituted the staple of the correspondence of visitors to St. Augustine for several years past. During the year 1878 this station was broken up, some of the Indians being released, and others transferred to Hampton, Virginia, where, under the charge of General Armstrong, there is an establishment for the education of certain negroes. By authority of the War Department, Capt. Pratt has recently gathered up a large number of Indian youth of both sexes, and taken them to Hampton, where they are subjected to moral and mental training, and show a great aptitude for learning. Quite recently, at the request of the Smithsonian Institution, Mr. Mills visited Hampton, and with the assistance of Capt. Pratt, has made a series of excellent casts of some fifty Indians of both sexes and different ages. They will in time be reproduced, properly worked up, and exhibited in the National Museum, which will thus, in more than a hundred busts from life of the American aborigines, possess a very remarkable anthropological collection. Numerous applications have been received from the anthropological museums of Europe for copies of these busts.

THE Gauss monument for the city of Brunswick is now very nearly completed, the casting taking place in the studio of Prof. Howald, at Brunswick, after Herr Schaper's model, the well-known Berlin sculptor. The figure will measure nine feet in height, and the great mathematician is represented in a fur coat with a book in his left hand, bearing the inscription "Disquisitiones," the work which rendered his name immortal. The head is said to be a masterpiece of the plastic art.

SOME new experiments on digestion (in which portions of the stomach in living dogs were isolated, and their phenomena studied) have been recently described by Herr Heidenhain in Pflüger's *Archiv*. He arrives at these two (preliminary) conclusions:—(1) Purely mechanical stimulation acts only locally on secretion of gastric juice; (2) The secretion (act), however, extends beyond the place of stimulation to distant parts of the mucous surface, when absorption occurs at that place. In other words, we must distinguish a primary and a secondary secretion. The primary is small and is produced by mechanical effect at the place of stimulation; the secondary is abundant and depends on the act of digestion, in so far as absorption is connected with this, in the stomach.

WE learn that experiments have been made at Mont Valérien by the French Ministry of War with a number of portable Gramme electro-magnetic machines and portable lenses for directing the rays to a great distance and exploring the horizon during night with the same accuracy as during open daylight. The optical apparatus is mounted on a special waggon and weighs no more than two or three tons. It has been ascertained that the machines can travel with the same velocity as mounted artillery. The electric light and directing-lines can also be used for signalling in a known direction at an immense distance, as from Paris to Orleans, if placed at a sufficient altitude.

RECENT researches by Herr Ammon prove that the gas-absorption of dry ground depends on various factors—varying with the state of mechanical division, and chemical nature of the constituents of the ground, and the temperature and nature of the gas. (Quartz, clay, lime, hydrated iron oxide, gypsum, and humus were examined as to their behaviour with various gases of the atmosphere and the ground under different conditions.) Physical forces have an undoubted *rôle* in the condensation, *i.e.*, the gases are held and condensed by surface-attraction of the particles of the soil; and as this attraction is a function of the surface the effect is greater the smaller the particles. But che-

mical forces seem to have often a larger share in the effects; this appeared most distinctly in the remarkable absorbing power of hydrated iron oxide, and soils containing most of it; which can only be attributed to chemical changes undergone on access of gases, in consequence of the easy decomposition of the constituents. The same holds good for humus materials; whereas with the other constituents the physical "moment" comes out more prominently. Those gases which are easily brought into the liquid state (H_2O, H_2S, CH_4, NH_3, and CO_2), and which undergo changes easily (NH_3, SH_2), are absorbed in greater measure by the constituents of soils, than gases which are not readily liquefied (O). Temperature has great influence; with increase of it the power of condensing gases diminishes. It is notable that at temperatures between 0° and 10° the gas absorption again diminishes. (A fuller account of these researches will be found in *Der Naturforscher* for March 15.)

ANOTHER new Italian journal has come to hand, *Vita Nuova*, published fortnightly. It is devoted to literature, science, and art, and the scientific notes seem to us to be done with care and discrimination, containing occasional information on scientific work in Italy. It is published at Padua.

THE *Colonies and India* states that excellent accounts have been received from Tasmania. Stone exceedingly rich had been discovered in the Alpine reef, Fingal district, estimated to yield 200 ounces of gold to the ton. Gold had also been discovered at Castray River in the North Meredith Range on the west coast.

MM. FIGUIER AND DE PARVILLE have issued their *Année Scientifique*, which is a summary of all the notable discoveries made during the past year.

THE additions to the Zoological Society's Gardens during the past week include a Black-backed Jackal (*Canis mesomelas*) from South Africa, presented by Mr. E. J. Redman; a Red and Yellow Macaw (*Ara chloroptera*) from South America, two Yellow-fronted Amazons (*Chrysotis ochrocephala*) from Guiana, presented by Mrs. Kelly; two Silver Pheasants (*Euplocamus nycthemerus*) from China, presented by Mr. W. Soper; two Bernicla Geese (*Bernicla leucopsis*), two Brant Geese (*Bernicla brenta*), a Bean Goose (*Anser segetum*), two Common Geese (*Anser domesticus*), two Ruddy Sheldrakes (*Tadorna rutila*), two Common Pintails (*Dafila acuta*), two Gadwalls (*Anas strepera*), four Chiloe Widgeons (*Mareca chiloensis*), two Common Widgeons (*Mareca penelope*), two Garganey Teal (*Querquedula circia*), two Common Teal (*Querquedula crecca*), European, four Canada Geese (*Bernicla canadensis*), a Summer Duck (*Aix sponsa*) from North America, two Mandarin Ducks (*Aix galericulata*) from China, three Australian Wild Ducks (*Anas superciliosa*) from Australia, a Wild Goose (*Anser ferus*), three Common Ducks (*Anas boschas*), British Isles, presented by Mr. R. J. Balston, F.Z.S.; a Tuberculated Lizard (*Iguana tuberculata*) from Trinidad, presented by Dr. J. F. Chittenden, C.M.Z.S.; two Mississippi Alligators (*Alligator mississippiensis*) from the Mississippi, presented by Lord Francis Conyngham, M.P., F.Z.S.; a Common Lobster (*Astacus vulgaris*), British Seas, presented by Mr. R. J. Milestone; two White-rumped Roe Deer (*Capreolus pygargus*) from Eastern Asia, a Chinese Water Deer (*Hydropotes inermis*) from China, received in exchange; two Common Boas (*Boa constrictor*) from South America, deposited; eight Indian Jerboa Rats (*Gerbillus indicus*) from India, purchased.

VIRCHOW ON THE PLAGUE

AT a recent meeting of the Medical Society of Berlin Prof. Virchow gave (by previous request) his views on the subject of the Plague of Astrakan. The following is a brief outline of his address (which appears in the *Revue Scientifique*) :—

The information received about the plague is very uncertain and conflicting, necessitating reserve in giving an opinion. Russia is to be reproached for failure to send competent observers of the disease.

The first question is, What is the true determinant of the malady? No one would hesitate to say that the *buboes*, or swellings of the lymphatic glands, take the first place. But it is still doubtful whether or not certain acute forms occur without glandular tumour. (It is not merely the exterior glands that are referred to.) Typhoid fever, the most nearly related disease, is, of course, always (except in children) marked by a peculiar affection of the glands.

The pestilential bubo, like the alterations characterising many of our indigenous infectious diseases, consists in a cellular hypertrophy, with more or less hyperæmia and hæmorrhage. We come again to an obscure point in asking, how does a bubo of this kind ulcerate? The ulcerations in typhoid fever show suppuration, *within the glandular tissue*, round a dry mortified centre. Accounts by some of the best observers of pestilential bubo affirm suppuration *round the gland*. But the chief point is, what is the prime cause of the suppuration? And here I cannot affirm that the abscess of the gland arises from mortification of the gland. Some observers speak only of a softening of the substance. If partial mortification induced the ulcerations distinguishing plague, as they do those of typhoid fever, the analogy between the two diseases would be more marked.

Though we do not really know whether glandular affections are, from the outset, an essential character of the disease, they yet present the essential criteria for diagnosis of the plague.

[Prof. Virchow proceeds to object to Hirsch's view regarding the Indian Plague of Pali, in 1838, which showed peculiar symptoms, as a particular species of plague. Either it was the plague or it was not the plague. If the malady of Astrakan were a peculiar Indian plague we should not be warranted in identifying it with the well-known plague of the Levant, for this alone is the true plague. Hæmorrhage of the urinary organs (as in the plague of Pali) is very common even in the Oriental plague; but he would not therefore create a special hæmaturic species of the plague, but say that hæmaturia is often a symptom of the plague. He does not now refer either to this plague of Pali nor to a known endemic plague, restricted to two districts of the Himalayas.]

Another special character of Oriental plague is carbuncle; it occurs in nearly a fifth of the cases; the symptoms are like those of ordinary contagious carbuncle. There is no sufficient evidence of carbuncle in internal organs. Then there are the petechiæ, or spots, and interior hæmorrhages. Tumour of the spleen seems very constant, and of great importance, and we find also tumefaction of the liver and kidneys. The swellings of the glands, the carbuncles, and the petechiæ are the most important symptoms.

At the beginning of most pestilential epidemics a Committee of doctors has generally declared that it was not the plague. They pronounced it petechial typhus. This was the case immediately before the outbreak of the plague at Rescht, when the disease had been long confined in Kurdistan and Mesopotamia. M. Tholozan was the first to say it was plague, and that the case was that, not of a great epidemic, but of a latent disease, spreading slowly and attacking only a few. It is indubitable that we have there a true centre, whence the disease gradually spread, and I do not see why we should go to India, where the disease has not prevailed for many years past. Proceeding logically, we shall accept this course: From Kurdistan and Mesopotamia to Persia and on to beyond the Caspian. Even if the present cases on the other side of the Caspian were accompanied by pneumorrhagia, I would not hesitate to say they belonged to the plague proper, and that the disease is the same as that in Mesopotamia. The symptoms are very different from those of petechial typhus, the disease which the Turkish doctors affirmed. If near Salonichi (Xanthi) there be really petechial typhus accompanied by *Metastasis bubonica*, I fear it is the plague. It remains to keep our eyes open and see what happens after the return of the Russian army from the infected country (an occurrence which may well rouse grave apprehensions).

What has been done for our protection is little apt to tranquillise us. A blockade comprising all the frontier as well as the coast, from the Baltic to the Black Sea, seems to me illusory.

One example of severe quarantine has occurred in this century in the case of the plague at Noja, in Bari (Kingdom of Naples), in 1815. Trenches were dug, and three cordons of sentinels were

formed (the third round the entire province), with orders to kill whoever tried to break the blockade and did not stop at the first summons; and some individuals were actually killed. But I cannot think an entire country is able to protect itself thus. Examination of passports would be excellent if those who deliver passports and certificates of health were angels. But the Russian functionaries are men, and think like men. The impossibility of always getting true certificates of origin has been seen in the case of the cattle plague. I consider, however, that pressure should be exerted on Russia to form a blockade of the infected districts. And especially it should be seen to, that the returning Russian army does not bring any pestilential germs with it. As to restrictions on communications by land, the greatest of these are ineffectual for the end desired.

I cannot give an opinion as to whether the matters which are now forbidden to be imported into the German Empire may propagate the plague. The negative does not seem to suffice. We know that the skin or hair of an ox affected with carbuncle may engender contagion after several months in distant countries; we should not forget this, and we have not the right to say that garments, linen, bedclothes, &c., are perfectly innocuous.

A word on two points relative to disinfection. On Prof. Pettenkofer's advice, the German empire has decided for sulphurous acid as a means of disinfection. I question if this substance would penetrate linen, clothes, wool, &c., in such a complete way as to annihilate all germs. In my opinion a better plan is disinfection of clothing, &c., by dry heat in a chamber surrounded by steam-pipes, the temperature being raised to 120° C. or more. This plan is more rational and easy, and damages the objects least.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

In the Cambridge Senate, last Saturday, Mr. Balfour, a recent examiner in the Natural Science "Specials" for the B.A. degree, spoke of the schedule for botany, to which we referred recently, as extremely unsatisfactory and incomplete; histology was altogether neglected, physiology was very unsatisfactorily dealt with, and cryptogamic botany was almost entirely omitted. He was in favour of an examination in elementary biology being substituted, and practical work being required. Physiology should be made a separate subject. Prof. Humphry spoke in favour of reducing requirements in schedules to the narrowest range, in order to make natural science attractive to men. Mr. Trotter thought the schedule in botany an absurdly small representative of a year's work for a man supposed to have no other definite study. Mr. Bettany found fault with the present constitution of the Board of Natural Science Studies, which dealt with too many subjects, each being insufficiently represented by men engaged in teaching and research. No doubt it is hard for many to realise that biology has very many distinctive aspects, each of which must be represented by proficiency in them to prevent injustice and injury to scientific progress. Hindrances also arise from the fact that many of our present leaders and directors of study were developed before the full recognition of cell-study, embryology, and the like, as the basis of sound biology.

Dr. Alexander Dixon, Professor in the University of Glasgow, has been elected to the Professorship of Botany in the University of Edinburgh, vacant by the resignation of Dr. Balfour.

SCIENTIFIC SERIALS

The *Archives des Sciences Physiques et Naturelles* (February, 1879), contain the following papers of interest:—On the hydrocarbons obtained through the action of methyl-chloride upon benzole in the presence of ammonium chloride, by MM. E. Ador and A. Rillet.—On the theory of *timbre* and particularly on that of vowels, by Dr. Schneebeli.—On the scientific principles of the fine arts; essays and fragments on the theory, by E. Brucke, followed by remarks on optics and painting, by H. Helmholtz.—On some rare mosses, by J. E. Duby.—Stratigraphical study of the south-western part of the Crimea, by E. Favre.—Natural history of batrachia, by Fr. K. Knauer.—On apogamic ferns and on apogamy generally, by Prof. A. de Bary.—On the prolification of the fruit of mosses, by Dr. N. Pringsheim.—On polyembryony, by Dr. Ed. Strasburger.

The *Journal de Physique* (March) contains the following more important papers:—On spectroscopes with direct vision and great dispersion, by M. Thollon.—On the logograph, by M. Barlow.—On a new phenomenon of static electricity, by M. E. Duter.—Note on spectrophotometers, by M. A. Crova.—On the vibratory motion generated in the air and in space by electric sparks, by E. Mach.—On the electricity of air, by Rob. Nahrwold.

SOCIETIES AND ACADEMIES

London

Royal Society, March 13.—"On the Influence of Coal-dust in Colliery Explosions." No. 2. By W. Galloway. Communicated by Robert H. Scott, F.R.S., Secretary to the Council of the Meteorological Office.

The first experiment is made with a very simple apparatus consisting of a continuous pipe about 18″ in diameter, which conducts a small portion of the return air from the point at which it is ejected into the atmosphere by the ventilating fan, to a convenient spot on the level of the surface, where it escapes as a strong current, amounting to 1,251 cubic feet per minute. About 6 feet from its point of exit a lamp can be placed in the centre of the current, and at a distance of about a foot still nearer the origin there is a means of allowing coal-dust to fall into and mix with the passing air. It is found that when the coal-dust is added the air becomes instantly inflammable, showing that all the return air in the workings may be easily brought into the same condition by a sudden disturbance such as that caused by a local explosion of fire-damp.

The second experiment is intended to illustrate the effects of an explosion of fire-damp in a dry mine containing coal-dust. One part of the apparatus represents a gallery with coal-dust lying on its floor as well as in the horizontal timbers, the buildings and other rough surfaces at its top and sides; another part represents a cavity in the roof containing an explosive mixture of fire-damp and air. When the explosive gas is ignited the flame sweeps down into the gallery, the disturbance raises the coal-dust, and the results are exactly those that have been foreseen. The gallery is a wooden pipe 14 inches square inside, by 79½ feet long. The explosion-chamber is a sheet-iron cylinder lined with thin wooden laths; it is 5 feet high by 15 inches in diameter, and it stands vertically on the top of the gallery at a distance of 5 feet from one end. Currents of air of different quality can be made to pass along the gallery from the end next the explosion-chamber, which can be isolated by means of a valve, to the other end which is open to the atmosphere; thus the return air of the mine can be made to traverse it, or a current of pure air, or a current of air mixed with any required proportion of fire-damp. At the point where they enter the gallery these air-currents are heated by a steam cylinder, which occupies part of the space between the explosion-chamber and the nearer end, so as to assimilate their temperature to that of the air in a mine. The coal-dust is spread along the floor of the gallery, and some is laid on shelves so that it may more easily mix with the air when it is disturbed.

The explosive mixture is made by admitting about half a cubic foot of fire-damp into the explosion-chamber at its upper end, while a corresponding quantity of air is allowed to escape through a plug-hole at its lower end. The bottom of the explosion-chamber is separated from the gallery by a diaphragm of paper during this part of the operation. After the requisite quantity of fire-damp has been admitted, its volume having been accurately measured so as to guarantee that the results will always be the same, the mixture is effected by rapidly revolving a small fan, situated at the top of the explosion-chamber, and so constructed as to draw in air from the centre of the chamber, and throw it out at the circumference. From the point at which the fan draws in its air a 4-inch pipe descends to near the bottom of the explosion-chamber, and when the fan is revolved the air is drawn up through this pipe and discharged at the top of the chamber, from which it finds its way again to the bottom, and so on. The circulation established in this way is so rapid that a perfect mixture can be made in half a minute. The explosive mixture is ignited by means of a spark from a powerful magneto-electric machine.

When there is no coal-dust in the gallery the flame of the fire-damp explosion does not extend further than from 7 to 9 feet from the bottom of the explosion-chamber.

When the gallery contains coal-dust, on the other hand, on the floor and on the shelves referred to, and when it is filled with the return air of the mine the explosion traverses its whole length, and shoots out into the air to distances varying from 4 to 15 feet. The flame of the fire-damp explosion is thus magnified ten times by the presence of the coal-dust and the return air.

When pure air is employed instead of return air, other things remaining the same, the explosion is only about one-half as extensive; and when an artificial mixture of air and fire-damp is employed, of the same composition as the return air, without its excess of moisture, the explosion is stronger than with the return air. The arrangements whereby pure air and air and fire-damp can be used have only been recently completed, and few experiments have been made with them as yet.

Although the apparatus employed in this experiment appears to be on too small a scale to solve the coal-dust question unequivocally, the results obtained with it appear to be sufficiently conclusive to enable us to affirm that an explosion, occurring in a dry mine, is liable to be indefinitely extended by the mixture of air and coal-dust, produced by the disturbance which it initiates.

The only means of avoiding the dangers due to the presence of coal-dust in mines appears to be to carefully and constantly water the roadways leading to and from the working-places.

It is very interesting to be able to mention a fact in connection with watering the roadways which, although not mentioned in Mr. Galloway's paper, is well worthy of a place here, viz., that the Abercarne explosion, ramified through every part of the workings, which were exceedingly dry and dusty, with the exception of one district from which it was entirely cut off by 200 yards of a very wet roadway, and that the men in the latter district not only escaped unhurt, but hardly felt the explosion. The wetness of this roadway was due to natural causes.

April 3.—"On the Thermal Conductivity of Water," by J. T. Bottomley, Lecturer in Natural Philosophy and Demonstrator in Experimental Physics in the University of Glasgow. Communicated by Prof. Sir William Thomson, LL.D., F.R.S.

The result arrived at by the experiments described, is that the thermal conductivity of water may be taken at from ·0022 to ·00245 in square centimetres per second.

Some experiments have been made on the thermal conductivity of solution of sulphate of zinc, a solution which happened to be convenient for preliminary trials. The specific heat of solution of sulphate of zinc at different densities, which it is necessary to know for comparison as to thermal conductivity of that liquid with water, has been determined.

Experiments are now being carried on on this subject with the assistance of a grant from the Government Fund of 4,000*l*.

Anthropological Institute, March 25.—Mr. E. B. Tylor, D.C.L., F.R.S., president, in the chair.—Mr. Henry Seebohm, F.Z.S., gave some interesting particulars respecting the native races of Arctic Siberia, accompanied by an exhibition of ethnological objects collected in that region. In 1874 he visited Lapland, of which he gave some account, and in the following year he proceeded from St. Petersburg to Archangel, and thence 600 miles eastward, where he first came in contact with the Samoeïdes, and obtained some particulars about the Voguls, who dwelt across the Ural range. But his most adventurous journey was in 1877, when he accompanied Capt. Wiggins on his expedition for the exploration of Arctic Siberia. After travelling 2,500 miles from London to Nishni-Novogorod, they took sledge thence, and pushed on 3,500 miles farther, until they reached the Arctic Circle. In the Tartar villages there which they visited they found that the crescent predominated over the cross, and what still more surprised them, it seemed to be the symbol of a superior civilisation and order. The native languages were akin to the Turkish. The copper-coloured Buriats, who dwelt behind the Baikal Mountains, were a somewhat different race, and bore a strong resemblance to the Chinese. The Ostiaks and Dolgans were located on the colossal river Yenisei, which was reckoned the third largest river on the face of the globe. The Tungoosks were settled on one of its chief tributaries. The costumes, weapons, tools, smoking-appliances, reindeer-harness, snow-shoes, snow-goggles, idols, &c., of these and kindred tribes were shown, together with a remarkable case of prehistoric bronzes, found in ancient Siberian graves, and thought to be from 4,000 to 5,000 years old.—A paper was read by Sir Charles Nicholson, Bart., D.C.L., LL.D., on some rock carvings found near Sydney, New South Wales. Rude carvings of human and other animal forms, especially kangaroos and fishes, including the whale, had been found at various points of the coast of New Holland, from Cape Howe to Moreton Bay. The present natives had no tradition as to their origin, yet there were no good grounds for refusing to regard them as works of indigenous art. Col. Vigors had copied many of them, and a number of his drawings were handed round. One of these carvings represented a wall thirty feet long. Those found in Sydney cavern included a kangaroo at bay and a man erect, with out-stretched arms. Another class of similar carvings were chromatic. They were found on the north-west coast, and had been plausibly supposed to be the work of Malay pearl-fishers or shipwrecked sailors.

Zoological Society, April 1.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—An extract was read from a letter addressed to the Secretary by Mr. Carl Bock, respecting the habits of the Mountain Antelope of Sumatra (*Capricornis sumatrensis*), of which he had obtained a living specimen destined for the Society's collection.—Mr. J. W. Clark exhibited and made remarks on a drawing of a Dolphin belonging to the genus *Lagenarhynchus*, which had been lately taken off Ramsgate.—Prof. Flower exhibited a coloured drawing of a young female of the common Dolphin (*Delphinus delphi*) lately taken off the coast of Cornwall, and made some observations on the published figures and geographical distribution of the species.—The birds' eggs collected during the *Challenger* Expedition were exhibited. The series was stated to contain about 250 eggs belonging to fifty different species. Amongst these were eggs of the Sheathbill (*Chionis minor*) from Kerguelen, and of the Wandering Albatross (*Diomedea exulans*), from Marion Island.—Prof. Mivart exhibited a figure of and made remarks upon a Kestrel with abnormal feet, in the collection of the Marquis de Wavrin, at Brussels.—Mr. R. Bowdler Sharpe, F.Z.S., read an account of the collection of birds made by Mr. F. W. Burbidge, in the Sooloo Islands. A new Jungle Fowl was described as *Gallus stramineicollis*, and a new Parrot as *Tanygnathus burbidgii*.—A second communication from Mr. Bowdler Sharpe, consisted of a list of the birds of Labuan Island and its dependencies, founded principally on the collections formed during the last four years, by Governor Ussher and Mr. W. H. Treacher, but including also descriptions of a large number of eggs carefully collected by Mr. Hugh Low. One new species, *Cypselus lowi*, was described.—A communication was read from Mr. R. Collett, C.M.Z.S., containing the description of a new fish of the genus *Lycodes*, from the Pacific, which he proposed to call *Lycodes pacificus*.—A communication was read from Prof. Garrod, F.R.S., containing an account of the variations in the trachea and tracheal muscles in the different forms of gallinaceous birds.

Institution of Civil Engineers, March 25.—Mr. Bateman, president, in the chair.—The paper read was on the electric light applied to lighthouse illumination, by Mr. J. N. Douglas, M. Inst. C.E. The author showed the progress of lighthouse luminaries from wood and coal fires to the introduction of tallow candles, fatty oils, mineral oils, coal gas, and electricity.

Statistical Society, March 18.—The president, G. J. Shaw Lefevre, M.P., occupied the chair.—Mr. H. H. Hayter, the Government Statist of Victoria, read a paper on the colony of Victoria; its progress and present position.—The following were elected as Honorary Members:—M. le Dr. E. Janssens, of Brussels, M. Arthur Chervin, of Paris, Signor Gerolamo Boccardo, of Genoa, and Prof. Dr. Fr. Xav. von Neumann-Sfallast, of Vienna.

Victoria (Philosophical) Institute, April 7.—Two papers were read, one by Thomas Karr Callard, F.G.S., and one, taking some special points, by Prof. Boyd Dawkins, F.R.S. The subject was the contemporaneity of man with the extinct mammalia (as taught by recent cavern exploration), and its bearing upon the question of man's antiquity. The first paper contended that the cavern evidence points to the more recent extinction of the mammalia referred to, rather than to the remote existence of man.

BOSTON, U.S.A.

American Academy of Arts and Sciences, March 12.—Prof. W. A. Rogers presented a paper on the coefficient of expansion of the brass bars used by the U.S. Coast Survey for standards of length. In order to compare different standards it was found

necessary to determine the coefficients of expansion of the particular bars on which the graduations were made. In the present case the coefficient was found to be ·0000097 by a process extremely simple and effective. The relations between water and air contact also seem to be well determined by this method of investigation.—Dr. J. J. Putnam showed a *pendulum myograph*, modified mainly for the sake of economy, from that of Wundt. The pendulum itself is made of the thickest plate-glass, and arranged so as to be moved up and down, with the aid of a racket and a counterpoise, together with the stage bearing the movable connections described by Wundt. Since for each position of the pendulum a tracing of given length would have a different significance from that in any other position, enameled cards were prepared with lines upon them diverging from the point of suspension, the intervals between which corresponded to ·01″ when the amplitude of swing was 20°. By means of this apparatus the *reliability of Marey's tambour* had been tested, with a view to its use in time-experiments in physiology. The delay for the tambour used, with a tube about 2 m. long, was found to be nearly ·01″, varying not more than ·002″ to ·003″ under impulses of different character and strength.

Geneva

Society of Physics and Natural History, December 19, 1878.—M. God. Lunel spoke of the variations of colour presented by the squirrel, and cited some cases of albinism of that animal in a special locality of the Valais.—Prof. Brun described observations of the phenomenon known as "rain of blood," made by him on May 14 last, on the Jebel Sekra, a summit of the Rist, at the western extremity of the Atlas, in Morocco. He observed it in the form of spots of a very bright red appearing in the rocks, and one to a mixture of siliceous sand and very fine lime, with abundance of unicellular algæ of the species *Protococcus fluvialis*, and containing especially peroxide of iron.—M. Wm. Barbey informed the Society of the gift recently made by Sir J. Hooker to the Museum of Lausanne of the herbarium of the botanist Gaudin of Nyon.

January 2, 1879.—Prof. Graebe made a communication on the discovery of alizarine in the various colouring matters extracted from it, and particularly on alizarine allies.—M. Alph. de Candolle gave an account of the number of specimens contained in his herbarium, commenced by his father in 1798. At the time of the death of the latter in 1841, the herbarium contained 161,748 specimens; now it contains 287,636 belonging to 80,000 or 90,000 vegetable species.

Paris

Academy of Sciences, March 31.—M. Daubrée in the chair.—The following papers were read:—Conformity of the systems of fractures obtained experimentally with the systems of joints which traverse the cliffs of Normandy, by M. Daubrée. These joints form two systems, and the general effect is like that produced in a plate by weak torsion.—Convenience of special denominations for different orders of fractures of the earth's crust, by M. Daubrée. He proposes the name *diaclase* for a fissure produced by rupture; *paraclase*, to express that the fracture is accompanied by displacement; and *lithoclase*, as a general term including the two large groups now specified.—New process for the gauging of rivers, by M. Boileau. This process is based on the property of water-courses, that at the surface there are two streams whose velocity of translation is equal to the mean velocity of the current. The hydrometric operations are reduced to use of a float.—On the last floods of the Seine, by MM. Lalanne and Lemoine. Last winter was marked by two successive floods reaching (at a short interval) very nearly the same high level (6·21 m. at the Pont Royal on January 8 and 6·05 m. on February 24). Since 1872 M. Lemoine has organised, under M. Belgrand's direction, a system of warnings of flood (three days previous) for the Seine and its larger affluents. They are sent by telegraph or otherwise to seventy four persons in Paris and sixty-seven outside of Paris, and have proved very correct.—On some observations of glazed frost similar to that of January last, and on the mode of formation of hail, by M. Colladon. In large hail-storms, the cumuli producing them are divided into several distinct groups, insulated electrically from each other by sections of dry and cold air, resembling smoke-columns from several chimneys. The columns of hail the author conceives as a huge descending piston; hence the violent whirling movements of wind near the ground, and the descent of cold, dry, highly electrified air from the upper regions, to restore equilibrium; this air divides the clouds into nearly vertical columns; hence the peculiar forms of lightning during those storms.—M. Du Moncel presented a work entitled "L'Eclairage Electrique."—Prof. Lawrence Smith was elected Correspondent in Mineralogy in room of the late Sir Charles Lyell.—Chemical researches on a filamentous matter found in the excavations of Pompeii, by M. de Luca. The substance (which seemed formed of numerous filaments) was black and almost completely carbonised; on simple pressure with the fingers it was reduced to powder. The threads seem to have been flax or hemp, altered by various natural agents underground during eighteen centuries.—On the cost of constructing lightning-conductors, by M. Melsens. His system applied to the large new barracks at Etterbeck-laz-Bruxelles, on buildings occupying 20,000 square metres, will be less than 6,000 francs.—Observations of Planet 193, discovered at Marseilles Observatory, by M. Coggia, February 28, 1879, communicated by M. Stephan.—On two equations with partial derivatives relative to the multiplication of the argument in elliptic functions, by M. Halphen.—On cylindric or logarithmic potential with three variables and its employment in the theory of equilibrium of elasticity, by M. Boussinesq.—Anomaly presented by magnetic observations of Paris, by M. Flammarion. Since 1870-71 the last maximum of sun-spots and of diurnal variation of the declination-needle, the amplitude of this variation has decreased everywhere except at Paris; here it seems stationary; and even the year 1877, which should approach a minimum, presents a maximum. Some like anomalies are observable in previous times.—On the thermal and galvanometric laws of the electric spark produced in gas, by M. Villari. *En résumé*, the thermal and galvanometric deflections produced, the former by the spark, the latter by the discharge of a condenser, are proportional to the quantity of electricity which produces them, and to the length of their active circuits.—Magnetic rotatory power of gases at ordinary temperature and pressure, by M. Becquerel. With improved apparatus he has not only got the rotation-effect but been able to measure it with precision. He gives results for coal-gas, &c.—On the magnetic rotatory power of vapours, by M. Bichat. The experiments were like the Strasburg ones, but with a brass instead of an iron tube (which is objected to as forming a hollow electromagnet).—Pressure exerted by galvanic deposits, by M. Bouty. A cylindrical thermometer bulb covered with gold leaf or silver is made negative electrode in decomposition, *e.g.*, of a salt of copper; the rise of mercury when deposit occurs is noted. All metals, zinc included, exert pressure thus; but the pressure is not necessarily normal nor the same at all points, and cannot serve directly as a measure of the phenomenon; it is the result of a change of volume of the metal in deposition.—On the alkalies of pomegranate, by M. Tauret.—On the formation of carbonic acid, alcohol, and acetic acid by yeast alone, without oxygen, and under influence of this gas, by M. Bechamp.—On glazed frost observed in Florida, by Mr. Collin.

CONTENTS

THURSDAY, APRIL 17, 1879

THE AUSTRALIAN AND TASMANIAN RACES

The Aborigines of Victoria, with Notes relating to the Habits of the Natives of other Parts of Australia and Tasmania. Compiled from various sources for the Government of Victoria by R. Brough Smyth, F.L.S., F.G.S., &c. 2 vols. (London: Trübner and Co., 1878.)

IN these two bulky volumes we are for the first time presented with a really comprehensive account of the natives of Australia; and by their timely publication under the auspices of the Victorian Government an emphatic reply is given to the charge often brought against the colonists of indifference to the past history, present condition, and ultimate fate of those races. The work, whose title gives a very imperfect idea of its varied contents, may be best described as a complete encyclopædia of Australian folk-lore, as complete, at least, as a judicious utilisation of all available materials could render it. As such, its appearance may fairly be regarded as an "epoch-making" event in the progress of ethnological studies, performing much the same office for the Australian that the writings of Castren, Uslar, and Bleek, and the Schoolcraft series have done for the Finno-Tartar, Caucasian, South-African, and North-American races. In the character and selection of the subject-matter it bears most resemblance to this last-named compilation, while differing widely from it in the method of its treatment, the confusion and discordant elements inseparable from Schoolcraft's erratic plan being here avoided by a clear arrangement of the materials and a uniform system consistently adhered to throughout.

A large portion of the work, it should be mentioned, has been composed by the distinguished geologist, Mr. R. Brough Smyth, at intervals during the sixteen years he has acted as secretary to the Board for the Protection of the Victorian Aborigines. In this capacity he has had exceptional opportunities of obtaining the most reliable information regarding the natives of that colony, who naturally occupy the largest share of attention. But the others are by no means neglected, and the subject is rendered sufficiently complete by several valuable papers on the tribes of New South Wales, Queensland, South and West Australia, supplied by the Rev. W. Ridley, Philip Channey, J. Moore Davis, and other contributors, all speaking from personal knowledge of the facts.

Besides an able introduction of some fifty pages, surveying the whole field and imparting a certain unity to the work, the first volume is devoted to strictly ethnological subjects. Under twenty separate headings the physical and mental qualities of the aborigines, their social habits, daily life, food, diseases, dress, weapons, implements, manufactures, and myths, are treated in detail. The general impression produced by a careful perusal of the vast array of facts here brought together is, that the "black-fellow" is not nearly so black as he has been painted, a statement which is quite as true in the material as it is in the moral sense of the word. Thus the prevailing colour is represented as not black at all, but rather a "chocolate brown," sometimes inclining to black, sometimes of a lighter hue, and it may be remarked that this is supported by the independent testimony of Richard Oberländer, a most accurate observer, who expressly states that "die Haut ist nicht schwarz, sondern von dunkler Kupferfarbe" ("Der Mensch vormals und heute," Leipzig, 1878, p. 41). The hair also, though in some cases jet black, would appear to be more frequently of a "deep brown," and with boys and girls, "in colour brown, not very dark" (i. p. 5). This, combined with its wavy character ("crisp," "waved," *passim*, but never "woolly" or even "frizzly," like the Papûan), will be regarded by many anthropologists as conclusive of the mixed origin of the aborigines. On this interesting point the compiler unfortunately throws very little light, though he adopts the view held by many, that "there are in Australia two distinct races of men, one of which is clearly of the white variety" (i. 328). By "the white variety" he obviously means what is commonly understood by the "Caucasian" type, and the countenance here and elsewhere given to the belief in the presence of a Caucasian element in Australia is one of the weak points of the work. The curious lists of words adduced, though with some reserve and hesitation, in support of a community of speech carries us back to the days when etymology did duty for science. Thus the native terms *kurrin*, *trippin*, *throkkun*, are compared with the English *inquiring*, *dripping*, *throwing*, leaving the impression that there might possibly be some sort of connection between a native verbal ending *in*, *un*, and the quite recent English verbal ending *ing*—recent, at least, in its modern participial and gerundial senses. Of course, those who bring together such fanciful comparisons have no sense at all of the growth of language, but they might have common sense enough to reflect that it is a growth, and consequently that in the case of idioms assumed to have been originally one, the comparisons must be made not between subsequent historical developments, but between primitive organic elements, if any such exist in common. Then when they have exhausted English, Anglo-Saxon, Greek, Mœso-Gothic, Sanskrit, they rush off to Hebrew, Phœnician, and especially Tamulic and Telugu, without reflecting that, however mixed, the Australian tongues can hardly be made up of such utterly discordant elements as Aryan, Semitic, Dravidian, or that if they are Aryan, they cannot be Dravidian, and so on; hence that their etymological method, proving too much, proves nothing, or, in other words, is unscientific. It is much to be regretted that these simple principles are not more generally understood, and that too many otherwise valuable ethnological works should still continue to be disfigured by linguistic discussions which, a generation or so hence, will be looked upon as amusing anachronisms.

The sections devoted to the native weapons, stone implements, canoes, &c., are of great interest to the anthropologist, who will here find more than one long-cherished doctrine rudely shaken. Thus the argument for a common Australoid race, embracing the Australians, natives of the Deccan, and others, based on the supposed identity of the boomerang with the Indian throwing-stick is shown to be utterly worthless. The question of the resemblance between the *wonguim* or true returning boomerang and other similar weapons met with amongst

other ancient and modern peoples is discussed at considerable length, and the result thus briefly summed up in the introduction:—"Those who have seen such a wonguim thrown by a native accustomed to its use, need not be told that the statements published from time to time in the scientific journals in Europe are founded on imperfect information, or dictated in an unphilosophical spirit by a too great desire to prove that the Dravidian races of the Indian Peninsula and the ancient Egyptians belong to the Australoid stock, and that the boomerang was known to the Egyptians. . . . . There is nothing to show that anything like the wonguim was known to any other people anywhere at any time, and it is at least doubtful whether any weapon resembling the barngeet [*i.e.*, the war boomerang that does not return] was known to the Egyptians. The wonguim and barngeet are altogether different from the saparu, or sickle-shaped sword, which is represented on Babylonian and Assyrian cylinders as the weapon of Merodach or Bel." Thus the boomerang goes the way of the etymologies, though it is but fair to add that the famous passage quoted at p. 327 of vol. i. from St. Isidore of Seville, descriptive of the Gaulish or Teutonic *cateia*, "Genus Gallici teli ex materia quam maxime lenta; quæ, jactu quidem, non longe, propter gravitatem, evolat, sed ubi pervenit vi nimiâ perfringit. *Quod si ab artifice mittatur rursum reddit ad eum qui misit.* Hujus meminit Vergilius dicens. *Teutonico ritu soliti torquêre cateias.* Unde et eas Hispani *Teutones* vocant" ("Origin." xviii. c. vii.), has not been satisfactorily got over. At the same time it may not be superfluous to remember that Gauls and Teutons were not Dravidians, and that, notwithstanding its return motion, the *cateia* was not necessarily a *wonguim*, for other weapons also can be made to behave in the same way. The author regards the boomerang as of native invention, and adds that it is not known in all parts of the Continent, and has not been found in New Guinea or Tasmania.

In the section devoted to the subject of canoes, another popular error is exploded, for it is here abundantly shown that seaworthy boats made of the bark of the gum-tree, and evidently of native invention, were common in the south and east, and not merely on the north coast, where they might have been introduced by the Papûans from New Guinea, or the Malays from the Eastern Archipelago. On the other hand, the practice of cannibalism, about which doubts have always been entertained, is fully confirmed. "It cannot be denied that cannibalism prevailed at one time throughout the whole of Australia. The natives killed and ate little children, and the bodies of warriors slain in battle were eaten. . . . . It is sad to relate that there are only too many well-authenticated instances of cannibalism," &c. (Introd. xxxvii.). It may be added that some years ago the writer received direct evidence of an undoubted case from a lad named Benedict brought to Europe by Dr. Brady, formerly Roman Catholic Bishop of Perth (West Australia), and who assured him that his own little sister had been "speared, roasted, and eaten" by a hostile tribe near New Norcia.

The current views regarding the extremely low mental capacity of the natives, and even regarding their moral qualities, are in other respects shown to be entirely at variance with the truth. In such a wide area there are, of course, great mental as there are great physical differences. But the author's assertion that the estimate commonly entertained of their intellect is, on the whole far too low, seems to be fully borne out by the evidence here accumulated. Though without permanent dwellings, they make provision for the future, construct permanent works of art, have a common property in some things, respect each other's rights, are skilful hunters, have five different ways of catching fish, and are far less cruel and ferocious than many savage races usually regarded as their superiors. They have a keen sense of justice, though their standard of right and wrong, and their notions of political economy may be different from ours, as is evident from the language addressed to Mr. G. F. Moore, Advocate-General of West Australia, by *Yagan*, Chief of the Upper Swan tribe, in the year 1843: "Why do you white people come in ships to our country and shoot down poor black fellows who do not understand you? You listen to me! The wild black fellows do not understand your laws; every living animal that roams the country, and every edible root that grows in the ground, are common property! A black man claims nothing as his own but his cloak, his weapons, and his name! Children are under no restraint from infancy upwards; a little baby boy, as soon as he is old enough, beats his mother, and she always lets him! When he can carry a spear he throws it at any living thing that crosses his path, and when he becomes a man his chief employment is hunting. He does not understand that animals or plants can belong to one person more than to another. Sometimes a party of natives come down from the hills, tired and hungry, and fall in with strange animals you call sheep; of course, away flies the spear, and presently they have a feast! Then you white men come and shoot the poor black fellows! But for every black man you white fellows shoot I will kill a white man! And the poor hungry women have always been accustomed to dig every edible root, and when they come across a potato garden, of course down goes the wanná (yam-stick), and up comes the potato, which is at once put into the bag. Then you white men shoot at poor black fellows. I will take life for life" (ii. p. 228). And so the comedy is played out, until there are no more "black fellows" left on the scene, and when they are gone the white man does them, perhaps, the tardy justice to admit that he never understood them, and that they were not, after all, quite so bad as he had supposed.

At one time the natives were thought to be so stupid that they could not recognise the pictures or other representations even of such familiar objects as kangaroos, emus, or gum-trees. But so far from that being the case, they are here shown to be tolerably expert draughtsmen; and at p. 258, vol. ii. there is given a facsimile of a drawing of some squatters by a native lad, in which the attitudes and expressions are admirably delineated, "clearly indicating the humorous train of thought passing through the mind of the artist, who must have been a close observer and a good mimic."

From the facts adduced in the section devoted to the native myths it is evident that they have some notion of a future state, though their ideas of the deity are often somewhat crude, and their conception of the universe decidedly materialistic. Thus we are told that *Bun-jil*

created all things, but he made no women. Bun-jil has a wife named *Boi-boi*, a son named *Bin-beal*, and a brother named *Pal-ly-yan*, and though the creator of all things, yet he had help from his son and brother. He always goes about with a large knife, and after making the earth he went all over it, cutting and slashing it into creeks and rivers, mountains and valleys. Such, at least, is the belief of the Boonooreng tribe, Coast of Victoria. But, on the other hand, that of the Barwen tribe differs little from the Christian conception; for their account is that *Baiame* (lit., the *builder*, *shaper*, cf. *Schöpfer*) made earth and water and sky, animals and men. "He makes the rain come down and the grass grow;" he has delivered their fathers from evil demons (cf. the mediæval doctrine of demonology); he welcomes good people to the great *warrambool, i.e.*, watercourse and grove, in the sky—the Milky Way—a paradise of peace and plenty; and he destroys the bad" (cf. Revelations, *passim*). Indeed, the parallel is often so striking, that a suspicion sometimes arises whether these myths may not be spurious, mere travesties of the Christian doctrines disseminated by the missionaries amongst the natives, and improved upon by them for the benefit of over-zealous collectors of popular traditions. The doubt is raised in this work, but not always removed. Some, however, are undoubtedly genuine, as, for instance, the account of the River Murray, which was made by a snake. "He travelled from the head of the river to the mouth, and as he went along he formed the valley and the bed of the river." But in doing this he disturbed the crow, which was perched on a tree, became angry, and cut him into small pieces. The pieces are left where the Hindu myth leaves the turtle that supports the elephant upholding the globe.

The second volume is mainly devoted to the native languages, but also contains a series of appendices consisting of a number of papers on incidental subjects supplied by the contributors already referred to. Of these the "Notes on the System of Consanguinity and Kinship of the Brabrolong Tribe," by A. W. Howitt, and the monograph on "The Crania of the Natives," by Prof. Halford, of the Melbourne University, are specially interesting. The latter, which is a very valuable contribution to anthropological studies, is illustrated by a series of carefully made drawings of five skulls, by Major Shepherd, from four different points of view, and is accompanied by complete tables of measurements on the plan recommended by Prof. Cleland, and for the purpose of "obtaining national distinctions of a most exact description." Amongst these skulls is that of "King Jemmy," of the Mordialloc tribe, which presents some very remarkable peculiarities. It is of an extremely brutal type, in the front view showing a mid-rib running along the top, like the crest of a gorilla, and bounded on either side by a temporal ridge, which, with immense orbits, nasal fossæ, and prognathous upper-jaw, give it a most ape-like appearance. Jemmy, lately deceased, is not stated to have been of an abnormal type; and the side view, in which the brutal aspect disappears, conveys rather the impression of a skull of large capacity.

The philological section, occupying altogether 220 pages, does not consist of a systematic treatise on the native languages, but is made up of a number of papers by more or less competent hands, on a large number of Victorian dialects. Some of these papers, as might be expected, are very sketchy and superficial, but others are extremely valuable, containing, besides vocabularies, many grammatical features, short specimens and sentences accompanied by verbatim and free English translations. Ample materials are here supplied for forming at least a general idea of the nature of these idioms, and often of their mutual relations to each other. It is obvious that while all are strictly agglutinating, and so far of uniform structure, they do not stand on the same level, some being much more highly developed than others. They also agree in the general employment of pronominal suffixes instead of prefixes; but this is such a common feature that no conclusions can be drawn from it as to their mutual affinities, still less, as has been argued, for a possible relationship with the Dravidian linguistic group. Owing to their different stages of development, the grammar of some is far more regular and consistent than that of others, and the Lake Hindmarsh dialect, amongst others, is specially interesting, in its present state clearly showing the growth of true inflexion by the gradual absorption of detached pronominal elements. This will be made evident by comparing together the first and second persons singular, present, and future, of the two verbs *woarta* (to come) and *nyä-ngă* (to see) which are as under:—

Present	woartin yan woartin yar	Present	nyangan nyangar
Future	woartin yuan woartin yuar	Future	nyakinyan nyakinyar

Here we see in the first column the full pronouns *yuan yuar* of the future reduced in the present to *yan* and *yar*, while in the second column they become in both cases fused with the root. It is easy to understand from this example how the fusion might, in course of time, become the universal law, and how the language might pass thence rapidly from the agglutinating to the purely inflecting state.

Amongst the arguments here advanced in support of the view that all the Australian languages flow from a common source is one based on their generally defective numeral system. The dialect spoken near Wickliffe, Western Victoria, has distinct words for 1, 2, 3, 5, and 6; but this seems to be almost a solitary case, and it seems safe to say that as a general rule the native arithmetic is limited to the first two numerals, beyond which reckoning becomes a series of sums in addition, which even then scarcely ever gets beyond 10. A typical instance is the Lake Hindmarsh system, which runs thus: (1) Ke-yap; (2) pullet; (3) pullet ke-yap *i.e.*, $2 + 1$); (4) pullet pullet ($2 + 2$); (5) pullet pullet ke-yap ($2 + 2 + 1$), &c. It is also interesting to note the very general prevalence of the word for 2—*pullet*, *bullat*, *pulla*, *bulla*, *bolita*, *polaïl*, &c., occurring in most of the dialects all round the south and east coasts, and reaching far inland, especially in Queensland and New South Wales. This may be accounted for either by supposing that some more advanced tribe at some remote period evolved the idea of *two*, and passed it on to its neighbours, or that it had been evolved before the dispersion. In the first case it would afford no argument for the original unity of the race; in the second we should have to believe that since the dispersion scarcely a single tribe ever got beyond that

low stage of development. But this would seem to be altogether incredible, when we reflect on the immense lapse of time intervening since the dispersion, as shown by the vast accumulations of kitchen middens on many parts of the coast, and by the numerous stone implements that are constantly being turned up, some belonging to an age answering to the Neolithic, some even to the Palæolithic period of Europe. "Chips for cutting and scraping, fragments of tomahawks and pieces of black basalt are found on the low silurian ranges near the rivers and creeks in all parts of Victoria; and wherever the soil is dug or ploughed over any considerable area, old tomahawks are turned up, thus showing the immense period of time that the land has been occupied by the native race." Introd. lvii. Some of the kitchen refuse heaps are over an acre in extent, and "there are also some large shell-mounds on the coast, especially near Cape Otway, where the largest is about 300 feet long, 40 or 50 feet wide, and 16 feet high. It must have taken ages for the fish-eating natives of the coast to build up such heaps" (ii. 234). It seems inconceivable that during all these ages they should never have made a single step in advance of the numeral "two," assuming that this had been inherited from the outset. Hence the first hypothesis appearing to be the most reasonable, the argument for racial unity based on the general currency of the word for "two" falls to the ground. All the reasons for the prevalent belief in the original unity of the Australian languages are briefly resumed at pp. 43 and 44 of Introduction. None of them, except that drawn from their common phonetic system is, perhaps, very cogent; but altogether, taken in connection with other circumstances, go a long way towards justifying the general conclusion arrived at by Threlkeld, Grey, Schürmann, Moore, Bulmer, Hartmann, Hagenauer, and nearly all recent Australian philologists.

The work is rendered still more complete by a final section devoted to the Aborigines of Tasmania. Here nearly everything is brought together that is ever likely to be known regarding the physical and mental characteristics, habits, speech, implements, dress, ornaments, &c., of that extinct race. The difficult question of their origin and affinities is fully discussed, and ethnologists will feel specially thankful for the reprint of Dr. Joseph Milligan's valuable paper "On the Dialects and Language of the Aboriginal Tribes of Tasmania and on their Manners and Customs," which appeared originally in the Journals of the Royal Society of Tasmania. The importance of this contribution to Tasmanian ethnology is due to the fact that the compiler "was for many years Medical Superintendent of the Aborigines' Establishment, first at Flinders Island, and afterwards at Oyster Cove, to which the remnant of the race was removed in the year 1848" (ii. p. 480).

Mr. Smyth evidently regards the Tasmanians as belonging to a different stock from the Australians. They "are darker, shorter, more stoutly built, and generally less pleasing in aspect than the people of the continent. Their hair was woolly and crisp, and some bore a likeness to the African negro. Their aspect was different from that of the Australians. In their form, their colour, and their hair they were rather Papuan than Australian" (Introd.," lxix.). This last sentence probably goes very near the truth, and there can be little doubt that the island was peopled "by some members of the dark-skinned populations of the north" (lxxi.). Their woolly [1] or at least frizzly hair is alone conclusive as to the presence of Papûan blood. But there are, on the other hand, scarcely less clear indications of Australian affinities. The compiler himself admits that "they were not all alike," adding that "there is reason to believe that the members of some tribes were scarcely distinguishable from the Australians" (ii. 379). On the whole, the balance of evidence goes to show that they were a mixed race in which the Papûan element was predominant, and in which special features had been developed by long local seclusion.

This race is generally stated to have become extinct with William Lanney and Truganina ("Lalla Rookh"), the former of whom died in March, 1869, the latter in June, 1876, but some half-castes are still living, "and it is nearly certain that the blood will mix with that of the whites and never be lost. But the race, the traditions of the race, and the language are lost for ever" (ii. 384).

It remains to be stated that the work is well printed and richly illustrated throughout. It is also supplied with an index, which might be fuller, and with two maps on a large scale—the Australian Continent and a tribal map of Victoria. The few misprints that occur will doubtless be corrected in future editions, when the curious English sentence at p. 79, vol. ii., beginning with "However I am inclined," might also be re-cast. There seems to be also something wrong with the paradigm given at p. 30, vol. ii. of the verb *to go*, unless it be made up of three different roots (*Yangan*, *blanga*, and *plapa*); but if so, the fact should be stated. As it stands, the arrangement of tenses is about as intelligent as that of the same verb in popular English and French grammars.

A. H. KEANE

[1] "As woolly as that of any native of Guinea" (Cook); "black and woolly" (R. N. Davies); "woolly hair" (Lieut. Breton); "courts, laineux et crépus" (M. F. Péron).

ON THE MAGNETISM OF ARTIFICIAL MAGNETS

Sur le Magnétisme des Aimants Artificiels. Par V. S. M. van der Willigen. (Haarlem: Les Héritiers Loosges, 1878.)

VISITORS to the Loan Collection of Scientific Apparatus at South Kensington in 1876 will remember a remarkable series of permanent steel magnets contributed from the museum of the Teyler Foundation of Haarlem. Most of these were the work of a famed artificer of the name of van Wetteren, who during a period of thirty years has been occupied in the construction of magnets of excellent quality, under the advice and with the co-operation successively of MM. Logeman, Elias, and van Willigen. The last named of these, whose posthumous monograph lies before us, devoted himself for the last four years of his life to important researches in magnetism.

The memoir, published originally in the *Transactions* of the Teyler Museum, commences by explaining the methods adopted in fusing, tempering, and magnetising the bars of steel. A succeeding chapter describes the

methods employed in measuring the distribution and amount of their magnetism. Then come three long chapters recounting very minutely the details of the dimensions, weight, strength, &c., of no less than forty-six individual magnets, together with particulars of the successive magnetisations imparted to them. The work concludes with a discussion of results and of the formulæ for empirically representing them, and with a brief obituary notice of the author, by Dr. Figee.

It appears from the observations of the constructor, van Wetteren, that bars of steel of apparently equally good qualities in other respects will not make equally good magnets; a point which the author tells us he was unwilling to recognise until he found all the magnets fabricated from one bar inferior to *all* the magnets fabricated from a bar of what appeared to be equally good steel. English bar steel was found inferior by comparison with that manufactured on purpose by M. Wetteren, but the author confesses his inability to assign any reason for the inferiority. Concerning the details of forging and tempering a judicious silence is maintained. The method of magnetisation which was found most efficacious both for bar and horse-shoe magnets, was to place their extremities upon the poles of a powerful electro-magnet of the form constructed by Ruhmkorff for diamagnetic experiments; and then, while thus magnetised above saturation, to remove them after having applied the appropriate keeper. For magnets weighing so much as half a kilogramme an Elias ring was also applied as an auxiliary in the process of magnetisation. The maximum power was not developed until after two or three such magnetisations, the keeper being momentarily removed between each repetition. Reversal of the poles always produced consequent points. The methods of touch, the best of which the author considered to be Hoffer's method of stroking the horse-shoe magnet with a second horse-shoe of soft iron from the poles toward the equator of the magnet, he finally rejects, *in toto*, as being hurtful to the strength and regularity of distribution of the magnetism.

The most important part of the memoir is that devoted to a discussion of the portative force of magnets. Häcker has given the ratio between the portative force of a horse-shoe magnet and that of a bar-magnet of the same weight and length as two to one. M. van Willigen found the ratio with an actual magnet of Häcker to be as three to one; and with van Wetteren's magnets more than four to one. The empirical formula assigned by Bernoulli to express the relation between the weight of a magnet and its portative force is—

$$p = CR^{\frac{2}{3}},$$

where p is the weight which the magnet will sustain, R its own weight, and C a coefficient dependent on the quality of steel and other undetermined conditions. A magnet was adjudged good by the author's standard for which Bernoulli's coefficient had a value of 20 or 21; though 22·5 was occasionally attained. The empirical formula now assigned by van Willigen for the portative force of supersaturated magnetisation is—

$$P = \alpha K \sqrt{S} . \sqrt[4]{\frac{l}{\sqrt{S}}};$$

and for the permanent portative force—

$$p = \beta K \sqrt{S} . \sqrt[4]{\frac{L}{\sqrt{S}}} . \frac{L}{l},$$

where K is the perimeter and S the area of the polar surfaces, l the length of the bar, L the reduced length (or distance between the actual poles or points of maximum free magnetism), and α and β two coefficients depending on temperature, quality of steel, temper, &c. It will be seen that since for magnets of similar form the quantity $K\sqrt{S}$ is proportional to the $R^{\frac{2}{3}}$ of Bernoulli's formula, M. van Willigen has determined that factor of the coefficient which is concerned with the length of the magnet and the position of its poles. It would be interesting, though out of place here, to compare these results with those recently obtained by M. Petrowchevsky in his researches on the distribution of magnetism in magnets.

The author falls into the common error of ascribing to M. Jamin the invention of magnets made of laminæ of steel bound together in bundles. Magnets of this description were employed by Dr. Scoresby in his Arctic explorations at the beginning of the century, and may still be seen in the Whitby Museum, where they are deposited. Similar magnets were in even earlier use by Duhamel and Coulomb; and a magnet almost the counterpart of those of Jamin is described in a memoir on magnets by Geuns published at Venlo, in Holland, in 1768.

SILVANUS P. THOMPSON

OUR BOOK SHELF

Mittheilungen aus dem k. zoologischen Museum zu Dresden, herausgegeben mit Unterstützung der königlichen Sammlungen für Kunst und Wissenschaft. Von Dr. A. B. Meyer. Drittes Heft, mit Tafel XXVI.-XXXV. (Dresden: Baensch, 1878.)

DR. MEYER has now issued the third volume of his "contributions" to science from the well-filled stores of the Dresden Museum—a volume which quite equals its precursors in value and interest. The Director first gives us an account of his new cases for the exhibition of zoological objects, and supplies exact details as to their cost. These particulars may be useful for those engaged on the fittings of several other national museums which are now in process of erection. A contribution from M. Edm. de Selys-Longchamps, which follows, contains a general account of the dragon-flies of New Guinea and the Moluccas, and descriptions of a large number of new species of these insects. We have next an account of the human skeletons and skulls in the Dresden Museum, drawn up by the Director and Herr E. Tungel jointly. The number of skulls in the collection is stated to be 836. We have then an important article by our countryman, Mr. R. Bowdler Sharpe, on the collections of birds belonging to certain groups, made by Dr. Meyer during his expedition to New Guinea and the Moluccas. The groups treated of in this paper are the Accipitres, Dicruridæ, and Campophagidæ, of all of which divisions Dr. Meyer obtained a goodly series of specimens, embracing among the Campophagidæ examples of nine new species.

Dr. Kirsch, the Entomologist of the Dresden Museum, follows Mr. Sharpe with descriptions of some new wasps found in the collection, and the volume is concluded by a second portion of Dr. Meyer's memoir on the Papuan skulls of which he obtained such a splendid series during his Eastern Expedition.

It is quite evident that the present director of the Dresden Museum is not only capable of doing good work

himself, but is likewise able to extract good work out of other people—a task often more hard to be accomplished than the former one.

The Countries of the World. By Robert Brown, M.A., Ph.D. Vol. iii. (London: Cassell.)

THIS volume is devoted to Central and South America, and appears to us to present a fairly full and trustworthy and certainly interesting account of the countries of this most attractive region. Dr. Brown has evidently taken the trouble to search most of the authorities likely to help him. The illustrations to this volume are unusually good and appropriate.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

A Carnivorous Goose

I INCLOSE to you an account of a Golden Eagle, which I have reason to know to be authentic. The possibility of a bird so purely graminivorous as a goose being taught to eat flesh, and acquiring the power of digesting it, is extremely curious. It is well known, however, that cows are largely fed on fish offal in Scandinavia, and I have heard of a Highland cow devouring a salmon which an unwary angler had hid among fern on the banks of a river in Sutherland. ARGYLL

Isola Bella, Cannes, April 7

"March, 1879.—There is in the possession of W. Pike, Esq., at Glendarary, in the Island of Achil, Co. Mayo, a Golden Eagle, now about twenty-five years old, which was taken from the nest and brought up in confinement. This eagle, in the spring of 1877 laid three eggs, which Mr. Pike took away, replacing them with two goose-eggs, upon which the eagle sat, and in due time hatched two goslings. One of these died, and was torn up by the eagle to feed the survivor, who, to the great tribulation of its foster-parent, refused to touch it, together with the other flesh with which the eagle tried to feed it, Mr. Pike providing it with proper food. The eagle, however, in course of time, taught the goose to eat flesh, and (the goose having free exit and ingress to the eagle's cage) always calls it by a sharp bark whenever flesh is given to it, when the goose hastens to the cage and greedily swallows all the flesh, &c., which the eagle, tearing its prey to pieces, gives it.

"I saw them in May, 1878, when, the goose being a year old, had made a nest in the eagle's cage, and laid eleven eggs, and the two birds were sitting side by side on the nest. I hear from Mr. Pike that he did not allow them to hatch out, fearing that it might interfere with their attachment to one another.

"The eagle is very tame and fond of Mr. Pike; he goes into the cage, and it allows him to handle it as he likes, but will not allow any one else near it. It never attempts to get out of the hole made for the goose to go in and out."

Sense of Force and Sense of Temperature

THE sense of force, or of resistance to pressure, and the sense of temperature, have been very commonly confounded under one name, "sense of touch." Indeed, I think they are still imperfectly distinguished in many modern works dealing with the subject of sensation. Nevertheless, there can be no doubt as to these two being sensations altogether distinct. It is even quite probable that they are observed and transmitted by distinct nerve-systems.

An important and interesting question arises as to the kind of information given to us by these two senses; viz., how far it is merely relative, and how far these senses may, by cultivation, be made to give us absolute information.

So far as the sense of force is concerned, it is with most persons chiefly relative. Every one is prepared to say, but generally very roughly, that of two bodies, A and B, A is heavier than B. To test their relative weights we lift first the one and then the other, and decide between them. Sometimes we may go a little farther towards making an absolute estimate by means of the sense of force. I can tell, for example, that a weight is greater than 20 lbs. and less than 30 lbs. by trying to hold it out at arm's length; and most likely with a little practice I could learn to estimate weights to within closer limits than 5 lbs. on each side of 25 lbs. But such testing as this is all that is done in ordinary cases.

There is, however, a very remarkable case in which the sense of force is made absolute to a high degree by practice. It is the case of letter-sorters in the Post Office, who learn to distinguish letters that are over a particular weight with accuracy that is perfectly marvellous. It would be very interesting to try a series of experiments with letters of different weight, some slightly under weight for a particular postage, and some slightly overweighted, and to observe the errors or rather the limits of uncertainty.

The sense of temperature may also be rendered absolute to a certain extent. Several instances might be mentioned, some of which depend, as in the case of testing force by lifting the greatest possible weight in a particular way, on the limit of endurance.

One remarkable case of an absolute determination of temperature by the senses is that of the plumber and tinsmith who are in the habit of holding up the soldering bolt to the face, and judging by feeling whether it is at the proper temperature for a particular piece of work in hand.

Probably there are other cases in the arts in which the sense of temperature is cultivated to a high degree. It is in the hope of getting information on this subject through your readers that I address this note to you. J. T. B.

April 7

Did Flowers Exist During the Carboniferous Epoch?

MR. A. R. WALLACE, in his review of Mr. Allen's, "The Colour Sense" (NATURE, vol. xix. p. 501), has been misled in supposing the fossil insect from the Belgian coal-fields, named *Breyeria borinensis* may be a moth. It was originally described as the hind wing of an orthopterous insect, under the name of *Pachytylopsis borinensis* (*Comptes Rendus*, Soc. Ent. Belg. xviii. p. xli). Subsequently it was transferred to the lepidoptera on bad advice, and re-named *Breyeria borinensis* (same *Comptes Rendus*, p. lx.). Its original location was nearer the truth. I examined the fossil at Brussels in 1877, and have no doubt it belongs to the pseudo-neuroptera, family Ephemeridæ (*vide* my note to this effect in the same *Comptes Rendus* for 1877, xx. p. xxxvi.). The very dense transverse reticulation did not receive sufficient importance when M. de Borre was induced to refer it to the lepidoptera. Thus we remain without any zoological evidence that would tend to prove the existence of flowering plants in the carboniferous age. R. MCLACHLAN

Lewisham, April 4

Water-level Indicators

I OBSERVE in NATURE (vol. xix. p. 518) a description of what is stated to be a new form of water-level indicator which has lately been erected by the India-rubber, Gutta-percha, and Telegraph Works Company, at the Leamington New Waterworks.

So far as mere form goes, it possibly may be considered new, but hardly so in any other sense, as a water-level indicator, fulfilling the purposes you mention, on a very extended scale, has been in action at the Nottingham Waterworks for many months past. It is not only capable of being made to give smaller indications than one foot, but is actually doing so. This apparatus was designed and constructed in the electrical department of the General Post Office, and has given great satisfaction. I may mention that it was under the consideration of Mr. Preece so far back as the latter end of 1877, and but for his determination to have an instrument perfect in every respect before he turned it out, it might have been at work early in 1878. H. ROFE

Nottingham, April 8

Eastern Yucatan

Is there any information to be had about Eastern Yucatan? In 1847 the Maya Indians there rose against Mexico and have become independent. The animosity between them and the Mexicans is so great that there is scarcely any possibility of

penetrating to the Independent Indians of Eastern Yucatan from the western part of the peninsula, which remains Mexican. But should this not be possible from Belize (British Honduras)? I have heard that the coloured people of the colony trade with the Mayas. Would it be possible then to obtain some information in this way?

As to the interest of a visit to the Maya country by an educated traveller it would bear especially (1) on the condition of the people since they are free from their white masters. How does it compare with the condition of the Mayas of Western Yucatan, who live in a *de facto* serfdom to the large landowners? (2) The antiquities, of which we have a description by Stephens, but certainly would know more. Very likely the Mayas will allow a white man who is not a Spanish-American to travel in their country; they have no special reason to hate anybody except the latter.
A. WOEIKOF

Jurschtatskaya, 9, St. Petersburg, March 25

Deltaic Growth

IN reference to the question as to the amount of sediment brought down by Delta Rivers, I had occasion in 1877 to ascertain the amount of sediment carried by the waters of the River Plate, and found it to amount to the $\frac{1}{7762}$nd part by weight. Mr. J. F. Bateman, the well-known hydraulic engineer, in his report on the proposed harbour of Buenos Ayres, fixes the minimum flow of the River Plate at 670,000 cubic feet per second. Assuming its mean volume at 700,000 cubic feet per second (a quantity very much under the mark), it would appear that this river carries seaward some 224,000 tons of sediment every twenty-four hours—or say, in round numbers, 82,000,000 tons every year.

Some portion of this sediment is deposited in the 100 miles of river that intervene between Buenos Ayres and the sea, forming the great banks that render the navigation of the River Plate so troublesome, but a large portion is carried out to sea and deposited beyond the mouth.

I have been informed by captains of steamers trading with Buenos Ayres that the soundings shown on the chart of the coast of Uruguay vary considerably, in many places, from the actual ones now existing, and I have little doubt that a correct re-survey of this coast would show changes as marked as those discovered by Mr. Doyle near Rangoon.

The subject is one of great importance, as the coast of Uruguay is a difficult and dangerous one to make, and from the low character of the coast, the frequency of fogs, and the great uncertainty of the currents, captains have frequently to depend a great deal on the lead to ascertain their position when making this land. During the last few years several fine steamers—French, German, and English—have been lost on this coast near the Castillos, when making the land.
GEORGE HIGGIN

3, Great George Street, Westminster, S.W., April 10

Temperature Equilibrium in the Universe in Relation to the Kinetic Theory

MY attention has been called to an ambiguous phrase in my recent paper[1] on the above subject (NATURE, vol. xix. p. 460) which it is necessary to rectify. On page 461 is the sentence "*Let us suppose now the excessive temperature to fall, or, in other words, the total energy to diminish.*" This is meant as a *supposition*, not as a possible case. The imaginary rise and fall of temperature in the universe are given merely for the sake of aiding the conceptions of the actual facts, by affording *imaginary* cases to show what the effects would be if such cases were possible.
S. TOLVER PRESTON

London, April 15

Transportation of Seeds

THE penetration of seeds of the so-called "flechilla" grasses into the flesh of Australian sheep is too well known to squatters. On some "runs" these grasses are so abundant that the annual loss of stock is a very serious matter. The ripe seed falls upon the wool, and, owing to the re-curved barbules with which it is armed, easily penetrates to the skin, when, its point being as sharp as a needle, every movement of the animal tends to drive it into the flesh. I have found the internal organs so crowded with seeds that they felt like a bag of needles if squeezed in the hand.
ARTHUR NICOLS

[1] "On the Possibility of Explaining the Continuance of Life in the Universe Consistent with the Tendency to Temperature-Equilibrium."

Earthquakes

A SHOCK of earthquake was felt in this neighbourhood on the evening of Tuesday, April 8, at 8·35 (about). We were sitting in the drawing-room of this house, when we heard a sound like the rumbling of a heavy waggon, or distant thunder. It increased in loudness till the room slightly vibrated and the window rattled, as it seemed to pass the house. From the peculiar nature of the sound, and the fact that we are some 50 feet above the road, and 80 or 100 yards from it, I felt certain the disturbance was due to an earthquake and not a passing waggon, but walked to the window to listen, when I heard the sound dying away in the distance. It seemed to come from the south-east, and travel towards the north-west, and to be audible, from first to last, for some seconds, perhaps five or six, because we spoke one to another during the time. I find that the shock was noticed by other people in the neighbourhood, and that in a cottage near Bettws Gormon, a mile or so from here, two glass bottles were thrown down from a high shelf and broken.
T. G. BONNEY

Bron Celyn, near Bettws y Coed, North Wales, April 10

WE were visited by an earthquake of some violence this morning at 2 A.M. (Cadiz mean time). The shock was preceded by a profound subterraneous noise like that of a distant carriage, and it extended to Port St. Mary and Port Royal (six miles). The earthquake travelled from south to north approximately; some clocks stopped.
AUGUSTO T. ARCIMIS

Cadiz, April 3

OUR ASTRONOMICAL COLUMN

BESSEL'S NEBULA IN PERSEUS.—On November 8, 1832, in zone 527, Bessel observed an object, which he recorded as a nebula, distant about one degree from 20 Persei. It is No. 1,063 of Weisse's second Catalogue, where, though called a nebula, it has 9m. attached. D'Arrest, in his "Resultate aus Beobachtungen der Nebelflecken und Sternhaufen," has two observations, in January, 1856, to the first of which he attaches a note that no nebulosity was visible in Bessel's position, and that possibly a comet was observed; the second observation records a star 9·10m., without trace of nebulosity or diameter, the place of which was found to be within a few seconds of arc from Bessel's position, preceded 24·22s. by a star 9m., 76″ to the north. In "Siderum Nebulosorum," &c., D'Arrest remarks: "Star 9m. quæ Besselio quondam nebulosa apparuit . . . Argelandro in Perlustratione ceu fixa 9·3 magn. apparuit; nobis sæpius inspicientibus nunquam nebulosa visa." This refers to the star in the "Durchmusterung," at 2h. 43m. 56·5s. + 36° 54′·2; Argelander has another star of the same magnitude, 9·3, 10′ south. Are we to infer that Bessel's star was surrounded in 1832 by nebulosity so conspicuous that it was caught at once in his zone observations, which had wholly disappeared in 1856, or, as appears the more probable conclusion, that at the time of his meridian observation a comet happened to be centrally over the star? In this case the observation gives its place for 1832 November 8 at 10h. 10m. 25s. G.M.T.; the catalogued position for 1825·0 is in R.A. 2h. 42m. 5·56s., Decl. + 36° 46′ 46″·7.

This observation of Bessel's might at first sight appear of some interest, considering that the comet of the November meteors (1866 I.) must have been near perihelion about November 1832, but upon further examination it will be found that with the elements of 1866 it is not possible to bring the comet near the observed position of the "nebula," upon any assumption as to the time of its arrival in perihelion.

BRORSEN'S COMET.—Comparing the second of the two observations on April 4, in Major Tupman's letter pub-

lished in NATURE, vol. xix. p. 527, with Dr. Schulze's elements, only with the perihelion passage assumed March 30·5716 G.M.T., the differences from observation are $\Delta\alpha = -2''\cdot1$ and $\Delta\delta = +1''\cdot3$. This position, therefore, with others obtained by Prof. Strasser and Dr. Tempel, show that when the mean anomaly is so altered as with the other elements of Dr. Schulze's orbit to bring about an exact agreement between the observed and calculated geocentric longitudes, there is still an outstanding difference between the latitudes of from one to two minutes, which indicates that, notwithstanding the apparently careful computation of the perturbations since the comet's last appearance in 1873, the elements determining the position of the plane of the orbit are susceptible of correction. The ephemeris we gave last week will, however, amply suffice for readily finding the comet, and we shall continue it for May in our next.

Mr. Tebbutt, of Windsor, N.S.W., writes that, aided by Dr. Schulze's ephemeris, he found the comet on February 22, and observed it again in the fading twilight on the following evening. It could hardly be seen with a telescope of less than four inches aperture. It had the appearance of an elliptic nebulosity, the major axis of the ellipse extending in the direction of the parallel of declination.

NEW MINOR PLANETS.—Prof. Peters, of Clinton, New York, notifies his discovery of No. 194, on March 22, in R.A. 12h. 11m., Decl. $+9^\circ\ 31'$, magnitude 10·5. No. 192 was found by M. Palisa at Pola on February 17, and No. 193 by M. Coggia, at Marseilles, on March 1.

GEOGRAPHICAL NOTES

AT the meeting, March 22, of the Russian Geographical Society, Col. Petroussevitch read a very interesting paper on his exploration of the Amu-daria, from Chardjui, in Bokhara, to the delta of the river, and on its former beds. M. Petroussevitch has arrived at the conclusion, based on a thorough levelling of the country, that the turning of the waters of the Amu-daria into the Sara-kamysh depression through one of the former beds, would not meet with great difficulties. This depression being, however, very wide and deep, the waters of the Amu River once arrived there, would form a great lake, and it would be difficult to direct them further to the Caspian. For this last reason it would be better to open a way for the waters of the Amu along one of its former beds which run south-east from the lake Sara-kamysh. All explorations make it very probable that in this way the Amu-daria could easily reach the Caspian. The Russian Trade Society sends, next summer, an expedition for the study of the lower parts of the Amu-daria, of the best direction for a railway to Central Asia, and of the possibility of a canal between the Amu and the Caspian. Several officers of the Russian general staff, with geodesists, a geologist, a botanist, an archæologist, and an artist will be members of this expedition. They will start from the Ural River, passing through Kara-tugay, Tashkent, and Samarkand; further they will go down the Amu to the Uzboi.

UNDER the title of "L'Afrique Centrale en 1522," M. A. J. Wauters, Assistant-Secretary of the Belgian Geographical Society, has drawn up an interesting memoir, in which he has gone with much care into the doctrine of Portuguese geographers respecting the discovery of Central Africa in the sixteenth century. M. Wauters was induced to study the subject by the recent discussions in regard to the geographical data furnished by the great globe in the Lyons Library, and if anything were required to dispose of its claims to originality, this memoir does it most effectually. He traces back the idea of a great central lake, under the name of Saphat or Sachaf, to the days of Martin Hylacomilus and Gerhard Mercator, so that the data on which it was based must have been known previous to the year 1522. M. Wauters's memoir appears in the current number of the *Bulletin* of the Belgian Geographical Society, and is illustrated by a facsimile map.

A LISBON paper gives the text of a letter which Major Serpa Pinto addressed to Sir Theophilus Shepstone from Shoshong, Bamangwato country, on January 2, and which adds some information to that already made public respecting his adventurous journey. He states that he went beyond the Zambesi and purposed proceeding to the east coast through the country of the Choculumbes, when successive obstacles obstructed his passage. Having lost all his resources and being abandoned by his carriers, he found himself in the greatest difficulties, when fortunately he heard of a missionary who had arrived at the Upper Zambesi, and he resolved upon finding him. After a journey of 200 miles he found the missionary, M. F. Coillard, a Frenchman of the Evangelical Mission of Sesuto, Basuto-land, director of the station of Lesibo. His strength being exhausted, Major Pinto was taken seriously ill, but on his recovery succeeded in reaching Shoshong with M. Coillard and accompanied by eight of his followers, the only ones who continued faithful.

THE Danish Government has appointed Lieut. Jensen to explore all the fjords in Greenland from Holsteinborg to the coast facing Disco. The explorations will bear on the moving ice-fields which send so many icebergs into the Polar Ocean.

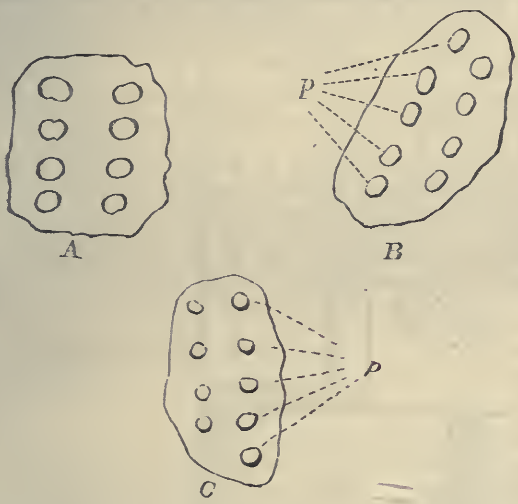
MR. IM THURN paid a second visit to the Kaieteur Falls, on the Potaro River, British Guiana, in February and March last, when the river was in full flood. Referring to our remarks on his previous visit, he states that he by no means intended to depreciate the grandeur of the fall. At his last visit he found it "so infinitely more grand, so infinitely more beautiful, that it is painfully hopeless to try to express in words anything of its beauty and grandeur." Mr. im Thurn's brief account contains several interesting notes on the botany and zoology of the region traversed.

THE WOLF FISH

OF late the wolf fish (*Anarrhichas lupus*) has been somewhat plentiful in the Frith of Forth. A specimen which lived in the Edinburgh Aquarium for a little over a week, came into my possession a few days ago, and I have thought that an account of the dental armature of this curious fish may prove interesting to readers of NATURE. The wolf fish is a near relative of the Blennies. In this fish we see the same elongated dorsal fin, and the same anal fin as in the Blennies; but the dental arrangements of the wolf fish are of a much more specific and unusual kind than are seen in the former group. The specimen dissected measured twenty-five inches from the top of the nose to the extremity of the caudal fin. It was therefore by no means a large specimen, since from six to seven feet is not an unusual length for the sea wolf to attain. Its dark grey body was faintly banded with brown, but the head was beautifully and distinctly marbled with black on a grey ground. The aspect of the mouth, provided with its well marked teeth, partakes somewhat of a feline look, and has suggested the name "sea cat," often applied to it on these northern shores, where the fish is frequently eaten, it somewhat resembling the cod in taste. The appearance of the mouth is highly characteristic. In front of both jaws is found an array of sharp incisor teeth. The upper jaw bears five of these pointed teeth, the two lateral teeth being large, and the central three teeth small. The lower jaw possesses six teeth of similar pattern, the two central teeth being larger than the four lateral ones; and when the jaws are closed

the lower teeth interlock in an exact manner with the upper. The hold or grip of a wolf fish must therefore be of a tenacious kind. Behind these incisor teeth, both above and below, are seen a few small teeth, destined by the ordinary laws of dental succession in the fish-group to replace the incisors in case of injury or loss. These front teeth are firmly anchylosed to the bones on which they are borne.

More interesting are the palatal teeth, and the corresponding teeth of the lower jaw. To these latter, the name of "molars" or "grinders" is frequently applied. Close to the front of the upper jaw we find a series of three tooth-masses, one central and two lateral, arranged in diverging fashion. The central and largest mass resembles the tuberculate molar of a bear in form, and is composed of four firmly united segments, each segment in turn consisting of two pieces. The lateral teeth of the palate, similarly consist of a double series of firmly united segments, but in each of these lateral pieces the outer row of pieces is composed of sharp-pointed segments, resembling miniature incisors. The accompanying diagram will afford an idea of these curious palatal arrangements:



A is the central piece; B and C are the lateral pieces, the outer teeth of which (*p p*) consist of pointed and incisor-like pieces. It follows from this description that the sea wolf possesses in its mouth an apparatus not merely adapted for tearing its food but for exercising a triturating and bruising action as well.

No less characteristic are the dental arrangements of the lower jaw. In the front of this jaw are four incisor teeth, each fully three-quarters of an inch in length; whilst two smaller incisors exist as already mentioned, one at each side of the larger series. Behind these incisors are the rudiments of succeeding teeth, and these rudimentary teeth gradually merge into the main dental arrangement of the lower jaw, which consists of a prominent row of blunt teeth anchylosed to form a common mass, and partially forming a double row on each side of the jaw. Section of the jaw shows that the teeth are imbedded in a common groove, and that complete and thorough ossification of the various dental pieces renders the whole apparatus compact and solid. The arrangement seen in the mouth of the wolf fish suggests the idea of the high specialisation of this type of fish, as indicated by the development of the dental apparatus. In none of the near neighbours of this fish have we at all a near approach to the perfection of teeth thus exhibited; and in respect of its complexity and differentiation of type, we may well be inclined to lend some countenance to the idea of the independent origin in widely removed fishes of structures seen in still greater perfection in such widely-removed fishes as the Elasmobranchiate Skates, Rays, and Cestracion.

The stomach of the specimen I dissected was greatly distended, and contained fully four ounces of digestive *débris*, consisting chiefly of disintegrated Ophiuroids, spider-crabs, broken shells, shrimps and prawns, along with sand and small gravel. The pyloric aperture was firmly contracted and the collection of matter in the stomach clearly pointed to some obstruction of the digestive canal as the cause of death. It was also instructive to find that close to the vent the rectum was largely distended with broken pieces of shells and fine gravel. These matters, along with those in the stomach, had evidently been intussuscepted before the arrival of the fish in the aquarium and probably caused death by the irritation consequent on their non-removal by digestion. ANDREW WILSON

THE ETNA OBSERVATORY

IT will be within the recollection of some of our readers that in September, 1876, Prof. Tacchini, of Palermo, communicated to the Accademia Gioenia of Catania a letter, "Sulla Convenienza ed utilita di erigere sull' Etna una Stazione astronomico-meteorologica" (*vide* NATURE, vol. xv. p. 262). This letter was published in the *Atti* of the Academy, and afterwards appeared in the form of a quarto pamphlet with ground-plan and elevation of the proposed building. The project was at once taken into consideration both by the Italian Government and by the Municipality of Catania; plans were prepared, money was voted, and it was confidently believed that the observatory would be commenced in July, 1878. Owing, however, to certain delays, this was found to be impracticable, and the commencement was postponed till June, 1879. There is every reason to believe that the building will be erected and the instruments in working order by the end of this year. The cost will be borne by the Government, the Municipality of Catania, and the Province of Catania. Merz, of Munich, has offered to construct a 12-inch lens for the great refractor, at the price of a 10-inch lens, and the enterprise has received encouragement and support from various sources both at home and abroad.

The observatory will be erected at the Casa degl' Inglesi, 9,652 feet above the level of the sea. At the present time the Casa is an oblong building constructed of blocks of lava, and containing three rooms (*vide* the accompanying plan). It was built by the English when they occupied Sicily in 1811, and has since been used by those who ascend the mountain as sleeping quarters. A few years ago it had fallen into decay owing to the accumulation of snow in winter and to other causes, but it was put into complete repair in 1862 on the occasion of the visit of the present King of Italy. The observatory will be the property of the University of Catania, and will indeed be a kind of offshoot of the Bellini Observatory of Catania. It is to be devoted not only to astronomical and spectroscopic observations, but it will also be furnished with a complete set of meteorological and seismological instruments. Between the Etna Observatory and Catania three or four meteorological stations will be established at different elevations, as at Nicolosi, and the Casa del Bosco, and observations will be made at the same hour daily at each of these stations, at Catania, and at the Etna Observatory.

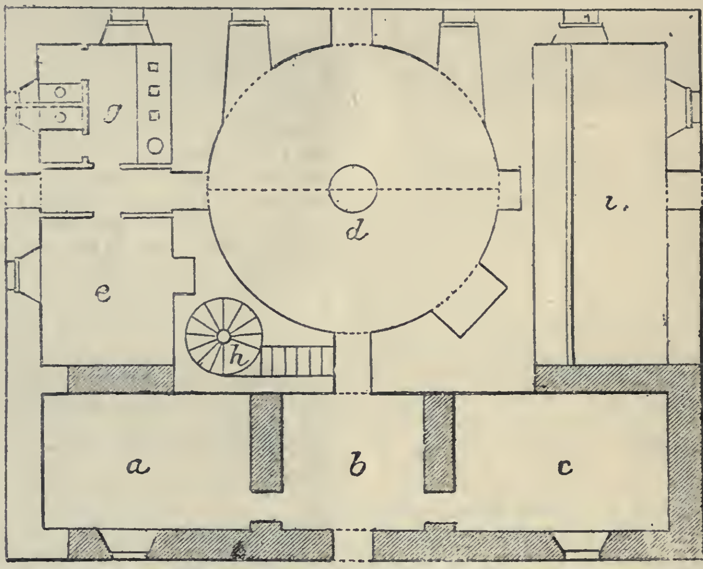
The Merz lens of 12 inches diameter, has a focal distance of $5\frac{1}{2}$ metres. The telescope and clock-work movement are in course of construction by Signor Carignata, the mechanician of the Padua Observatory, who constructed the instruments which were employed by the Italian astronomers who went to India to observe the transit of Venus in 1874. The observatory will only be inhabited during the months of June, July, August, and September, and the large lens will then be transported

to Catania, and fitted to a duplicate mounting in the Bellini Observatory. But a number of self-recording instruments will remain in the observatory, and thus results will be registered during the winter months.

Prof. Tacchini, to whom the whole undertaking owes its existence, specially desires that it should be an International Observatory. With this object in view, the large telescope will be fitted with a second tube, the length and aperture of which can be altered at will, so that astronomers from any part of the world can bring with them their objectives and eye-pieces, and can fit them to the telescope of the observatory. Special arrangements will be made for photographing the sun and moon, and for spectroscopic observations.

The observatory will contain the large telescope covered by the usual dome in the centre; on each side there will be rooms for the other instruments, and below, sleeping quarters, a dining-room, and kitchen for the use of travellers. The following plan, for which, together with many of the above facts, we are indebted to Prof. Tacchini, will show the general arrangement of the observatory.

In his report on the subject Prof. Tacchini commences by pointing out that since the year 1868 the study of the physical constitution of the sun has made very rapid progress. In these studies the spectroscope has played a very important part. But the spectroscopic observations are often hindered, and their exactitude is disturbed by atmospheric causes, and these disturbances are mainly due to the lower layers of the atmosphere. Hence an observatory at a considerable elevation would greatly facilitate such observations. The isolated Mount Etna affords an admirable locality for such an observatory. The blueness of the sky is intense, the stars shine with an extraordinary brightness, Venus casts shadows, spectroscopic lines which cannot be seen at the Palermo Observatory are perfectly distinct when viewed from an elevation of 10,000 feet. "Ora il mio desiderio," says Tacchini, "sarebbe quello di andare sull' Etna per verificare la tanto decantata purezza del cielo e il suo colore speciale, osservare l'aureola del sole, studiarne lo spettro se sarà possibile e fare anche qualche tentativo di fotografia. . . ." He then mentions some of the observations made by Young with a 9-inch refractor at an elevation of 2,800 metres, and describes his own observations made on Etna with a Dollond refractor of 99 mm. aperture,



a, b, c, the three rooms of the already existing Casa Inglesi; *d*, circular chamber, 8 metres in diameter, with a massive pillar in the centre, upon which the great refractor will be placed; *e*, a room for guides who accompany those who ascend the mountain; *g*, kitchen and offices; *i*, stable for mules; *h*, staircase. On the upper floor, *a, b, c*, will be bed-rooms; *e, g, i*, instrument-rooms; and *d*, the circular chamber for the refractor, extending through both floors, and surmounted by a movable dome of iron.

and a spectroscope of great dispersive power constructed by Tauber. "La cromosfera era magnifica e dettagliata abbastanza tenendo conto della piccolezza del magnesio e dello 1474, ciò che non vidi a Palermo collo stesso cannocchiale." Then having made the suggestions which are in part carried out, and which have entirely been adopted, he concludes by a fervid peroration which we heartily endorse:—"Io ritornerò dunque all' Etna, lo spero, e in migliore stagione, e sin d'ora mi figuro coll' immaginazione, la vista della nuova specola, che mi ricorderà l'epoca fortunata e la circonstanza solenne, che qui mi tiene ora unito a voi, e il nome del grande Bellini, che prima ancora di dare alle scene il suo capo lavoro, la Norma, volle in omaggio alla scienza degli astri, suonare all' osservatorio di Palermo la celeste melodia della preghiera alla casta Diva."

While we feel perfectly convinced that important results will accrue to more than one science from the establishment of the new observatory, we must not shut our eyes to the fact that many difficulties will have to be encountered. The observatory on Vesuvius stands upon a projecting spur of rock, and lava-streams of any ordinary magnitude breaking out on this side of the cone would divide, and leave the observatory as on a rocky island. But the Etna Observatory will stand near the upper termination of the Piano del Lago, the plain out of which the great cone of Etna rises. A great eruption, leading to the breaking down of the cone and flow of lava in this direction, could not fail to overwhelm the Observatory. Fortunately the lava usually finds vent by a *bocca del fuoco* on the sides of the mountain below the great cone. The building must be of very great strength; it will be subject to violent shocks of earthquake, to fierce storms of wind, and to the accumulation of great masses of snow upon its dome and roof. Those who have read the history of the mountain know with what tremendous power it scatters its terrors abroad; how hours of loud bellowings and detonations are followed by days of violent earthquake, and weeks during which many square miles of country are inundated by millions of cubic yards of molten lava. That the new Observatory may flourish, unassailed by the fearful forces of the imprisoned Cyclops, will be the wish of all our readers.

G. F. RODWELL

NOTE ON THE SPECTRUM OF BRORSEN'S COMET

On April 1 and 2 I succeeded in determining the position of the green band in the spectrum of Brorsen's comet. The spectrum was so faint that the other bands could not be measured. The instrument was the 9½-inch equatorial of our astronomical laboratory, armed with a one-prism spectroscope. The observations were made by bringing an occulting bar, movable by a micrometer screw, into such a position that the well-defined lower (less refrangible) edge of the band in the comet spectrum should be just visible as a thin line, the rest of the band being hidden by the bar. After the pointing the flame of a Bunsen burner was brought in front of the slit, and the position of the band in the comet-spectrum was thus fixed.

It was found by four independent pointings (which all agreed within about the interval of the *b*-lines) that the central band of the spectrum of the comet coincided precisely (within the limits of perception) with the green band in the flame of the hydrocarbon.

The interest of the observation lies entirely in the fact that it seems irreconcileable with the result obtained by Mr. Huggins in 1868, who found for the same comet a spectrum having bands distinctly different in position and appearance.

According to my observation, the spectrum of Brorsen's comet no longer stands out as exceptional, but agrees with that of other comets.

The comet itself appears in my telescope as a small round nebulosity, about 40″ in diameter, without definite nucleus, but much brighter in the centre, easily visible in the finder of 3 inches aperture, and about equal in brightness to a star of the 7th or 8th magnitude. Before the new moon a straight narrow tail, about half a degree long, was faintly visible with a low power.

C. A. Young

Princeton, N. Jersey, U.S.A., April 5

NOTES

We learn that Congress has sanctioned the scheme for the reorganisation of the American Surveys recently commented upon in these columns. It is understood that the Geological Survey will be placed under the control of Mr. Clarence King, who has so long had charge of the Geological Exploration of the 40th Parallel. But no details have yet reached us.

In the Paris Academy of Sciences, M. Alphonse Milne-Edwards has been elected a member in place of the late M. Gervais, in the Section of Anatomy and Physiology; M. Abich a correspondent in the Section of Mineralogy, in place of M. Damour; and Mr. Lawes a correspondent in the Section of Rural Economy, in place of the late Marquis de Vibraye.

The following are the probable arrangements for the Friday evening meetings at the Royal Institution after Easter:—April 25. Francis Galton, F.R.S.: "Generic Images." May 2. Prof. John G. McKendrick, M.D.: "The Physiological Action of Anæsthetics." May 9. Sir John Lubbock, Bart., M.P., F.R.S.: "The Habits of Ants." May 16. Prof. A. Cornu: "Étude Optique de l'Élasticité" (in French). May 23. W. H. Preece, M.R.I.: "Multiple Telegraphy, or Duplex and Quadruplex Telegraphy." May 30. Grant Allen: "The Colour-Sense in Insects; its Development and Reaction." June 6. Prof. Dewar, F.R.S. June 13. Frederick J. Bramwell, F.R.S.: "The *Thunderer* Gun Explosion."

A catalogue of the library of the Museum of Practical Geology and Geological Survey has been lately published, compiled by Messrs. H. White and T. W. Newton, which cannot fail to be of use beyond the walls of the library of which it is a record. The arrangement is alphabetical, the author's name and important groups of works, as "Geological Surveys" and "Statistics," being printed in black thick type, secondary titles and subdivisions in italics. The pages are clear and easily read and the titles full and accurate. The price, considering that there are over 600 pages, and that only 270 copies appear to have been printed, is somewhat less than that which is generally fixed on the publications of this Department. When the Geological Survey was instituted in 1843, its first Director, Sir Henry De la Beche, C.B., commenced the formation of a museum illustrative not only of the palæontology of the country, but of the economic application of geology to the arts and manufactures. These collections were exhibited in a small building in Craig's Court, and wishing still further to foster geological study, Sir Henry presented to the Survey the whole of his scientific library. This was added to from time to time, until in 1851 these collections were removed to the Museum of Practical Geology, which was built for their reception, and to provide accommodation for the Royal School of Mines, which was then instituted, and to a certain extent associated with the Survey. Since this time, partly by the annual grant from Parliament, partly from the gifts of the various scientific societies of the world, and partly by the bequest of the late Sir Roderick Murchison's library, it now numbers no less than 28,000 works, on the special subjects taught in the School of Mines, geology, mining, chemistry, biology, &c., and it is specially rich in foreign transactions, and the works of reference useful to a mining engineer.

Lord Dufferin, who had accepted the presidency of the Birmingham and Midland Institute, having been compelled to relinquish that office on his appointment to the Embassy at St. Petersburg, Prof. Max Müller was communicated with by the Council, and has signified his acceptance of the post.

The publication of weather warnings in Switzerland will begin on May 1 in Zurich, and on June 15 in Geneva. A telegraphic despatch, containing a description of the weather in Europe, with weather warnings, will be sent from the observatory every day, by telegraph, to any person who will pay quarterly 4*l*. 8*s*., and a shorter bulletin containing only a weather prognostic, will cost 1*l*. for three months.

Swiss papers are much alarmed by a case of infection by birds. Two brothers, merchants at Uster, in the canton of Zurich, who have a large collection of various birds and monkeys, lately received some tropical birds which were sent in a cage from Buda-Pesth. Immediately after the arrival of the birds the two brothers, the wife of one of them, and a shop-girl became sick. A third brother, who is a surgeon, understood the cause of the illness and ordered all suspicious birds to be killed, fifty or sixty in number, the cages to be destroyed, and a strong sanitary cordon around the house to be established. A tinker who had done some repairs to the cages also became sick and died in hospital, as well as an innkeeper and his wife, at whose inn the birds stayed for some days. The number of sick already has reached eight, and their state is very bad. The illness is described as a black typhus.

The third fascicule of the "Pflanzenleben der Schweiz," by M. Christ, has just appeared, and contains a very fine map which illustrates the subdivisions of the Swiss flora.

A committee has been appointed by the Paris Society of Photography to collect funds in order to erect a statue to Nicephore Niepce, who was born at Chalons-sur-Saone in 1765. The subscriptions are to be sent in Paris to M. Pector, 9, rue d'Albe, or M. Koziell, 20, rue Louis-le-grand.

Mr. Clifton Ward's papers on the physical history of the English lake-district have been reprinted in a separate form from the *Geological Magazine*.

M. Marie Davy, the director of Montsouris Meteorological Observatory, has set up a registering Thomson electrometer. The indications are recorded by photography in accordance with the system which has been established at Kew by the British Association. A comparison of readings taken by these two establishments will render essential services to the progress of meteorology. M. Descroix, the observer in charge of this instrument, was assailed before the Meteorological Society by a French *savant* who has invented another electrometer, in which the registration is taken with a pencil and mechanical force. But the discussion proved that such instruments are useless without knowing whether their indications are worth being recorded, as many objections can be raised against their use and have been published by competent electricians.

The French artists are preparing for an Exhibition of Fine Arts, to be held as usual in the Palais de l'Industrie from the beginning of May. When this exhibition is over a scientific exhibition will be organised by M. Nicolle, the successful organiser of the Exposition Maritime et Coloniale held in the same building in 1877. M. Jules Simon, M. du Moncel, and others are taking an active part in the preparations. The chief peculiarity will be the execution of all the scientific experiments capable of being made before an audience and on an unusual scale. The organiser will try to fill the lacuna, which was so much complained of in the last Champ de Mars exhibition.

A correspondent of the *Hiogo News* mentions that in the city of Kioto there are to be seen many semi-foreign buildings which are the Shogakko, or elementary schools. Of these there are 64 in the city, and 445 in the whole *fu*. Kioto is divided into two large districts, called Kami and Shimo Kiyo, which are subdivided respectively into 33 and 32 *ku*. Each *ku* is obliged to establish and maintain one of these schools, except in the case of poor *ku*, when two are allowed to unite and form one school district. Besides the usual Japanese course of studies, the pupils are taught the elements of foreign mathematics, history, geography, and philosophy, and they are also trained in gymnastic exercises. Education is compulsory, only those (who are of age) being exempted who are necessary to the support of their parents.

We learn that the organisation of the captive balloon has been altered by M. Giffard. The aëronautical and scientific staff has been constituted by him into a private company. He has placed at their disposal the funds required for the working of the balloon, and placed the enterprise in their own hands on their own responsibility and care. This arrangement has been agreed to by the public authorities, and will leave a handsome surplus on behalf of the men who spend their lives in the aëronautical career, as a large part of the profits, exclusive of a handsome weekly pay, is to be divided by equal part amongst them. A few important alterations have been made in the car, and ring, and netting. The net ascending power will be enlarged by a ton. A kind of india-rubber gas-meter, containing 100 cubic metres, has been constructed, in order to render inflation instantaneous between two ascents.

The Académie des Ascensions Météorologiques opened its Museum in Paris on April 3. More than 600 persons visited the gallery. It is to remain open every Thursday, and any day admission may be obtained by letter to the General Secretary, 50, rue Rodier. The number of exhibitors exceeds 120. Amongst the collection of aëronautical medals we note some commemorating ascents made in England in the end of the last century. A registering barometer for ascents to 5,000 metres is exhibited by MM. Richard frères; it was purchased by the government for the Meudon aëronautical establishment. M. Egasse exhibits an apparatus for filling balloons with hydrogen extracted by the action of zinc and chlorhydric acid. The result of the reaction is employed in large quantities for destroying miasmatic influence in barracks and other similar establishments. A new paddle has been constructed for ascending and descending without gas or ballast when the balloon has been placed on equilibrium with surrounding air, a new guide rope for diminishing the velocity by friction when the balloon is travelling at sea, a safety-car for these perilous occasions, differential valves, &c. Lectures will be delivered and ascensions executed by the members of the institution.

The *Cologne Gazette* reports, under date the 9th inst., a slight shock of earthquake at Buir and Elsdorf. The shock is stated to have been noticed at five minutes past midnight. It had a rolling noise and took an east-south-easterly direction.

The *Tiflis Messenger* says that a strong earthquake was felt on March 27 in Persia, at the Gulf of Miama.

A general exhibition of the various systems and apparatus used for electric lighting is to be held in the Royal Albert Hall in May. An inaugural lecture, at which the Prince of Wales has promised to be present, will be delivered in the evening of Wednesday, May 7, by Mr. W. H. Preece.

As an agitation prevails in France against the laws proposed by M. Ferry for prohibiting the members of unauthorised bodies from taking any part in public instruction, it is very likely that Government will retain the exclusive right of conferring degrees, but the proposed restrictions will be rejected by Parliament.

The following details (published by Herr Landauer in the *Reports* of the Berlin Chemical Society) regarding the behaviour of safranine when spectroscopically examined, may interest the readers of our recent article on "Absorption Spectra" (vol. xix. p. 495). It is well known that the salts of safranine show the remarkable reaction that the red colour of their solution turns into violet, indigo-blue, bluish green, and finally emerald green, upon addition of concentrated acids, particularly of sulphuric acid. The change of colour takes place in the opposite direction if safranine is dissolved in strong acids and water is gradually added to the solution. If these solutions are examined with the spectroscope, then it appears that each of the colours mentioned shows a separate spectrum. The green solution absorbs the violet, blue, and red rays, the bluish-green one behaves in the same way, but lets a part of the red rays pass; the blue solution absorbs only the yellow rays, and the more the colour of the solution turns violet and red through the addition of water, the more the absorption passes over to the green part of the spectrum. Herr Landauer attempted to determine the cause of these changes of colour in the liquid, and the corresponding alteration of the spectrum. Finding it difficult, however, to isolate the various chemical compounds formed in the liquid, it was only possible to try an explanation from the chemical behaviour of the liquid itself. Finally he arrived at the conclusion that of the two possibilities, formation of different acid salts, or of different hydrates of the same salt, the latter is the more probable one. The change of colour takes place also when the solution is evaporated, and reappears in the opposite direction when water is again added. The sulphate is the most characteristic salt of all; blotting-paper impregnated with a solution of this salt, and dried by heating until it has become green, soon becomes blue by attracting moisture from the atmosphere, and a drop of water thrown upon it instantly gives a red stain with a blue margin. Herr Landauer supposes that there exist three different hydrates (or two hydrates besides an anhydrous salt), which are red, blue, and green respectively, the violet and bluish-green tints resulting from mixtures of these. Herr Vogel's maxim, therefore, that the rule "each body has its own spectrum" can be admitted only with restrictions, may be still further restricted, inasmuch as it may be asserted that

absorption spectra only indicate the component parts of a compound so long as the colour of a given substance is characteristic of its chemical composition.

WE notice an interesting discussion which arose at the last meeting of the Russian Society of Hygiene. M. Malarevsky, pointing out the yearly increasing myopy of scholars, proposed to print books with white letters on a black field, and proved the superiority of this system by experiments he has made with fifty scholars, as well as by experiments on the facility of discernment of black drawings on a white field and of white drawings on a black field, these last always being better seen from a greater distance than the former.

WE have received a very full Catalogue of Official Reports upon Geological Surveys of the United States and Territories and of British North America, by Mr. F. Prime, assistant-geologist of Pennsylvania. It seems to be an enlarged continuation of the Catalogue by Prof. O. C. Marsh, published in the *American Journal* for 1867. The present list covers 50 pp.

AT the last session of the U.S. Congress an appropriation of 250,000 dols. was made for the construction of a fire-proof building for the reception of such collections belonging to the National Museum as cannot be at present accommodated in the Smithsonian Building; and as the plans have already been prepared, it is understood that the work will be begun without delay. The design contemplates a building 301 feet square, with certain projecting corners, the whole covering a space of about 97,000 square feet. Although not quite equal to the area of the Government Building at the Centennial, it is capable of containing a much larger mass of material. The general plan is that of a pavilion, of one story, with brick walls and iron roof, the floor to be of concrete. The corner buildings or projections constitute offices connected with the administration of the Museum, to include a library room, a small lecture-room, and others. It is expected that the entire edifice will be completed and ready for occupation by April 1, 1880.

THE whale whose bones have been so long exposed in the court-yard of the Jardin des Plantes at Paris, is to be demolished in compliance with a report from M. Quatrefages, who shows that the original number of vertebræ has been enlarged, and a series of important alterations have been successively made.

THE publishing office of *Science News*, hitherto published at Salem, Mass., will shortly be removed to New York.

MANY of our readers might like to know of Dr. Karl Möbius' address on March 5 last, on his assumption of the rectorship of the University of Kiel. Its subject is the passage from Goethe—"Leben ist die schönste Erfindung der Natur, und der Tod ist ihr Kunstgriff viel Leben zu haben."

THE Second Annual Report of the Board of Trustees of the Western Pennsylvania Institution for the Instruction of the Deaf and Dumb is one of great interest, and we are sure would be perused with pleasure and profit by all who are interested in the important subject. The Institution seems to be conducted on thoroughly scientific principles, and its success seems very marked. The Report is published by Stevenson, Foster, and Co., Pittsburgh.

WE have received a cheap edition of "The Caves of South Devon and their Teachings," by Mr. J. E. Howard, F.R.S., in which he endeavours to combat the long chronology assigned to the human race by Mr. Pengelly and others. Hardwicke and Bogue are the publishers.

THE *North British Daily Mail* of March 29 contains reports of recent meetings of the Geological and Natural History Societies of Glasgow. In the former Mr. Young gave a descriptive notice of an interesting specimen of *Elephas primigenius* discovered about four years ago when sinking a pit-shaft on Mainbill Farm, near Baillieston, east of Glasgow. In the latter Mr. Harvie-Brown read a paper on the Mammalia of the Outer Hebrides.

"NEW Views in Astronomy, illustrated by Working Models and Diagrams, and Demonstrated by Inductive Philosophy," is the title of a quarto pamphlet by Mr. John Harris, published by Wertheimer, Lea, and Co., of Finsbury Circus.

THE additions to the Zoological Society's Gardens during the past week include a Yak (*Bison grunniens*) from Bhootan, presented by the Hon. Ashley Eden, K.C.S.I.; a Japanese Goat Antelope (*Capricornis crispus*) from Japan, presented by Mr. H. Pryer, C.M.Z.S.; a Rhesus Monkey (*Macacus erythræus*) from India, presented by Mr. J. Roberts; a Grivet Monkey (*Cercopithecus griseo-viridis*) from West Africa, presented by Mr. W. B. Greenfield; a Common Seal (*Phoca vitulina*), British Isles, presented by Capt. Chas. Rawle; a Red-throated Diver (*Colymbus septentrionalis*), British Isles, presented by Mr. J. S. Thompson; a Masked Parrakeet (*Pyrrhulopsis personata*) from Fiji, an Entellus Monkey (*Semnopithecus entellus*), from India, deposited.

THE RESULTS OF RECENT RESEARCHES IN ANIMAL ELECTRICITY[1]

I.—INTRODUCTORY OBSERVATIONS

The State of the Subject Ten Years Ago

UNTIL the year 1867 certain theoretical conceptions based upon the classical investigations of du Bois-Reymond prevailed in the whole department of animal electricity. It may indeed be said that du Bois-Reymond created this branch of modern physiology, for it was he who first struck out the modern method of research and gave the lead to recent investigation. To his individual labours we owe not only the establishment and the orderly arrangement of many facts which had been left ill-defined by his predecessors, but also the actual discovery of a still greater number of fundamental value. These fundamental facts, briefly summarised, were the following:—

1. Muscular fibres and nerve-fibres when cut across exhibit a current directed within them from the transverse to the longitudinal surface, the electromotive force of which may equal one-twelfth of a Daniell's cell.

2. The negative potential of the transverse section also belongs, though in a less degree, to the natural terminations of the fibres of muscle ("natural transverse section"); but in this case it may be wanting, or even be changed to a positive potential. This diminution, absence, or reversal, of the common condition, to which the term "parelectronomic state" is applied, would seem to be favoured by the continued action of cold.

3. If a certain portion of a nerve-fibre be traversed by a galvanic current, the remaining portions, or the extra-polar regions, become the seat of an electromotive force which has the same direction as the traversing current, and which is strongest in the vicinity of the poles (electrotonus). This influence extends only so far as the structural integrity of the fibre is complete.

4. Muscles and nerves with artificial transverse sections exhibit during the period of stimulation a diminution ("negative variation") of their proper current. In uncut muscle the negative value of the diminution or variation becomes algebraically added to whatever current may be already present in the natural termination of the muscle.

Upon these facts du Bois-Reymond had based the following theory:—

1. Muscle- and nerve-fibres contain electromotive molecules suspended in an indifferent conductor, which present positive surfaces to the longitudinal surface or section, and negative surfaces to the transverse surface or section.

2. At the natural terminations of the muscular fibres are arranged particles of a peculiar kind, the presence of which is

[1] A lecture delivered on February 2, 1878, before the Medical Society of Zurich, by Dr. L. Hermann, Professor of Physiology in the University of Zurich, and published in the "Vierteljahrsschrift d. naturf. Ges. in Zurich," 1878. All the papers referred to in this lecture which have no author's name attached are papers of the author himself.

more or less marked, and which present positive instead of negative surfaces towards the ends of the fibres. Cold, according to the theory, favours the development of this "parelectronomic layer."

3. The electrical molecules of a nerve, under the influence of a current which traverses it, assume a new arrangement which consists in their turning negative surfaces towards the positive pole and positive surfaces towards the negative pole. If we conceive these molecules as electrically dipolar in the state of rest, they would be arranged in inseparable pairs, the positive pole of each member of a pair being turned to the positive pole of the other, the negative surfaces of the molecules being all turned to the transverse sections; and the action of the current would consist in an arrangement of all the molecules in the form of a pile. As this rearrangement of the molecules extends in a less degree beyond the portion of nerve traversed by the current, electrotonic forces are developed.

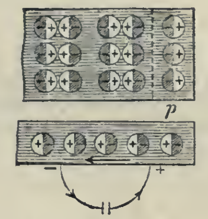


FIG. 1.

In Fig. 1 the upper diagram represents the normal, the lower the electrotonic, arrangement of molecules; above *p* are seen the parelectronomic molecules of the natural termination of the fibres.

4. During stimulation either the electromotive forces of the molecules diminish, or the latter assume a new arrangement, in which they appear less active externally; but the parelectronomic molecules take no share in these changes.

The promulgation of this molecular theory aroused many expectations. It appeared probable that the essence of the condition of activity and of its conduction in muscle and nerve was connected with the electrical properties of certain particles, rotation, oscillation or some other change in one particle leading to similar changes in neighbouring particles. Moreover, it appeared conceivable that even the contraction of muscle might be explicable by the actions of attraction and repulsion among the same particles.

Although speculation upon the origin of these electro-motive forces in muscle and nerve had hardly taken shape, and although the founder of the molecular theory observed a praiseworthy reserve in regard to all such speculation, yet up to the year 1867 it was pretty generally, if tacitly, assumed that the chemical processes occurring in muscles and nerves at rest were essential to the maintenance of the electromotive molecules in constant activity. In a similar way it was taken for granted that excitation depended ultimately upon certain movements of the electromotive molecules which were, of course, associated in some manner or other with increased consumption of oxygen and increased oxidation.

Researches which called forth New Views

Researches which I made upon the exchanges of the gases and other constituents of muscle [1] furnished me with results which differed materially from the then prevailing conceptions of the functional processes occurring in these organs. I found that muscles contain no oxygen capable of being yielded to a vacuum, and nevertheless that they are capable of prolonged exertion in a medium entirely free from oxygen; hence I concluded that the chemical process which underlies muscular work is not a process of oxidation, but a process of decomposition, in which, by the saturation of stronger affinities through the passage of atoms into more stable atomic combinations, energy is set free, just as in the alcoholic fermentation of sugar.[2]

Amongst the products of this decomposition in muscle there occurs carbonic acid. A comparison of the quantities of carbonic acid set free during muscular exertion, and during muscular rigor, showed that in both cases the carbonic acid must spring from the same source, and thus, with the help of an analogous result to which J. Ranke had arrived in the case of lactic acid, a complete analogy between the chemical processes of *contraction* and of *rigor*, was found to hold good. Both processes are processes of decomposition, and as products of the decomposition are known carbonic acid, lactic acid, and in the case of rigor an albuminous coagulum discovered by Brücke and Kühne, which may perhaps in the future be ascertained to be a transitory phenomenon even in the case of contraction. The absorption of oxygen by muscle has nothing to do with this process of decomposition; the former is related to a synthetical process of restitution in which certain of the products of decomposition are perhaps again utilised. In this way was explained the independence (in point of time) between the absorption of oxygen and the production of carbonic acid by muscle.

According to this conception there occurs, during the condition of rest, a perpetual but slow decomposition and a slow process of restitution; the latter is dependent upon a supply of blood containing oxygen. If the supply be intercepted, then the whole store of decomposable matter within the muscle becomes exhausted, and the muscle passes into rigor. During contraction the process of decomposition is suddenly quickened, and the process of restitution can barely keep pace with it: when the latter lags behind the former *fatigue* results.

The analogy between contraction and rigor had attracted attention long before, although, excepting the shortening of muscle, no other feature common to the two processes was known. Rigor had been designated as the last contraction of dying muscle. The new conception reversed the comparison, inasmuch as it assimilated contraction to a momentaneous and transitory rigor; and, since then, the physical analogies between contraction and rigor have been discovered to be more and more numerous. It has been found that in rigor as well as in contraction the volume of the muscle diminishes somewhat, and that in both processes heat is developed; while, to complete the relationship, it has been lately observed that certain evident transitional stages occur between the two.[1] Thus stimuli of excessive intensity, when applied to muscle, lead to a condition in which the state of contraction never perfectly disappears, but leaves behind it a residue of shortening. Fatigue and the process of death favour this condition of persistent, rigor-like contraction (Schiff's "Idiomuscular Contraction"), and a similar condition is induced by many muscular poisons, as veratria, delphinia, digitalin, emetin, caffein, &c.

While pursuing the analogies between the conditions of contraction and rigor I came upon a view of the nature of animal electricity which differed essentially from that which was then in existence.[2]

Fundamental Conceptions of the New Theory

The most important and at that time the most certainly ascertained fact was this, that a transversely divided muscular fibre exhibits, until it has become completely rigid, a negative potential in the plane of the section, and that this difference of potential diminishes or disappears during the state of functional activity. Having regard to the chemical and physical analogy between the processes of activity and of rigor; and considering the fact that when an artificial cross-section is made there is from the very first a portion of matter within the fibre which is passing into rigor, I explained this phenomenon on the assumption that the process of rigor, like the process of contraction, so modifies the protoplasm that it takes on a lower potential than the unchanged inactive protoplasm.

If this be so then an artifical cross-section must preserve a lower potential so long as the fibre has not become rigid throughout its entire extent, and on stimulation of the portion yet alive a diminution of the electrical difference must result.

II.—The Electrical Currents of Organs at Rest

The Absence of Currents in Muscles which are at Rest and Uninjured

In order to sustain the new theory of animal electricity (which, in opposition to the "Pre-existence Theory" of du Bois-Reymond I have designated the "Alteration Theory"), it

[1] Untersuchungen über den Stoffwechsel der Muskeln ausgehend vom Gaswechsel derselben. Berlin, 1867.

[2] Similar views have been more recently expressed, and with a somewhat more general application, by J. Liebig, in the "Sitzungsber. der bayr. Acad.," 1869. ii. 4; and by Pflüger, "Arch. f. d. ges. Physiol.," x. p. 251, 1875. I myself had extended this view to the chemical processes in nerves and glands.

[1] "Arch. f. d. ges. Physiol.," xiii. p. 371, 1876; xvi. p. 252, 1878.

[2] "Untersuchungen zur Physiologie der Muskeln und Nerven," 2 u. 3 Heft. Berlin: 1867, 1868.

was in the first place necessary to determine whether absolutely uninjured muscles are the seat of a muscular current.

Muscles cut out of the body possess almost invariably imperceptible injuries of a mechanical or chemical nature. In the first researches of du Bois-Reymond the muscles were regularly moistened with saturated solutions of common salt, and thereby, to a certain extent, corroded. Hence he was led to ascribe to the natural terminations of muscular fibres the negative potential which is exhibited by artificial transverse sections. This source of error was indeed discovered by du Bois-Reymond himself; but, after he had removed it, there yet remained other electrical phenomena apparently dependent upon the terminations of the fibres, differing, however, entirely from that just mentioned and obeying no strict rule in respect of direction of current. For these phenomena the hypothesis of the parelectronomic layer was introduced.

The more, however, all injuries are avoided in the preparation of muscles about to be used for such observations, the nearer is the approach to complete absence of currents. Chief among the injuries in the case of the frog is that produced by contact with the caustic secretion of the external skin.[1] The gastrocnemius is a muscle which may very readily be defended from such contact during its preparation, and it then exhibits merely weak currents of indefinite direction, such as are found in all circuits containing moist conductors.[2] The muscles of the thigh are, without exception, so connected with one another or with neighbouring parts (skin, bones, &c.) that they cannot be prepared without sustaining *mechanical* injuries in the course of the necessary manipulation.[3]

The statement that cold favours the development of a currentless condition, or of opposing currents, in muscle is not confirmed. The gastrocnemii of freshly-caught frogs do not differ electrically from those of frogs kept for a long time previously in an ice-cellar.[4] It is perhaps as well to observe that muscles which have been actually frozen suffer injury in the subsequent process of thawing, and are therefore to be avoided in these researches.

The most promising method of testing perfectly uninjured muscles appeared to be to examine them in the unskinned animal. Du Bois-Reymond,[5] who was the first to hit upon this method of observation, encountered an unexpected difficulty in the cutaneous currents which almost all animals possess. If it is first attempted to abolish the cutaneous currents by cauterising the skin with salt solutions, the solution rapidly penetrates the skin and attacks the underlying muscles; this is observed to occur by the gradual development of the same current as is induced when the naked muscle is moistened by caustic agents. That the muscles have already been attacked by the time the muscular current is developed[6] may be directly shown by employing silver-nitrate as the cauterising solution, when its action upon the tissues subjacent to the skin is manifested by rendering them opalescent.[7] If the skin is cauterised in places beneath which there are no aponeurotic muscular surfaces, no muscular current can be discovered, or at the most only such weak and irregular currents as count for nothing in the case of a circuit formed of moist conductors; not even the action of caustics, nor abrasion renders the skin absolutely currentless.[8]

Fishes possess no cutaneous current; in their case it suffices to connect any two points on the surface of the skin with the galvanometer in order to ascertain the absence of a muscular current.[9] These animals, like the frogs used in these experiments, must be rendered motionless by the action of curare.

According to the recent researches of Engelmann the heart offers an example in which it is easy to demonstrate the absence of a muscular current.[10] To do so it is merely necessary to remove the pericardium, an operation which can be performed without inflicting any injury upon the muscular substance. The heart is currentless, but every injured spot in it possesses a negative electric potential in reference to the rest. The pre-existence theory can only explain this fact, and the previously mentioned fact of the want of a current in the fish, by the most improbable surmise made expressly to suit the cases, that no muscular fibre has its termination directed towards the surface.

1 "Arch. f. d. ges. Physiol.," iii. p. 37, 1870.
2 *Ibid.*, p. 16, 35. 3 *Ibid.*, xv. p. 227, 1877. 4 *Ibid.*, xv. p. 226, 1877.
5 Du Bois-Reymond, "Untersuchungen über thier. Electr.," ii. 2 Abth. p. 7.
6 "Untersuchungen," Heft iii. p. 6, 1868. 7 *Ibid.*, p. 14.
8 *Ibid.*, p. 14; "Arch. f. d. ges. Physiol.," iii. pp. 16, 23, 26, *et seq.*; iv. p. 149, 1871.
9 "Arch. f. d. ges. Physiol.," iv. p. 152, 1871.
10 Engelmann, "Utrecht'sche physiol. Onderzoek." (3), iii. p. 82, 1874.

No Electromotive Force pre-existent in Muscle

Another method of deciding whether a current pre-exists in muscle appeared to be to determine whether, immediately after a transverse section is made, the current is present in full force, or whether an interval, however short, is required for the development of it. Were the latter the case it would be impossible for the doctrine to be correct which assumes that electromotive molecules exist ready formed in muscle and are merely laid bare by the knife. The experiments,[1] which were made by me in the years 1875 to 1877, to decide this question, have settled it in opposition to the pre-existence theory. With the help of a special apparatus the galvanometer circuit was closed at the moment when an injury was inflicted upon a previously uninjured muscle and then opened again after a very minute interval of time. The deviation observed was smaller than in a second trial made after the muscular current had already been developed, and in which the current was allowed to act on the galvanometer for the same period of time. The muscular current, therefore, requires time for its development: it does not pre-exist.

The Currents of Nerves, of Glands containing Blood, of Plants, &c.

It was to be surmised that other protoplasmic tissues than muscle would exhibit the phenomenon that portions of the tissue which are in process of dying have a lower potential than the yet living parts. First in importance in this respect is nerve: the current of nerve was discovered by du Bois-Reymond, and, like that of muscle, explained by a molecular hypothesis. Uninjured terminations of nerves are, for many reasons, not available for electrical researches; nevertheless, on the ground of the analogy to muscles, no one doubted the existence of a current in nerves, nor the applicability to them of the molecular scheme. With the discovery of the absence of a current in uninjured muscles, however, the analogy cuts the other way.[2] Henceforward there was not the slightest ground to assume the existence of a current in nerves at rest, except that developed by artificial sections. We shall see that even the phenomena of electrotonus do not in the least justify the molecular hypothesis.

Besides nerves, two other groups of protoplasmic apparatus have been examined by myself; and it has been found that in them, also, artificial sections possess a negative potential when compared with the rest of the organs. In the cases of both groups it afterwards appeared that the observations had previously been made by others. The first case was that of the glandular organs of the frog,[3] in which Matteucci had already found artificial cross-sections to be negative. I made out that this character is only present when the vessels contain uncoagulated blood, to the changes in which at the exposed surface it seems the electromotive force is due. The second case was that of the negative potential of artificial cuts and of cauterised spots in plants,[4] facts first observed by H. Buff.[5] Both phenomena are of such a kind that it is absolutely inconceivable to apply a molecular theory to them; yet notwithstanding the attempt has been seriously made.

The Dependence of Currents upon the Contact of Normal and Injured Protoplasmic Tissue

When examining the artificial cross-sections of plants I observed that the lower potential of the exposed surface quickly disappeared, but that a new section soon restored the original condition. I explained this behaviour by supposing that, according to the fundamental law, the lower potential of the cross-section only persists so long as the cells which are implicated in the section possess a remnant of living protoplasm. If the cells are entirely dead, the current must cease. In this manner we can explain why, in plants with obviously long fibres, artificial longitudinal sections have a higher potential than artificial transverse sections;[6] for the cells which are split up lengthwise die much more rapidly than those which are cut across. The same transitory character of the current which I have discovered in artificial cross-sections through vegetable tissues has lately been observed by Engelmann[7] in the case of the heart and of organs possessed of unstriped muscular fibres, and has been similarly explained. These organs are composed

1 "Arch. f. d. ges. Physiol.," xv., p. 191, 1877.
2 "Untersuchungen," Heft iii., p. 25, 1868. 3 *Ibid.*, p. 88.
4 "Arch. f. d. ges. Physiol.," iv. p. 155, 1871.
5 Buff, "Ann. d. Chemie," lxxxix. p. 76, 1854.
6 "Arch. f. d. ges. Physiol.," iv. pp. 159, 163, 1871.
7 Engelmann, "Arch. f. d. ges. Physiol.," xv. p. 116, 1877.

of independent cellular elements placed one behind the other in rows, and the current of one section can therefore only last until the injured cells have died from end to end.[1] In the last place the same phenomenon was discovered, in the case of nerves, by Engelmann. Here the nodes of Ranvier constitute cellular boundaries which effectually confine the process of death[2] to the internode immediately injured during section, although they offer no greater barrier to the transmission of the excitatory condition than do the cell-boundaries in the heart, intestines, or uterus.[3]

Disappearance of Demarcation Currents (Demarcations-ströme) under the Influence of the Natural Reparative Processes of the Body

It was reserved for Engelmann to discover yet another fact opposed to the doctrine of the pre-existence of electrical forces in muscle.[4] If in a living frog a muscle be divided subcutaneously the negative potential of the artificial section diminishes continually, and ultimately disappears, under the influence of the blood circulating in the part and of the nerves supplying it. As, therefore, nature tends to render artificial sections currentless, it is clear that in the natural condition no muscle can be the seat of currents, but every muscular current observed during rest must spring from injuries.

Inasmuch as all currents exhibited by muscles and nerves in a condition of inactivity (except those caused by inequalities of temperature or by the passage of extraneous currents) depend upon the contact of dying and living matter, and since the electromotive force has its origin in the surfaces which form the boundary between the two, I have called these currents of rest "demarcation currents" (Demarcations-ströme).

Influence of Temperature

If the muscular substance within the same fibre exhibits differences of temperature the warmer spots possess[5] a higher potential than the colder, so long as the temperature does not attain the limits of heat-rigor, and the consequent negative potential ensue. In exactly the same degree as living substance acquires through heat a higher potential than other living substance, is its potential exalted as regards dying substance. Hence, it is not merely that the demarcation-current becomes more powerful by heating the whole muscle, as was in part revealed to du Bois-Reymond, and recently established by Steiner in regard to nerves, but the force of the demarcation-current is dependent only on the temperature of the living substance at the point of application of one galvanometer-electrode, and not on the temperature of the substance lying between the poles. In a word, portions of muscular substance in different conditions form together a voltaic series.[6]

The Currents of Entire Muscles

As has been already remarked, wholly uninjured muscular fibres possess no current. All currents of muscles at rest are, therefore, variations in temperature excepted, the results of injuries. The properties of the current are most simple in the case of a muscle with parallel fibres, and cut transversely at right

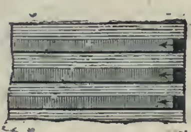


FIG. 2.

angles. In this case all the boundary surfaces between dying and living tissue are parallel to the section, and every point in the transverse surface has a lower potential than any point in the

[1] I made an exactly similar observation in the spring of 1877 on the tissues of young Medusæ which were sent to me through the kindness of Prof. Hensen, of Kiel.

[2] Engelmann, "Arch. f. d. ges. Physiol, xiii., p. 474, 1876.

[3] Gad und Tschiriew (" Verhandl. d. physiol. Ges. zu Berlin," 1877, Nr. 21) deduce the disappearance of the current in nerves from the fact that, after the death of the divided internodes the seat of electromotive force, viz., the ends of the internodes of the next row are no longer in one transverse plane, whence the resulting current is much diminished by partial equalisations. The value of this observation is obvious when it is remembered that the length of the internodes of Ranvier is, generally speaking, only 1—1½ mm.

[4] Engelmann, "Arch. f. d. ges. Physiol.," xv., p. 328, 1877.

[5] "Arch. f. d. ges. Physiol.," iv. p. 163, 1871. [6] *Ibid.*, p. 178.

longitudinal surface.[1] As, however, the muscle possesses between its fibres and on its surface indifferent conducting tissues through which the demarcation-currents can in great part become equalised—such tissues as sarcolemma, perimysium, the dead tissue of the cross-section—two important results follow. In the first place the force of the currents obtained by applying the galvanometer poles to two spots of different potential represents but a fraction of the electromotive force of a single fibre. In the second place the positive potential of the longitudinal surface and the negative potential of the transverse surface are so distributed as to be most marked at the centre of the respective surfaces. Hence arise the so-called "weak currents" when the galvanometer poles are applied to unsymmetrical points on the transverse or longitudinal surface.[2] But the weak currents of longitudinal surfaces may, especially in nerves, be in part due to an electrotonic extension of the demarcation-current which will be the subject of discussion later on.

In oblique sections there occurs a peculiar arrangement of the level-lines[3] (Niveau-linien), inasmuch as there intervenes an electromotive force which is directed from the acute to the obtuse edge of the slanting section: such currents are called "currents of inclination." Du Bois-Reymond explains this force by the step-like arrangement which the terminal molecules form in the slanting section; but it is clear that the same mode of explanation is just as well adapted to the step-like arrangement of the boundary-surfaces between dying and living tissue in the successive sets of fibres (Fig. 3).

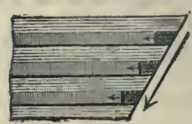


FIG. 3.

The molecular theory, therefore, is not needed to explain the currents of inclination of a *bundle* of fibres; it would only be indispensable in the case of a current of inclination of a *single* obliquely-cut fibre, did such a fibre possess one; but this is precisely what no one has demonstrated or can demonstrate. The circumstance that many muscular fibres present slanting facettes at their terminations in tendons[4] offers no opportunity to support the molecular theory; for such tendons possess no current of inclination in the uninjured condition, while, in the injured condition, the tissue of each fibre invariably dies down to a surface at right angles to the long axis of the fibre, and thus the conditions for the current of inclination arise (Fig. 3).

In all cases of partial injuries to muscle it is only the injured fibres which are the seat of electromotive action; the remainder merely form indifferent conductors which give facilities for local equilibrations. The weak currents of such muscles have, therefore, no regular relation to the surface of the muscle, as all depends upon the situation of the injuries: hence the irregular currents of so-called parelectronomic muscles.

When injuries, of whatever kind, affect the whole surface of a muscle, the probability is that a current will be detected in the muscle flowing from one end of it towards the longitudinal surface; and, in fact, when tested, this is found to be almost always the case. If a slight injury has been inflicted upon the lateral surface of a muscle the death-changes in fibres which have been opened longitudinally will speedily be checked at the boundary of the next fibre, while the death-changes which have been started at the ends or cross-sections will creep along the whole length of the fibres and occasion a lasting current. This has been previously alluded to, when the transitional nature of the demarcation-current in the heart, intestines, &c., was explained.

Local injuries to the external surface of a muscle give rise to local currents; but these currents, when a conductor is stretched from the longitudinal to the transverse surface, produce hardly any effect; whilst the demarcation-currents at the dying end of the muscle are under very favourable conditions for conduction, and

[1] The best method of applying the electrode to an artificial transverse section of muscle; so as to avoid any abduction of current from the living portion, is to produce a heat-rigor of the end of the muscle, and to apply the electrode to the rigid part (" thermal cross-section "); cf. "Arch. f. d. ges. Physiol.," iv. p. 167, 1871.

[2] Du Bois-Reymond, "Untersuchungen," i. 1848.

[3] Du Bois-Reymond, "Arch. f. Anat. u. Physiol.," 1863, p. 521; "Monatsber. d. Berliner Acad.," 1866, p. 387.

[4] Du Bois-Reymond, "Monatsber. d. Berliner Acad.," 1872, p. 791.

the forces of the local currents are, on account of the usual oblique application of the muscular fibres to their tendons, generally summed into currents of inclination.

III.—Electrotonus

Considerations opposed to the Molecular Theory of Electronus

The explanation adduced in the Introductory Remarks, in order to elucidate the electrotonic condition of nerves has many theoretical difficulties which cannot be here entered upon. The theory appears, however, to be amenable to an experimental proof. If, namely, the molecules of the portion of a nerve traversed by a current arrange themselves in the direction of the current, the electromotive force of the current should receive a very appreciable increase; or, in other words, the current should become very much greater when it passes through a piece of living nerve than when it traverses a dead piece of similar dimensions. On trying this experiment, however, I find no such difference; or the differences found are far from agreeing with the molecular theory.[1]

Electrotonic Phenomena observed in Conductors with Polarisable Cores

An experiment of Mattencci's[2] gave the clue to an explanation of electrotonic phenomena. He found that a metallic wire which is surrounded by a moist envelope exhibits currents possessing the characters of the electrotonic currents of nerves, whenever a galvanic current is passed, at any point, through the moist envelope. Matteucci discovered, in addition, that the currents cease when the wire is of amalgamated zinc and the envelope is moistened by a saturated solution of zinc sulphate. Hence it follows that the phenomenon depends upon a polarisation between the core and the fluid.

I examined the phenomenon more closely,[3] passing metallic wires through a glass tube filled with fluid and possessing lateral processes for the conduction of currents, and my experiments confirm the fact that electrotonic currents only occur when a polarisable core is present. Further, it was seen that the currents only extended so far as both core and envelope possessed unbroken continuity, whereas continuous contact of the two was not necessary. Lastly, I determined the laws regulating the time of development of these currents, their duration, their cessation on opening, their dependence upon the distance, and the length of the portion of core traversed by the polarising current, their combination and superposition, &c. All the phenomena may be grouped without difficulty under a simple theory.

As the current *a* E *k* (Fig. 4) applied to the envelope endeavours to reach the core K K, it splits up, if no polarisation be present,

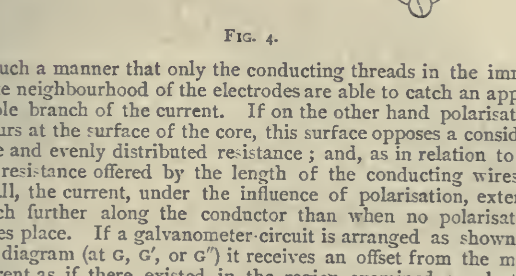


Fig. 4.

in such a manner that only the conducting threads in the immediate neighbourhood of the electrodes are able to catch an appreciable branch of the current. If on the other hand polarisation occurs at the surface of the core, this surface opposes a considerable and evenly distributed resistance; and, as in relation to it, the resistance offered by the length of the conducting wires is small, the current, under the influence of polarisation, extends much further along the conductor than when no polarisation takes place. If a galvanometer-circuit is arranged as shown in the diagram (at G, G′, or G″) it receives an offset from the main current as if there existed in the region examined an electromotive force of like direction with that of the polarising current.

This polarisational extension of the current only continues where both core and sheath are uninterrupted.

The abducted currents are at the same time, on mathematical grounds, a measure of the degree of polarisation at the points touched; and thus afford a means of tracing the extent of polarisation along the core. The curve representing the polarisational values at various points of the core—in general an exponential curve—has at the positive pole a positive maximum and a negative at the negative pole. In the region traversed by

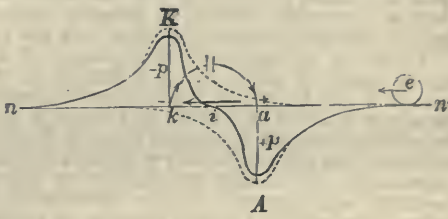


Fig. 5.

the current the curve cuts the axis at the so-called "indifferent point" (Fig. 5, *i*); and in the extra-polar regions it approaches the axes on both sides asymptotically. The curve is represented in Fig. 5, where, as a matter of subsequent convenience, the ordinates of positive polarisation are taken below the axis instead of above.

Internal Transverse Polarisation of Muscles and Nerves

In the year 1871 I discovered that the resistance offered by muscles and nerves to the passage of a current across their fibres was from five to nine times greater than the resistance offered by a current passing in direction of the fibres.[1] In the case of muscle this difference disappears almost entirely when rigor sets in; whilst in dead nerves it certainly persists, though reduced to one half. Further investigation of these facts disclosed their dependence upon a specific internal polarisability, which, in the case of muscle, is entirely associated with the living condition, and, in the case of nerve, in great part so associated. This fitness for polarisation by transversely-directed currents can only be due to a stratification of heterogeneous conductors across the tissue, which is wanting in its longitudinal axis. And since it is common to muscles and nerves, the stratification on which it depends must be sought in the typical structure which is common to the two organs, viz., the tubular nature of the fibres. We may assume, therefore, that polarisation takes place between the peculiar, active, substance of muscle- and nerve-fibres and the indifferent tissue ensheathing it.

Explanation of the Electrotonus of Nerves

As, accordingly, there exist in nerve-fibres all the essential conditions of the electrotonic extension of currents in conductors with polarisable nuclei, it may be assumed that the electrotonus of nerves is fully explained.[2] It is true that the core-substance of nerves is no better a conductor of electricity than the sheath-substance, while in the sheathed wire of Matteucci's experiment the core was of metal. But theory teaches that the electrotonic extension occurs even when the conducting powers of sheath and core are equal, if only polarisation takes place between the two.

This explanation of electrotonus, as I have shown in detail, completely covers all the phenomena. Especially does it account for the facts that the electrotonic state requires no appreciable time for its establishing;[3] and that it cannot extend past a ligatured spot. Whenever a nerve is crushed, the continuity of the core is broken, since, at the crushed spot, the core is killed and converted into indifferent tissue.

As muscle-fibres also possess polarisable cores, they must be endowed with electrotonic properties; nevertheless, neither du Bois-Reymond nor I at first succeeded in demonstrating them by galvanometric means; though, truly, just as little could their absence be distinctly affirmed. But theoretical considerations disclosed the reasons why muscle was less favourable to electrotonic phenomena than nerve;[4] and lately I have suc-

[1] "Untersuchungen," Heft iii. p. 67, 1868; "Arch. f. d. ges. Physiol.," vi. p. 328, 1872.
[2] Matteucci, *Comptes Rendus*, lvi. p. 760, 1863; lxv. pp. 151, 194, 884, 1867; lxvi. p. 580, 1868.
[3] "Arch. f. d. ges. Physiol.," v. p. 264, 1871; vi. p. 312, 1872; vii. p. 302, 1873.

[1] "Arch. f. d. ges. Physiol.," v., p. 223, 1871.
[2] *Ibid.*, vi. p. 328, 1872.
[3] "Helmholtz, "Monatsber. d. Berliner Acad.," p. 328, 1854; L. Hermann, "Arch. f. d. ges. Physiol.," viii. p. 272, 1874.
[4] "Arch. f. d. ges. Physiol.," vi. p. 350, 1872.

ceeded, by employing better contrivances, in demonstrating with the galvanometer the electrotonus of muscle.

An important addition to our knowledge of the electrotonus of nerves will be discussed at length in the sequel.

IV. The Electrical Currents of Organs in Activity.—A. Muscles

The Fall in Potential accompanying the Excitatory Wave

After Helmholtz had proved in the case of nerves, and Aeby in the case of muscles, that excitation is transmitted along the fibres with a measurable velocity, J. Bernstein undertook to investigate the electrical processes associated with the transmission.[1] To do so, he employed an instrument of his own devising, by means of which he could discover the electrical conditions of a muscle at any period after stimulation. He found in muscle on direct stimulation that a zone or section of negative potential arose which travelled down the fibre from the stimulated point with the same velocity as the wave of contraction. This phenomenon was readily explained by the theory of du Bois-Reymond, according to which the molecules of excited tissue suffer a diminution of activity, whence all excited spots must be electrically negative in relation to the inactive parts of the organ. Accordingly the phenomenon was designated "undulatory propagation of the negative variation."

On the assumption already made that excited tissue has, like dying tissue, a lower potential than the unchanged tissue, the fact is also at once explained, and I have named the currents called into being by the contact of excited with resting tissue, "functional currents" (Actions ströme).[2]

The Phasic and the Tetanic Functional Current

When a single excitatory wave runs over a muscular fibre which is connected at two points with the poles of a galvanometer, that point is of lower potential than the other beneath which the excitatory wave is passing at the time; or, if a wave happens to be passing both points at once, that point has the lower potential where the wave has the greater intensity or the stronger phase. Hence arises what may be called a "phasic functional current," the initial phase of which is a current proceeding *from* the stimulated spot, and the final phase a current in the opposite direction. The two phases are equal when the excitatory wave suffers no diminution in its course.

If a muscular fibre is tetanised, whether excitation travel in an undulatory form down the fibre or seize it as a whole, every galvanometer-pole applied to the muscle will be of higher or lower potential, according to the excitatory value of the points of application.

The Tetanic Functional Currents of Injured and Uninjured Muscles.

The first observations of du Bois-Reymond related to the tetanic functional current of muscles with artificial cross-sections. In such muscles the tetanic functional current is opposed to the demarcation-current, and manifests its influence in a "negative variation or deflection" of the latter. Since I regard the functional current as due to living muscle approximating in a certain degree to the condition of dying muscle, I have denominated such functional currents as diminish a demarcation-current "levelling," or "equalising," currents (ausgleichende).

Subsequently du Bois-Reymond discovered that uninjured muscles also exhibit a tetanic functional current proceeding from the stimulated point towards the end of the muscle. In order to reconcile this with his theory it became necessary to assume that the parelectronomic layer at the extremity of the fibre took part either not at all, or but slightly, in excitation. Finally the latter alternative was adopted, and it proved to be nearer the truth; for it was discovered that the functional current of uninjured muscles is less powerful than that of muscles cut across.[3] In a word, in order to explain the functional current of uninjured muscle, it was found necessary to assume that the extremities of a muscular fibre are less concerned in excitation than the middle.

While du Bois-Reymond was thus confining the limited participation in excitation to the extremities of the fibre, or, in other words, was locating the origin of the functional current in the end of the fibre, it occurred to me that the excitatory wave in its course over a muscular fibre might diminish in intensity;[1] whence it would follow, according to what was said in the foregoing section, that in tetanus an electrode placed near the stimulated point would have a lower potential or be electrically negative, to an electrode more remote. The direction of the diminutional or decremental current so obtained would agree both with observed facts and du Bois-Reymond's theory; but its force, instead of residing in the end of the fibre, is equally distributed over the whole course of the excitatory wave.

[1] J. Bernstein, "Monatsber. d. Berliner Acad." 1867, p. 72; "Arch. f. d. ges. Physiol." i. p. 173, 1868; "Untersuchungen ü. den Erregungsvorgang, &c.," Heidelberg, 1871.

[2] "Untersuchugen," Heft iii. p. 61, 1868; "Arch. f. d. ges. Physiol." xvi. p. 193, 1877.

[3] Du Bois-Reymond, "Arch. f. Anat. u. Physiol.," 1873, p. 548.

The Diminution in Intensity of the Excitatory Wave in Excised Muscles

Shortly after I had expressed my belief that the excitatory wave would be found to diminish in intensity in its passage down the muscle, Bernstein actually observed that it was so.[2] But du Bois-Reymond surmised that the phenomenon might be the result of the abnormal conditions of the experiment, and ought, in fact, to be attributed to the moribund state of the excised muscle; and in support of his surmise he stated that perfectly fresh muscle, on direct stimulation, exhibits no decremental functional current between any two points of its substance.[3] He persisted, therefore, in assigning the origin of the functional current to the parelectronomic layer. Moreover, he expressed a doubt whether, on stimulating a muscle through its nerve, excitation is propagated along the muscle in the form of undulations.

Meanwhile, in my own experiments I invariably detected the decrement of excitatory wave in excised muscles, a decrement, it need hardly be observed, which increased with the degree of exhaustion of the muscle.[4] Further, I hit upon the following proof that the force of the functional currents is evenly distributed over the whole fibre. If, while a muscle is being tetanised at one end, the points of application of the galvanometer electrodes be shifted in relation to one another and to the seat of stimulation, a functional current will invariably be discovered proceeding away from the stimulus; and the force of the current will be found to depend solely upon the interval between the galvanometer poles, irrespective of their position in relation to the end of the muscle.[5] The same observations may be made on a muscle which is tetanised through its nerve.[6] A functional current may always be abducted from the muscle, the force of which is exclusively determined by the respective distances of the abducting electrodes from the "nervous equator"[7] of the muscle.

The above experiments prove that excitation, even on nervous stimulation, proceeds in the form of a wave; that in excised muscles it always suffers a decrement; that the tetanic functional current is decremental in its nature; and that its origin is not restricted to the ends of the muscular fibres, but is distributed over the whole course of the excitatory wave.

The Phasic Functional Current developed on Stimulation through Nerve

The first examination of the phasic functional current of a muscle which is excited through its nerve was made by S. Mayer, under the supervision of Prof. Bernstein;[8] in their researches the gastrocnemii of frogs were used. They made out that stimulation was followed by the development of two currents in succession, of which the first to appear was a descending current and the second an ascending current. If the gastrocnemius had been previously injured at its lower end, the second, or ascending, current was less pronounced.

These results were subsequently established by some experiments of du Bois-Reymond,[9] as well as by others which I made with a special non-repeating apparatus adapted to single stimuli.[10] Du Bois-Reymond explained the phasic current as due to the asynchronism of the two forces concurring to produce the

[1] "Untersuchungen," Heft iii., p. 60, 1868.

[2] Bernstein, "Untersuchungen, &c.," p. 64, 1871.

[3] Du Bois-Reymond, "Arch. f. Anat. u. Physiol.," p. 369, 1876.

[4] "Arch. f. d. ges. Physiol.," xvi. p. 194, 1877.

[5] *Ibid.*, p. 217. [6] *Ibid.*, p. 229.

[7] Each constituent fibre of a muscle receives the motorial end-organ of a nerve-fibre. The motorial end organs of all the fibres are not absolutely in the same plane; and by the term "nervous equator" I have designated their *mean level* in a given muscle. The "nervous equator" is that equatorial section of a muscle in relation to which the distances of the various motorial plates, when algebraically summed, amount to nothing. See "Arch. f. d. ges. Physiol.," xvi. pp. 234, 414, 1878.

[8] S. Mayer, "Arch. f. Anat u. Physiol.," 1868, p. 655.

[9] Du Bois-Reymond, "Arch. f. Anat. u. Physiol.," 1873, p. 584.

[10] "Arch. f. d. ges. Physiol.," xv. p. 235, 1877.

functional current. These forces were assumed to be located in the two extremities of the muscular fibres and to be directed each to the correspondent end of the fibre, on which account they would of necessity overlap. The descending functional current belonging to the lower end was supposed to arise more quickly and to disappear earlier than the ascending current of the upper end; and hence the appearance of phases. Inasmuch as injury to one of the ends of a muscle (or in other words, removal of the parelectronomic layer), according to Du Bois-Reymond, intensifies the functional current proper to that end, it is clear that, when the inferior extremity is injured, the lower (descending) functional current obscures the upper.[1]

My own explanation[2] is essentially different from du Bois-Reymond's. According to my views the muscle must exhibit a descending current when the excitatory wave is at the upper end of the muscle, and an ascending current when at the lower end. Hence it is not the first, but the second phase, which is proper to the lower end of the muscle. The former, as I have shown, pertains to the moment when the excitatory wave is in the neighbourhood of the upper electrode—a condition most advantageously secured, owing to the peculiar structure of the gastrocnemius, by placing the electrode so as to abduct a current from the mid point of the fibres rather than from their upper end.

According to this theory it is clear that the descending phase must precede the ascending: for every excitatory wave is started at the middle point of the muscular fibre where the nerve enters, and only reaches the ends of the fibres at a later period. If, however, the lower end of the fibre have suffered injury, the excitatory wave running towards the injured part will be powerless to cause a current, owing to the constant negative potential of the injured end, and hence the second, ascending phase comes to nothing.

The fitness of this explanation was placed beyond doubt by experiments on the phasic functional current of regularly-constructed muscles.[3] In such muscles there invariably appeared in each half first an atterminal and then an abterminal phase

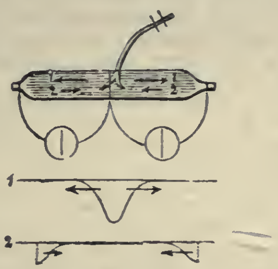


FIG. 6.

(Fig. 6). The atterminal phases in both halves (1 in Fig. 6) coincide in time, arising, as they both do, in the mid-point of the fibre at the starting of the excitatory wave; in other words, the middle point of the fibre becomes negative in comparison with either end.

In like manner the abterminal phases of both halves coincide (2 in Fig. 6), being produced by the arrival of the excitatory waves at the extremities of the fibre: in other words, either end of the fibre becomes negative in comparison with the mid-point.

In this manner the wave-like propagation of the excitatory condition in muscle was established for the case of muscle stimulated through nerves, and, at the same time, another proof was afforded that the force of the functional current does not exclusively reside in the ends of the fibres.

Furthermore, it was directly shown in these experiments that the excitatory-wave in excised muscles, while traversing the fibres, experiences a diminution; for the second, abterminal phase was invariably much weaker than the atterminal, as is indicated in Fig. 6 by the length of the arrows and the height of the representative curve, and it invariably further diminished in the course of an experiment.

The Functional Current in wholly Uninjured Muscle in Man

In the case of man the currents of normal resting muscle were secured from investigation by the obstacle of the skin. But Du Bois-Reymond found, during the violent voluntary contraction of the muscles of an arm or a leg, that the limb exhibited an ascending current. This he took to be the algebraical sum of the tetanic functional currents of all the muscles exerted, although such an explanation only became very probable by the exclusion of certain other experiments designed to elucidate the matter.[1] But the current could never be demonstrated under the most favourable conditions of abduction, viz., by the application of the galvanometer wires to individual groups of muscles.[2]

[1] Du Bois-Reymond, *loc. cit.*, 1873-76.
[2] "Arch. f. d. ges. Physiol.," xvi. p. 236.
[3] *Ibid.*, p. 239.

The question whether the diminution of the excitatory wave only occurs in excised muscles as a result of death changes, could, of course, only be settled by experiments on living human beings. And since the manifold disturbances inseparable from the method of experiment rendered the tetanic functional current almost useless for this investigation, I undertook to examine the *phasic* current in the muscles of the fore-arm.[3] I found that the same relationship held for the muscles of man as had before been shown to exist in the case of the frog. The first phase is an atterminal current in which the region of the nervous equator—about 10 cm. below the elbow—becomes negative in comparison with the two ends; the second phase is abterminal, *i.e.*, the ends of the muscles become negative in comparison with the equator (Fig. 7). But in the perfectly

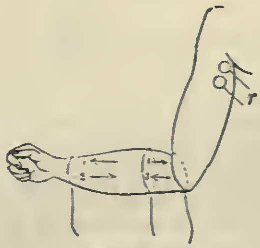


FIG. 7.

normal muscles of man the second phase was *not* weaker than the first—a relation which was constant in the excised muscles of the frog. And hence we may conclude that in absolutely normal muscles the excitatory wave does not diminish in intensity. These experiments, which constitute the first regular examination of muscular action in living man, further taught approximately the rate of propagation of the excitatory wave in human muscles, viz., from 10 to 13 metres per second.

The Absence of the Functional Current when none of the above Conditions of Electrical Inequality exist in Muscle

If an uninjured, currentless muscle be thrown into the active condition by a stimulus affecting its whole mass at once, it never exhibits any functional current,[4] whether the stimulus be single or tetanic. The explanation of this is obvious. In such a totally excited muscle the whole substance passes at the same moment into the same degree of excitation; and, hence, is nowhere the opportunity given for the contact of excited with non-excited or imperfectly excited tissue.

If, on the other hand, the muscle possesses an artificial transverse section with the associated boundary current, then indeed a levelling or equalising functional current arises on total excitation and diminishes the current of injury.[5]

When muscles are tetanised through their nerves, functional currents only appear where the excitatory wave has not the same intensity throughout the tetanised mass. Hence in man, whose muscles when under absolutely normal conditions exhibit no diminution or decrement of wave, it is impossible to demonstrate a tetanic functional current. I have, indeed, when using very powerful and fatiguing stimulation, now and then succeeded in getting indications of such a decremental functional current, but from some cause or other, we cannot depend upon this experiment to lead to a constant result, perhaps for the reason that the conductivity of human muscles is but slightly hampered by fatigue, or possibly because the collateral effects of stimulation mar the experiment.[6] I may be allowed to remark here that certain experiments now in course of publication show very clearly that stimulation of human muscles causes a secretory cutaneous current directed from without inwards. This current is the true

[1] Du Bois-Reymond, "Untersuchungen," ii., Abth. 2, p. 267.
[2] "Arch. f. d. ges. Physiol.," xvi., p. 257, 1877.
[3] *Ibid.*, xvi., p. 410, 1878.
[4] "Arch. f. d. ges. Physiol.," xvi., p. 203, 1877.
[5] *Ibid.*, xv., p. 238; xvi., p. 203, *et seq.*, 1877.
[6] *Ibid.*, xvi., p. 416, 1878.

cause of the negative potential exhibited by the voluntarily contracted muscles of one limb when compared with the opposite but unexerted limb, and is therefore the current which du Bois-Reymond took for the muscular functional current of man.[1]

B.—Nerves

The Functional Current of Nerves remote from all Transverse Sections

In the nerves of the frog, according to Helmholtz, excitatory waves are propagated at the rate of twenty-eight metres per second. Hence, if the two ends of a galvanometer circuit were laid upon two points of a nerve a functional current should be manifest on stimulation of the nerve, and should consist of two phases according as the excitatory wave was passing one or the other of the electrodes. Nevertheless the functional current of nerves, owing doubtless to its exceedingly fleeting character, has hitherto escaped detection.[2] Since the excitatory wave of nerves does not diminish in its course, it is to be expected that the two phases of the nervous functional current will be equal; and hence also it is that in tetanic stimulation, where we have to do with the algebraical sum of these two equal and opposite phases, we obtain no functional current whatever in uninjured nerves.

The Functional Current of Nerves at a Transverse Section

The functional current of nerves bounded by an artificial transverse section was discovered by du Bois-Reymond; it is an equalising current, and consists, therefore, in a diminution of the constant demarcation current. Du Bois-Reymond only found this current in nerves tetanically stimulated, but Bernstein, by means of his apparatus already referred to, succeeded in demonstrating its presence in the case of single excitatory waves.[3] If the poles of the galvanometer circuit are applied, one to the artificial cross-section, and the other to a point in the longitudinal surface, the diminution of the demarcation current occurs in the instant that the excitatory wave passes the latter, or longitudinal pole. By altering the position of this pole, the progress and the course in time of the excitatory wave may be examined. The rate of progression so deduced was found to agree with that determined in other experiments by varying the distance between the point of the nerve at which a stimulus was applied and the fixed point at which the result of stimulation was manifested, viz., the dependent muscle, or the pole applied to an artificial cross-section. And this similarity of result established the identity of the process occurring during excitation, and the wave of negative deflection.[4] The way in which the wave comports itself on approaching the artificial cross-section will be explained below.

The Functional Current of Polarised Nerves: the Polarisational Increment of Excitation

In 1866 Bernstein found [5] that the electrotonic currents of nerve, on stimulation of the nerve, suffer diminution like the demarcation-currents. As a disciple of the molecular theory he explained this phenomenon in the following way: Since the electrotonic currents depend upon an altered arrangement of the molecules, and since the force of each molecule diminishes on stimulation of the nerve, therefore the electrotonic currents must also be lessened during excitation. And so the new phenomenon seemed to be completely covered by the molecular theory. In my view, however, the electrotonic currents are merely offsets diverted from the main polarising current owing to the internal polarisation of the nerve itself. Since these offsets could not be supposed to be modified during excitation, I concluded that every apparent diminution depended upon a functional current which arises owing to the polarised condition of the nerve, and which is opposite to the polarising current in direction. I assumed, as the cause of this functional current, that the excitatory wave failed of maintaining its magnitude while passing through the polarised portions of nerve; it increased as it reached more positively or less negatively polarised areas, and diminished under the opposite conditions. This is called the doctrine of the "polarisational increment of excitation,"[1] and it is clearly competent to explain Bernstein's observations.

If this assumption be reasonable, the excitatory condition travelling along the nerve should be most intense at the anode of the polarising current, and least intense at the cathode; and hence there should be present in the intrapolar region a powerful functional current of like-direction with the polarising current and reinforcing it. Such an intrapolar current I did, in fact, discover to be constantly present;[2] though afterwards it appeared that a similar observation had previously been made by Grünhagen, by whom, however, the current was otherwise explained as the effect of a diminished resistance in the nerve during excitation, leading to an increase in the polarising current.[3] Before I had any knowledge of this early observation of Grünhagen, the probability of the explanation which he assigned to it had been tested by me, and numerous indications had been found that the intrapolar increase of current was indeed an electromotive phenomenon and not due to a diminished resistance.[4] But later I was enabled to settle the question in the most direct manner, by the discovery that the transverse resistance of nerves is *not* diminished during excitation—excitation having in general no manner of influence upon the resistance offered by the nerve.[5]

Further Physiological Support of the Doctrine of Polarisational Increment

In order to grasp the doctrine of polarisational increment, let us regard the axis *n n'* in Fig. 5 as representing a nerve, the conditions of polarisation of which are indicated by the vertical ordinates, positive polarisation being exceptionally represented by descending lines, and negative by ascending lines. With these ordinates we can trace out the polarisation curve, *n* K *i* A *n'* (already spoken of in describing Fig. 5, which see), the lowest point of which corresponds to the anode, and the highest to the cathode. Let us now suppose a ball, *e*, devoid of friction, and travelling through space with a certain horizontal initial velocity, to be set rolling along this curve. The *vis viva* of the ball will then represent the magnitude of excitation. It is at once evident that the initial velocity is increased in the part of the curve below the line *n n'*, *i.e.*, in the anelectrotonic region, but is diminished in the upper portions of the curve, *i.e.*, in the catelectrotonic region. If the initial velocity is too small, the ball will not be able to reach the summit of the catelectrotonic portion of the curve, or, in other words, the excitation becomes dissipated in the corresponding region of nerve, and never succeeds in passing the cathode. Moreover, if the ball, with a certain initial velocity, were to be set going at some point of the inferior (anelectrotonic) portion of the curve, it would reach the outlying parts beyond the polarised region with a diminished velocity; while, if it were set going upon a part of the curve (catelectrotonic) superior to the line *n n'*, it would reach the outlying parts with an increased velocity.

All these deductions from the doctrine under discussion have been verified, in part by already established facts and in part by recent observations. The experiments by Eckhard and Pflüger have shown that a certain stimulus applied to a nerve produces a greater effect in the catelectrotonic region than in the anelectrotonic. And it is clear that these phenomena are as intelligible under my theory as under the assumption usually made to explain them, viz., that the irritability of the nerve itself is diminished during anelectrotonus and increased during catelectrotonus.[6]

Moreover, certain facts are known which seem to imply that, with a sufficient degree of polarisation, or with a sufficiently slight stimulus, the excitatory wave becomes blocked at the cathode.[7] If to this we add that excitation does not indefinitely increase with the stimulus, but soon reaches a maximum, we may further conclude that, under certain conditions, a diminution of excitation must take place even during the passage of the wave through the anodic area.[8]

In the last place it is to be noticed, that the artificial section of a nerve induces a negative polarisation or catelectrotonus of

[1] These experiments have been recently published; cf. Hermann and Zuchsinger, "Arch. f. d. ges. Physiol.," xvii. p. 310, 1878.

[2] I have succeeded in detecting these currents by extending the rate of propagation in the nerve by cold; and by using a bundle of four or six nerves together. Cf. "Arch. f. d. ges. Physiol.," xviii., p. 574, 1878.

[3] J. Bernstein, *loc. cit.*

[4] The new experiments referred to in the last note confirm this indirect conclusion in a more direct manner.

[5] J. Bernstein, "Arch. f. Anat. u. Physiol." p. 596, 1866.

[1] "Arch. f. d. ges. Physiol.," vi., p. 359, 1872; vii., p. 323, 1873.

[2] *Ibid.*, vi., p. 560, 1872; vii., p. 355, 1873; x., p. 215, 1875.

[3] Grünhagen, "Zeitsch. f. rat. Med." (3), xxxvi., p 132, 1869.

[4] "Arch. f. d. ges. Physiol.," x., p. 215, 1875.

[5] *Ibid.*, xii., p. 151, 1875.

[6] *Ibid.*, vii., pp. 325, 497, 1873.

[7] *Ibid.*, vii., p. 354, 1873; x., p. 226, 1875.

[8] *Ibid.*, vii., p. 361, 1873.

the fibres in the neighbourhood of the section, owing to the extension of the demarcation-current along the nerve.[1]

By this electrotonic extension we can explain—or for the most part explain—the so-called "weak currents" of the longitudinal section.[2] A stimulus applied to the nerve near the line of sec-

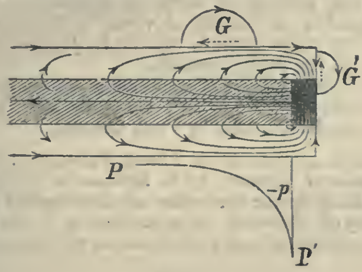


FIG. 8.

tion, according to the law of polarisational increment, should have a greater effect than when applied to points more remote, and this experiment shows to be the case. Finally, an excitatory wave travelling along the nerve towards the cut end of it must, according to the same law, gradually diminish before it disappears entirely in the area of section itself.

V. CONCLUDING REMARKS

The whole of the electrical phenomena of muscle and nerve, therefore, may be readily deduced from a few very simple propositions. Irritable protoplasm responds both to destructive and to exciting influences by an electromotive sign. The altered substance takes on a negative potential with respect to the unaltered. This, together with the doctrines of internal transverse polarisation and of the polarisational increment of excitation, appears fully competent to explain all the facts hitherto observed.

That these fundamental doctrines have the closest reference to the whole life of irritable tissues no one will be disposed to doubt. Yet much examination will be needed to disclose the exact nature of the interdependence.

Although it must now be confessed that the theories which were based upon facts discovered more than thirty years ago, have failed to withstand the criticism of a wider experience, the domain of animal electricity has not lost, but rather gained in interest. And the services of the man who not only discovered this region of physiology, but created the means of conquering it, and who made himself master of its most important fundamental features, are in no danger of becoming dimmed in our estimation by the theoretical changes we have been compelled to accept.

BAROMETRIC PRESSURE

IN a work of great importance,[3] recently published by Prof. Bert, on the physiological effects of barometrical pressure, the author sums up the conclusions to be drawn from his researches as follows:—

A. The diminution of barometric pressure acts on living beings only by diminishing the tension of the oxygen in the air which they breathe, in the blood which animates their tissues (*anoxytienne* of M. Jourdanet), and by thus exposing them to the dangers of asphyxia.

B. The increase of barometric pressure acts only by increasing the tension of the oxygen in the air and the blood. Up to about three atmospheres this increase of tension gives rise to intra-organic oxidations a little more active. Beyond five atmospheres the oxidations diminish in intensity, probably change in their nature, and, when the pressure rises sufficiently, are completely arrested. It follows that all living beings, aërial or aquatic, animal or vegetable, complex or mono-cellular—that all the anatomical elements, isolated (blood-globules, &c.) or grouped in tissues, perish more or less rapidly in air sufficiently compressed. This rule appears only to suffer exception for the reproductive corpuscles of some microscopic beings. For the higher animals death is preceded by tonic and clonic convulsions of extreme violence. Among vertebrates the rapid accidents due to the too great tension of oxygen only commence to manifest themselves at the moment when the hæmoglobin, being saturated with oxygen, that gas enters into the state of simple dissolution in contact with the tissues.

C. Diastases, poisons, and true virus resist the action of oxygen at high tension.

D. The inconvenient effects of diminution of pressure may be efficaciously combatted by the respiration of an air sufficiently rich in oxygen to maintain the tension of that gas at its normal value (20·9). Those of the increase of pressure may be combatted by employing air sufficiently poor in oxygen to arrive at the same result.

E. Generally the favourable or noxious gases (oxygen, carbonic acid, &c.) act only on living beings in accordance with the tension which they possess in the surrounding atmosphere, a tension which is measured by multiplying their centesimal proportion by the barometric pressure; the increase of one of the factors may be compensated by the diminution of the other.

F. When animals possess reservoirs of air either completely closed (swimming bladder of acanthopterygians, &c.) or in communication with the air during decompression alone (swimming vessel of the Cyprini, intestines of aërial vertebrates, &c.), or in communication with the air during both compression and decompression, but by very small orifices (lungs of aërial vertebrates, &c.), the diminution or increase of pressure may have physico-mechanical effects.

G. Sudden decompression from several atmospheres has only the effect (except for some cases comprised under conclusion F) of allowing to return to the free state the nitrogen which was, under favour of pressure, dissolved in the blood and the tissues.

H. The beings actually existing in a wild state on the surface of the globe are accommodated to the degree of oxygenated tension under which they live; all diminution, all increase, appears to be unfavourable to them when they are in a state of health. Therapeutics might make something out of these modifications in various patholgical conditions.

I. Barometric pressure and the proportion per cent. of oxygen have not always been the same on our globe. The tension of that gas has apparently been, and will without doubt continue to go on, diminishing. There is here a factor which we have not yet taken into account in biogenetic speculations. The power of reaction against these various modifications leads to the supposition that microscopic beings must have appeared first, and that they will be extinguished by the insufficient tension of oxygen.

K. It is inaccurate to teach, as is ordinarily done, that vegetables must have appeared in the earth before animals, in order to purify the air of the great quantity of CO_2 which it contained. In fact, germination, even that of mildew, does not take place in air sufficiently charged with CO_2 to be fatal to warm-blooded animals. It is quite as inaccurate, as I have observed long ago, to explain the anteriority of reptiles with warm-blooded animals by the impurity of the air tainted with too much CO_2; reptiles, in fact, are more injured by this gas than birds, and still more so than mammals.

SCIENTIFIC SERIALS

THE *Sitzungsberichte* of the Vienna Imperial Academy of Sciences (Natural History Section, vol. lxxvi. parts 1–5, and vol. lxxvii. parts 1–4) contain the following more important papers:—Addenda to our knowledge of annelids, by Dr. Aug. v. Mojsisovics.—On the orthoptera of the Senegal River, by Dr. H. Kraus.—On the fauna of the Cypris slates of the Eger tertiary strata, by O. Novák.—On the natural history of glimmer, by G. Tschermak.—Researches on cystolithes and some similar formations in the vegetable kingdom, by K. Richter.—On the genesis of salt deposits, particularly of those in western North America, by F. Pošepny.—On the fresh-water fish of South-Eastern Brazil, by Dr. F. Steindachner.—On the "Salse di Sassuolo"

[1] "Arch. f. d. ges. Physiol.," vii., p. 363, 1873. This polarisation, of course, still occurs even when the demarcation-current is not abducted, or when the abducted portion is counterbalanced by an opposite current. In the latter case, according to Bosscha's law, the nerve behaves just as if no abducting circuit were applied to it. "Arch. f. d. ges. Physiol.," ix., p. 29, 1874; x., p. 237, 1875.

[2] In Fig. 8 the core of the nerve-fibre is obliquely shaded. Even in the absence of polarisation of the core the boundary current would become distributed after the manner shown in the figure, and would pass into the galvanometer circuits G and G' as the so-called weak longitudinal and transverse currents of du Bois-Reymond. But, with polarisation, the extension along the core is very much greater than without it, and at the same time the polarisational curve pp' is produced.

[3] "La Pression Barométrique; Recherches de Physiologie Experimentale." Par Paul Bert, Professor à la Faculté des Sciences de Paris. (Paris: G. Masson. 1878.)

and the "Argille scagliose," by Theodor Fuchs.—On the flora of the countries round the Mediterranean and their dependence from the soils, by the same.—On the fossil flora of Parschlug, in Styria, by Dr. C. von Ettingshausen.—On the development of the embryo of *Asplenium shepherdi*, Spr., by F. Vouk.—On the internal cells in the antheridium cell of the pollen grain of certain coniferæ, by A. Tomaschek.—On the origin of aptychus-limestone, by Th. Fuchs.—On the light line in the prism cells of seed scales, by Dr. R. Junowics.—On the encircled specks in the wood of trees, by Dr. J. Kreuz.—On the firmness and elasticity of vegetable tissues and organs, by Th. Weinzierl.—On the resinous ducts of certain coniferæ, by Dr. Kreuz.—On the development of the pollen of *Colchicum autumnale*, L., by A. Tomaschek.—On some accessory appendages to the skull of Leporidæ, by Dr. A. von Mojsisovics.—On cork and cork tissues, by Dr. F. von Höhnel.—Histo-chemical researches on xyllophiline and coniferine, by the same.—On the phanerogamic flora of the Sandwich Islands, by Dr. H. W. Reichardt.—On the protoplasm of the pea, by Dr. Ed. Tangl.—On the undulating nutation of the internodes of the stems of plants, by J. Wiesner.—On the behaviour of Phloroglucine and of some similar substances towards the woody cell membrane, by the same.—On the degeneration in the leaf-shoots of some Amygdaleæ, caused by species of Exoascus, by E. Rathay.—Researches on Tunicata, by C. Heller.—On some new genera and species of Neuroptera, by Dr. F. Brauer.—On the originals to Ign. von Born's Testaceis Musei Cæsarei Vindobonensis, found in the Imperial Zoological Museum, by the same.—On the embryology of ferns, by H. Leitgeb.—Geological researches in the western part of the Balkan and the surrounding districts, by Franz Toula.—On some peculiar apertures in the corolla leaves of *Franciscea macrantha*, Pohl, by M. Waldner.—On the basaltic lava of the Eifel Mountains, by E. Hussak.—On the origin of holes and indentures in the leaf of *Philodendron pertusum*, Schott, by F. Schwarz.—Ichthyological researches, by Dr. F. Steindachner.—On the subterranean water-courses and basins, as well as on the clearness and transparency of certain lakes, and on the formation of lakes generally, by Dr. A. Boué.

Sitzungsberichte der physikalisch-medicinischen Societät zu Erlangen (part 10, November, 1877—August, 1878) contain the following more interesting papers :—On the fertilisation and division of the ovum of *toxopneustes*, by Dr. E. Selenka.—On the history of development of Jacobson's organ, by Dr. R. Fleischer.—On the theory of absorption and fluorescence, by E. Lommel.—On the physiological action of nitro-benzole and of aniline, by W. Filehne.—On the so-called soor-fungus and its identity with *Mycoderma vini*, by M. Reess.—On the changeability of the angles of crystals, by Dr. Fr. Pfaff.—On the theory of normal and abnormal dispersion, by E. Lommel.—Various mathematical papers, by M. Noether and Prof. E. Lüroth.—On the modification of sound phenomena in the human body, by F. Penzoldt.—On the theory of double refraction, by E. Lommel.—On the equations of the seventh degree, by F. Klein.—On some experiments made with *drosera*, by Drs. C. Kellerman and E. von Ramner.—On chelidonic and malic acids, by Dr. O. Liezenmayer.—Thermophysiological investigations, by J. Rosenthal —On the derivates of cymol and of toluylic acid, by E. von Gerichten.—On the sexual organs of dibranchiate cephalopoda, by Dr. J. Brock.—On two new fluorescent substances, by E. Lommel.—On the influence of the changes in temperature and pressure upon the double refraction of light, by Dr. E. Pfaff.

Jahrbuch der k.k. geologischen Reichsanstalt (vol. xxviii. part 4, October—December, 1878) contains several highly interesting treatises, viz. :—On Alpine phosphates, by J. Gamper.—On the production of common salt from the Russian steppe lakes, by Dr. C. O. Cech.—Observations on the Jurassic formation in the Carpathian cliffs, by Victor Uhlig.—On the artesian well in the Stadtwäldchen near Budapest, by Wilhelm Zsigmondy.—On Emanuel Kaiser's views on the hercynian fauna, and the limit between the Silurian and Devonian formations, by Dr. E. Tietze.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xii. fasc. iii. We note the following papers in this number :—New phenomena observed in treatment of wine and must, with lime (continued), by Prof. Pollacci.—New physio-pathological researches on pulmonary phthisis (continued), by Prof. Giovanni.—Project of an electrical indicator of the level of water in a flood, by Prof. Ferrini.—Amplitude of oscillations of the declination-needle during 1877 and 1878, at the observatory of Brera, in Milan, communicated by S. Schiaparelli.—Determition of the difference of longitude between Milan Observatory and those of Padua, Monaco, and Vienna, by Prof. Celoria.

Fasc. iv.—First lines of introduction to the study of Italian bacteria (continued), by S. Trevisan.—Composition of butters in Lombardy, and analysis of butter in general, by Dr. Menozzi.—New researches on the rot of vines, by Prof. Garovaglio.—Researches on polar systems, by Prof. Jung.—On provision against trichina, by Prof. Bizzozero.

Fasc. v.—A new process of microscopic art, by Prof. Golgi.—Fruitful copulation of a dog with a cat, by Prof. Lemoigne.—On the intestinal anguillula, by Prof. Cantoni.

Atti della R. Accademia dei Lincei, February, 1879.—Necrological memoir of Gastaldi, by S. Sella.—On the expression of one of the limits in the correction of the elliptical co-ordinates in the theory of planetary perturbations, by S. De Gasparis.—On the composition of rocks of the mines of Montecortini, by S. Cossa.

Rivista Scientifico-Industriale, Nos. 4 and 5, 1879.—We note in these numbers a memoir by Prof. Perotti, on governing combination of the elements of gaseous mixtures.

No. 6.—On a baricentric property of the triangle, by Prof. E. Cavalli.—On a new experiment on electrolysis with weak electromotors, by Prof. A. Bartoli.—On the telephone and microphone as musical instruments, by G. Mocenigo.—Description of some new plants recently introduced into horticulture, by E. O. Fenzi. There plants are *Gentiana algida*, *Primula capitata*, and *P. stuartii*, *Nicotiana acutifolia* and *N. suaveolens*, *Eremurus robustus*.—On two new species of Myriapoda, *Polydesmus siculus* and *Atractosoma nigrum*, by Prof. F. Fanzago.—On a new reagent for cobalt, by Mr. Tattersall.—On poisonous colours, by the editor.

Archives des Sciences physiques et naturelles, March.—From this part we notice the following papers of interest :—On the influence of coloured light upon the development and growth of animals, by Emile Yung.—On the effects of induction coils upon the nervous system, by M. Schiff.—On an acceleration of the process of tanning by means of phosphoric acid, by E. Ador.—On methyl-aniline and toluidine and the colouring-matters derived from these compounds, by MM. Reverdin, Monnet, and Nölting.—On alizarine blue, by M. Graebe.—The other papers contained in the part have been noticed by us elsewhere.

SOCIETIES AND ACADEMIES

London

Mathematical Society, April 10.—C. W. Merrifield, F.R.S., president, in the chair.—Mr. Donald McAlister was elected a Member, and Messrs. A. J. C. Allen and E. Anthony were proposed for election.—The following communications were made :—Notes on quantics of alternate numbers, used as a means for determining the invariants and covariants of quantics in general, by the late Prof. Clifford, F.R.S. (communicated by Dr. Spottiswoode, P.R.S.).—Note on geometrical maxima and minima, Mr. J. Hammond.—On a class of fractions, Mr. R. Tucker.

Linnean Society, April 3.—William Carruthers, F.R.S., vice-president, in the chair.—Mr. W. T. Thiselton Dyer exhibited the inflorescence of *Gynerium saccharoides*, grown at Kew, but which had died during the winter severe weather. Excepting through Mr. Spruce's researches on the Amazons, little is known respecting this handsome plant, which differs from the Pampas grass in habit, and is tropical like maize, &c.—Dr. H. Trimen, in dealing with the subject of the myrrhs of commerce and pharmacy, showed the unique *Balsamodendron myrrha*, Nees. It was gathered by Hildebrandt in Somali Land, 1873, and possesses but few leaves and a single fruit; the traveller, however, saw the myrrh exuding from the tree itself. The original type specimens of *B. myrrha*, collected by Prof. Ehrenberg in Arabia, were also exhibited, and, according to Dr. Trimen, Hildebrandt's late statement of their identity with the foregoing seems well founded. Ehrenberg's other myrrh plant, the *B. Ehrenbergianum*, Berg., with his notes attached, and the *B. Playfairii*, Hook. fil., from Somali Land, with its gum called "Hotai," and other examples of varieties of myrrh and bdellium were placed before the Society and commented on by Dr. Trimen. He specially adverted to the liberality of the authorities

of the Royal Berlin Herbarium and of the Hanbury Collection for being enabled to study the rare valuable specimens laid before the Fellows of the Linnean Society.—The account of a remarkable peat flood in the Falkland Islands, by Mr. Arthur Bailey, was communicated by Mr. W. T. Thiselton Dyer. About midnight, November 29, 1878, it was discovered that a black moving mass of peat, several feet high, was making its way towards the settlement at the rate of between four and five miles an hour. The next morning (30th) it was found that the town of Stanley was cut in two, intercourse between its east and west ends alone being possible by boats. Fortunately no lives were lost, and by the energy and activity of the inhabitants in the formation of a trench, much injury and destruction were considerably arrested.—The Secretary read in abstract some notes on Moquilea, with a description of a new species by Mr. John Miers. The author specially compares and marks the differences between the genera Moquilea and Licania, they having often been confounded, and he afterwards points out the distinctive characters constituting his new species, *Moquilea organensis.*

Chemical Society, April 3.—Mr. Warren De La Rue, president, in the chair.—The following papers were read:—On terpin and terpinol, by Dr. Tilden. The author has continued his previous researches, and has succeeded in obtaining crystals of terpin hydrate from essence of lemon; the author considers terpinol to have the constitution of an alcohol. Oil of lemon cajeputol, oil of coriander, and citronella contain bodies closely resembling terpinol.—On a gold nugget from South America, by Mr. G. Attwood. These nuggets are found in alluvial soil in Venezuela. Numerous gold-bearing quartz-veins are found in the neighbouring hills. About one-half of the nuggets are covered with a dark brown substance resembling a silicate of iron. When this is dissolved much finely divided gold separates, and the nugget is partly covered with dull fine gold. The gold obtained from the quartz is less pure than that of the nuggets. The author concludes that gold nuggets gradually increase in size owing to the accumulation of fresh particles of finely-precipitated gold.—On lead tetrachloride, by Mr. W. W. Fisher. The author has not isolated this compound, but has obtained it in solution by dissolving lead dioxide in hydrochloric acid; the yellow solution thus formed precipitates brown hydrated peroxide of lead when treated with solutions of alkalis, &c. The author also suggests the use of chlorine or bromine in the presence of sodium acetate as a means of quantitatively determining lead by precipitation as a peroxide.—On the transformation of aurin into trimethyl pararosanilin, by Messrs. Dale and Schorlemmer. This is effected by the action of an aqueous solution of methylamine at 125° on aurin.—On the solution of aluminium hydrate by ammonia and a physical isomeride of alumina, by C. F. Cross. By boiling the ammoniacal solution of aluminia hydrate a precipitate is obtained, which on drying and ignition furnishes alumina which is extremely hygroscopic, absorbing 35 per cent. of water.—Researches on dyeing, Part ii. Note on the emission of colouring matter, by Dr. Mills and Mr. Campbell. The experiments were made with silk and a dilute solution of Nicholson's blue. The authors affirm that a real and uniform dyeing effect can always be obtained with silk and Nicholson's blue, the heat and souring used by dyers being unadvisable. The authors recommend the addition of common salt to the vat.

Geological Society, March 26.—Henry Clifton Sorby, F.R.S., president, in the chair.—William Adamson Barron, Gregory Dent, Julian John Leverson, and Rear-Admiral Francisco Sangro Tremlett, R.N., were elected Fellows of the Society.—The following communications were read:—Results of a systematic survey (in 1878) of the directions and limits of dispersion, mode of occurrence, and relation to drift-deposits of the erratic blocks or boulders of the west of England and east of Wales, including a revision of many years' previous observations, by D. Mackintosh, F.G.S. The author's researches lead him to the following conclusions:—Boulders from the North-Criffel range and Lake-district can be traced from the Solway Firth to near Bromsgrove (about 200 miles), and over an area in greatest breadth (from near Macclesfield to Beaumaris) of 90 miles, those from Criffel being particularly abundant near Wolverhampton. Boulders from the Arenig occupy a triangular area, limited by a line drawn northward from Chirk to the Dee estuary, and to the south-east of that town are found as far as Birmingham and Bromsgrove. The dispersion of the more distant Criffel boulders would require submergences of from 400 to 1,400 feet; of the Lake-district a little deeper; while the distant dispersion of the Arenig boulders took place at submergences between 800 and 2,000 feet. The author describes several of the more local drifts, and correlates the lower boulder-clay of the north-west with the chalky boulder-clay of the east of England. He considers floating ice, not land ice, to have been the agent of dispersion.—On the glaciation of the Shetland Isles, by B. N. Peach, F.G.S., and John Horne, F.G.S. After an account of previous opinion on the subject, the authors proceeded to describe the different islands, reviewing in succession the physical features, geological structure, the direction of glaciation, and the various superficial deposits. From an examination of the numerous striated surfaces, as well as from the distribution of boulder-clay and the dispersal of stones in that deposit, they inferred that during the period of extreme cold Shetland must have been glaciated by the Scandinavian Mer de Glace, crossing the islands from the North Sea towards the Atlantic. The authors described the order of succession in the Old Red Sandstone formation in Shetland, and referred to the discovery of an abundant series of plant-remains in rocks which have hitherto been regarded as forming part of the series of ancient crystalline rocks. The plant-remains are identical with those found in the Old Red Sandstone rocks in Caithness, Orkney, and Shetland, from which it was inferred that the quartzites and shales in which the fossils are imbedded must be classed with this formation. The authors also described the great series of contemporaneous and intrusive igneous rocks of Old Red Sandstone age, adducing evidence in proof of the great denudation which has taken place in the members of this formation in Shetland.—On the southerly extension of the Hessle boulder-clay in Lincolnshire, by A. J. Jukes-Browne, B.A., F.G.S.

Manchester

Literary and Philosophical Society, January 8.—Charles Bailey, F.L.S., in the chair.—Mr. Thomas Rogers read a paper on, and exhibited many specimens of, ballast plants collected at Cardiff in September, 1878.

February 25.—E. W. Binney, F.R.S., in the chair.—On the mean temperatures of the winters of the last twenty-nine years, by the Rev. Thomas Mackereth, F.R.A.S., &c.

March 4.—J. P. Joule, F.R.S., president, in the chair.—On a modification of Bunsen's calorimeter, by Prof. Balfour Stewart, LL.D., F.R.S.—The poisonous qualities of the yew, by William E. A. Axon, M.R.S.L., F.S.S.

March 18.—J. P. Joule, F.R.S., president, in the chair.—On siliceous fossilisation, part 2, by J. B. Hannay, F.R.S.E., F.C.S., Assistant Lecturer on Chemistry in the Owens College.

Edinburgh

Royal Society, March 17.—Prof. Kelland, president, in the chair.—Sir William Thomson communicated a paper on vortex motion, gravitational oscillations in rotating water. This paper contained an investigation of oscillations under the influence of gravity, of a mass of rotating liquid; former communications having been chiefly directed to the discovery of the vortex theory of atoms. In Laplace's great work on the theory of the gravitational oscillations of a mass of water spread over an approximately spherical body, he takes account of the fact that the earth is rotating and of the effects produced thereby on the motions of the ocean, and how these motions are affected by the great continents. Sir William Thomson finds that vortex motion due to the rotation of the earth affects the tides very considerably, even in such comparatively small areas as those of the English Channel and the North Sea. He shows that in a limited basin without an aperture, covering from, say, one to ten degrees of latitude, any tidal phenomenon which there may be, due to the gravitation attraction of the moon, is greatly affected by the rotation of the earth, if the greatest period of free oscillation of liquid in the basin is comparable with the period of rotation of the earth. It is to this fact that the peculiar phenomenon of the tides in the English Channel is due. The peculiarity is this, that for instance when it is high water at Dover, there is low water at the other end of the channel, and simultaneously a nodal line at St. Alban's Head, *i.e.*, no rise or fall there; moreover, there are currents across this nodal line towards the end of the channel at which the tide is rising, *i.e.*, water is flowing east across this line when tide is rising at Dover, and west when it is rising at the other extremity. This phenomenon holds true only for ten or twenty miles on the English side of the Channel. On the French side there is nothing of this kind but a gradual transition of the time of high tide along the coast. On the English coast, within a comparatively short distance, not more

than thirty miles, on either side of the nodal line referred to, there will be high tide on the east simultaneously with low tide on the west. He explains this by showing that in a canal of uniform breadth and depth, along which a wave is travelling, the effect of the rotation of the earth is to make the wave cling to the right hand side in whichever direction the wave is travelling. This manifests itself by the crest of the wave not being of equal amplitude all across the canal, but falling off from the right side down to nothing on the other side if the breadth of the canal is great enough. Where y is the distance of a point from the right bank and x the distance along the bank, the expression for the height of the crest is $e^{-my} \sin (px - qt)$. In a canal which has non-parallel sides, *i.e.*, in which the sides converge, the effect is more marked. This is true of the English Channel or of any other where the time of an oscillation running across from one side to the other and back, is comparable with the period of rotation of the earth. He has worked out the problem in the case of the canal mentioned above, and also for forced and free oscillations in a circular basin.—The next paper was one by Dr. Joseph Coats, Dr. Wm. Ramsay, and Prof. McKendrick, on the action of anæsthetics on the blood pressure. The question they originally wished to solve was whether, in cases where the use of chloroform destroyed life, the result is due to its effect on the respiration or to the action on the heart. They found that at first sight it affected the respiration, but by keeping up artificial respiration they found that it also had an action upon the heart. They experimented both on rabbits and on dogs with the following results:—Chloroform and ethydene chloride reduce the blood pressure, while ether has no appreciable effect. Chloroform reduces the blood pressure much more and much more rapidly than ethydene. It has also an apparently capricious effect on the heart's action, the blood pressure being reduced to nothing and pulsation being very rapid. Sometimes the heart's action was affected as much as a minute or more after the chloroform had ceased to be administered and after the blood pressure had recovered nearly its normal state. The effect of ethydene was to reduce gradually the blood pressure. Chloroform causes death in dogs primarily by paralysing either the heart's action or the respiration according to the individual's peculiarities. The respiration generally stops before the heart's action ceases. They found that artificial respiration was very effective in restoring animals in danger of dying from the effects of chloroform. Ethydene never produces absolute cessation either of the heart's action or of respiration. The results obtained confirm and amplify those of the Committee of the Royal Medical Chirurgical Society of 1864.—Prof. McKendrick showed some experiments by Mr. Aitken on the physiological action of rotating disks on the retina.—Mr. Thomas Muir gave some general theorems on determinants, viz., an expression for the product of a determinant by one of its minors; a theorem for the reduction of the order of a determinant, another for the multiplication of a determinant by an expression of a number of terms equal to the order of the determinant. He laid on the table a note on alterants.

PARIS

Academy of Sciences, April 7.—M. Daubrée in the chair.—The following papers were read:—On the iodides of stannpropyls, by M. Cahours.—On complementary pirouettes, by M. Chevreul. A disk having one half painted with colour a, and the other half white, and being rotated not more than 160 times a minute, nor less than 60, the complementary c of a appears on the white half.—Consequences of experiments made to imitate terrestrial fractures, with regard to various characters of exterior forms of the ground, by M. Daubrée. He points out several examples of the influence of diaclases and paraclases in determining the form of land, their directive influence on erosion, &c.—M. de Lesseps presented a *brochure* of the International African Association, containing a recent lecture by him, and a catalogue of African products at the recent exhibition.—The following elections were made:—M. Alphonse Milne Edwards, member in anatomy and zoology, in place of the late M. Gervais; M. Abich, Correspondent in Mineralogy, in place of M. Damour, elected Free Academician; Mr. Lawes, correspondent in rural economy, in place of the late Marquis de Vibraye.—Analysis of the physiological action of sulphates of magnesia and soda, by M. Moreau. This describes an experiment wherein, some time after ingestion of magnesian sulphate into the intestine, he introduced yellow cyanide of potassium as a test of absorption. The urine afterwards showed no trace of cyanide. The sulphate causes afflux of liquid in the intestinal cavity; so that this occurred in the present case without manifest absorption.—On the summation of a particular species of series, by M. André.—On displacements produced in the interior of an elastic ground by normal pressure exercised at a point of its surface, by M. Boussinesq.—Heat-centre produced by molecular shocks, by Mr. Crookes.—Reply to M. Flammarion's note on the declination of the magnetic needle, by M. Marié-Davy. The reason of the alleged different action of the needle at Paris he finds in the dissimilarity of the methods employed in calculation of the averages grouped in M. Flammarion's tables.—On the gravivolumeter, by M. Houzeau. In this instrument liquid is forced up out of a vessel into a siphon by blowing through a caoutchouc tube, which is then closed with a spring pincer; on pressing the latter, air enters, and the liquid comes from the siphon drop by drop, with great regularity; the numeration of the drops gives precisely the weight of the liquid.—On determination of the presence of fire-damp in the atmosphere of mines, by MM. Mallard and Le Chatelier. They use a lit jet of hydrogen, which gives a larger and more distinct blue aureola than the flame of a common safety-lamp in presence of fire-damp, and reveals the presence of even 0·25 per cent. of the latter gas. The flame, within a cylinder of copper, is viewed through a lens closing a lateral orifice.—On some conditions of alcoholic fermentation, by M. Richet. Oxygen renders more rapid lactic fermentation of milk. Boiling, by coagulating an albuminoid matter originally soluble, diminishes by one-half the activity of the fermentation. Digestive juices which give soluble albumen and peptones increase the rapidity of lactic fermentation.—On the amylaceous and amyloid granules of the egg, by M. Dastre. He opposes M. Dareste's affirmation of the presence of *amyloid* bodies in eggs, maintaining that they are certainly not starch, and have not even the appearance of it.—Determination of sugar in the blood, by M. d'Arsonval. He defends a method of the late Claude Bernard's against recent objections by M. Cazeneuve.—On the method used by Claude Bernard for determination of reducing sugars in the blood, by M. Picard. If there are some animal substances which have the same action on cupric liquors as glucosic solutions, there are a very large number which have rotatory power.—On the distribution of phosphates in the different elements of the blood, by M. Jolly. Alkaline phosphates predominate in the aqueous part of the blood. All the elements contain a variable quantity of phosphate of iron, but it is chiefly accumulated in the corpuscles.—On the formation of a peculiar amyloid matter in the asci of some Pyrenomycetes, by M. Crié. What distinguishes this essentially is (1) its formation in profound darkness by a protoplasm without chlorophyll, and (2) its insolubility in the cellular liquids. This amyloid matter, the physiological *rôle* of which is not yet known, M. Crié calls amylomucine.—On ancient glaciers in the Maritime Alps, by M. Desor.

CONTENTS

THURSDAY, APRIL 24, 1879

SCIENTIFIC WORTHIES

XIV.—JEAN LOUIS RODOLPHE AGASSIZ, BORN MAY 28, 1807; DIED DECEMBER 14, 1873

LOUIS AGASSIZ, by which names he was everywhere known, was born at Motier, in the canton of Freiberg, Switzerland, on May 28, 1807. He belonged to an old French Protestant family, who had been compelled to quit their native country by the revocation of the Edict of Nantes, we are told by Dr. Steindachner, in his paper on Agassiz, contributed to the Vienna Academy, to which we are largely indebted for what follows. His father was pastor at Motier, where his forefathers for six generations had filled the same office. Agassiz received his earliest education at home under the care of his mother, a woman of high endowments and rare culture. At the age of eleven years he betook himself with his younger brothers to the Gymnasium at Biel, in the canton of Bern, where he was mainly occupied with the study of ancient and modern languages, the knowledge of which proved of important service to him in his later biological investigations. His play-hours he devoted to fishing and the collecting of insects. Thus early did his leaning towards ichthyological researches show itself, and his knowledge of the habits of fish often astonished even experienced fishermen.

In the meantime Agassiz's father was transferred from Motier to the little town of Orbe at the foot of the Jura, and here young Agassiz became intimate during the holidays with a young clergyman named Fivaz, who first introduced him to the study of natural history, and especially botany. After four years' stay at Biel he entered the Academy of Lausanne, and in 1824 betook himself to Zurich to study medicine, in accordance with the earnest desire of his parents. Soon after, Agassiz left Switzerland to continue his medical studies at Heidelberg, where, at that time, the celebrated anatomist, Tidemann, carried on his work. After a year's stay Agassiz exchanged Heidelberg for Munich, where Schelling, Oken, Martius, Döllinger, Wagler, Zuccarini, Fuchs, von Kobell, &c., were lecturing; and these soon became not only the kind teachers, but also the friends of young Agassiz. Döllinger, especially, the great master in physiology and embryology soon recognised the high talent of his pupil, and ripened in Agassiz a long-cherished plan of devoting himself to zoology in the widest sense of the term.

It was here in Munich that the young Agassiz, who occupied a small room in Döllinger's house, soon gathered around him a circle of young and talented students, to talk over and discuss matters of scientific interest. Agassiz's room was the meeting-place of this club, which soon assumed the title of the Little Academy, and of which Agassiz acted as president. Before this society did Michaelis lay the results of his researches in the Adriatic Sea, Born exhibited his beautiful preparations of the anatomy of the lamprey, Rudolphi lectured to the students on the Bavarian Alps and the coasts of the Baltic, and Schimper and Braun here first expounded the laws of phyllotaxis. Döllinger himself did not disdain to initiate his disciples and friends of this Little Academy in his newest discoveries and ideas, ere he made them known to the scientific world, and here he taught them the use of the microscope in embryological research.

Meantime the Bavarian members of a great scientific expedition to Brazil, under the leadership of Spix and Martius, returned to Munich, bringing with them rich collections; and after the death of Spix, the celebrated botanist, Martius entrusted Agassiz with the working out of the ichthyological material. Agassiz had scarcely reached his twenty-first year when he concluded this task in so brilliant a fashion that with this, his first-born work, on the Fishes of Brazil, he gained a reputation as one of the first ichthyologists. This work was published in Latin at Munich in the year 1829, and was dedicated to Cuvier. About the same time Agassiz began his investigations on fossil fishes. The immediate occasion of this step was a short notice by Prof. Rud. Wagner on the fossils of the Munich Museum, in which he praised the number and beauty of the unnoticed fossil fishes. Agassiz immediately applied to Prof. Fuchs, who had the care of the palæontological collection, for permission to investigate the ichthyolites in detail; Professors Wagler and Schubert placed freely at his disposal the collection of recent fishes and their skeletons, and Döllinger, Oken, and Martius in various ways encouraged him in this difficult undertaking.

From this time Agassiz devoted all the spare time left him by his medical studies to the investigation of fossil fishes, which naturally implied an adequate knowledge of the most nearly related living forms; in the holiday months he made short visits to the museums of the larger towns of Central Germany, to examine their palæontological treasures. In 1830 Agassiz went to Vienna, where he stayed a year, attending at the hospitals, and studying in the Imperial Museum the splendid collection of sturgeons of the Danube region, as well as the fossil fishes of Monte Balca. Moreover, he was so interested in the Cyprinoids of the Danube, which were already partly known to him from those of the Isar, that he concluded a work on the Freshwater Fishes of Central Europe; on account of the revolution of July, 1830, this work was not published.

The years 1831 and 1832 Agassiz spent in France, and in Paris had much pleasant intercourse with Cuvier and Alex. von Humboldt. Cuvier was then giving a course of lectures on the history of natural science, and combated with all the power of his science and his detailed knowledge of the organic structure of the whole animal world the development theory of Geoffroy based on the variability of species, which the latter defended in the sittings of the Paris Academy. From this time Agassiz adhered to Cuvier's ideas on the classification of the animal kingdom and on creation catastrophes especially, and with but little modification, defended them in his teaching and writing to the end of his life.

In Humboldt, again, Agassiz found an attached and powerful patron, whose support at a later time essentially facilitated the publication of many of his costly works, and to whose recommendation he in part owed the brilliant reception he met with in America, which he chose as his second home.

The Paris Museum was then in the zenith of its reputation; its zoological, palæontological, and anatomical collections were then the richest and most celebrated of

Europe, and Cuvier granted to the young Agassiz with genuine disinterestedness and liberality the complete use of its rich material. With untiring diligence and rare perseverance, Agassiz here continued his researches on fossil fishes, and anew worked thoroughly through the rich palæontological collection of Conte Gazzola, which contained the originals of Volta's celebrated treatise "L' Ittiologia Veronese."

In 1833 Agassiz again visited the great museums of Germany and Switzerland, and had already distinguished 500 species of fossil fishes, when, in August, 1834, he sailed for England, in order to study there the exceptionally rich public and private collections, in which he found 300 new species. In the year 1835 and 1840, he extended his journeys into Wales, Scotland, and Ireland, visiting London again and again, in order there to test the notes collected for many years for the completion of his work, and to make himself acquainted with the newest discoveries in the department of palæontology.

In 1844 Agassiz concluded the publication of his gigantic work on the Fossil Fishes, which appeared at Neuchâtel in five quarto volumes, with 311 folio plates. Eighty of the greatest museums of Europe had furnished the material for it, and the number of described species amounted to 1,700 in about 20,000 examples. The "Recherches sur les Poissons Fossiles" is undoubtedly Agassiz's most important work, and forms, with Cuvier's and Valenciennes' "Histoire Naturelle des Poissons" and Johann Müller's treatises, the foundation of our present knowledge of fishes, while it does not confine itself to the region of ichthyolites, but extends over the entire wide field of the anatomy and classification of fishes, essentially modifying the latter. Agassiz considered, and with justice, that the separation of the ganoids from the other fishes into the rank of a special order, as the greatest step towards progress for which science was indebted to him; and, on the basis of the comparison of the fossil fishes of all formations with living forms, he enunciated several generally valid laws, which have had an important bearing on the development theory of the whole organic world.

As a special fruit of Agassiz's stay in England appeared in 1844 and 1845, a monograph on the Fossil Fishes of the Devonian System and a smaller treatise on the Fishes of the London Clay.

As early as 1832 was Agassiz called to be professor at Neuchâtel, and in a short time raised the little town to be a chief seat of science in Switzerland. He created a Natural History Museum, and was the chief founder of the Scientific Society, which issued the first volume of its *Memoirs* in 1835. From all parts of Switzerland came young and talented pupils and friends of nature thither, and gathered round Agassiz, who understood how to inspire them with his great ideas. They followed him in his frequent zoological and geological excursions in the Jura and the Alps, and assisted him in procuring scientific material and helping him in the time-consuming preliminaries of those numerous works which date mainly from the years 1835 to 1845. Along with his friends Desor and Valentin, Agassiz published the great monograph on living and fossil Echinoderms.

The rich collection of fossil conchifera of Switzerland, which a young and able geologist of the name of Gressly had brought back from his travels, led Agassiz to work out the fossil conchifera of the Jura and the chalk, the result being published under the title of "Études Critiques sur les Mollusques fossiles du Jura et de la Craie," in four parts with 100 plates. This was followed by several similar supplementary publications on fossil conchifera, of which the "Iconographie des Coquilles tertiares, reputées identiques avec les espèces vivantes ou dans différens Terrains de l'Époque tertiaire," and "Mémoire sur les Moules de Mollusques vivans et fossiles," are the most important.

In spite of these numerous and comprehensive works, Agassiz found time to devote to the study of living fishes. Thus may be mentioned his treatise on the cyprinoids (1834); the great work brought out in conjunction with Carl Vogt between 1839 and 1845, on the freshwater fishes of Central Europe; in 1842 he brought out his most important "Nomenclator Zoologicus," the result of many years' gatherings, and which contained an alphabetical arrangement of the specific names of the entire animal kingdom, their etymology, information as to the authors who had proposed these names, as well as the year of their appearance. The "Nomenclator" found a magnificent conclusion in the "Bibliographie générale d' Histoire Naturelle," printed at the cost of the Ray Society, unfortunately not without some ugly mutilations on the part of the editor.

While Agassiz no doubt exercised a considerable influence on geology by means of his palæontological researches, still it is as a glacealist that his name will always be prominently associated with that science. Venetz', Schimper's, and especially Charpentier's observations and theories on the greater extension of glaciers, and their relations to erratic blocks attracted the attention of Agassiz in 1836 to the glacial phenomena of Switzerland. Charpentier's theories on the former extension of glaciers and other points especially interested Agassiz, who gave himself with his peculiar energy and fertility of idea to the study of glaciers. With Gyot, Desor, Studer, and other young friends, did Agassiz during several years visit most of the glaciers of Switzerland, and examined them in their entire extent, from their origin to their lowest margins. In 1841 was the ascent of the Jungfrau undertaken. In the middle of the Aar glacier, at a height of 8,000 feet above the sea, twelve miles from any human habitation, protected by a huge block, was a station erected, which latterly obtained a European celebrity, under the name of "Hôtel des Neuchâtelois." Here for fully eight years were researches carried out on the origin of glaciers, the forward and backward oscillations, the structure and thickness of the ice, as well as its formation, the origin of moraines, &c. In 1840 appeared Agassiz's first great work, "Études sur les Glaciers," in which he thoroughly discussed the chief phenomena of glaciers, and developed his views on their earlier extension. In a second work, "Système glacial," he gave a satisfactory account of the observations made in the years 1841–45, especially on the progress of glaciers in various years and under the influence of conditions of temperature. The adoption of a special glacial period was the final result of Agassiz's research among the glaciers of Switzerland, as well as those of Scotland and Wales. An immense ice-bed, the

result of a depression of temperature toward the end of the tertiary covered North and Central Europe, Asia, and North America, and a similar phenomenon was also found to have occurred in South America, from the South Pole to Monte Video and Chile, as Agassiz ascertained during the *Hassler* Expedition of 1872. The immense importance of this theory, both in geology and biology, was soon recognised, and its discussion has engaged the earnest attention of the ablest men in all departments of science. In the autumn of 1846 Agassiz went to America, partly on a commission from the Prussian Government and partly to fulfil an engagement to give a series of lectures on Comparative Embryology at the Sewell Institute of Boston. This course of lectures led to important results; it aroused an enthusiasm for the study of nature in the widest circles, and Agassiz understood how to make the scientific development of North America in this direction a matter of honour for the whole nation. The offer of the use of the steamers of the Coast Survey led to a scientific cruise in the summer of 1847 along the coast of Massachusetts, followed a few years later by a second larger cruise to the coral reefs of Florida.

After Agassiz had been released from his scientific mission by the Prussian Government, he accepted with pleasure the Chair of Zoology and Geology in the Lawrence Scientific School of Harvard College, Cambridge, created specially for Agassiz by the founder of the school, Mr. Abbot Lawrence. Agassiz thus gave up all thought of returning to Europe; he placed his activity, his science, and his talents, at the disposal of the nation that showed itself so anxious to keep him, and where he would enjoy a social power and a liberty which were hardly possible to the *savans* of the Old World.

As in Neuchâtel, so in Cambridge, Agassiz in a very short time attracted around him a circle of young men, enterprising lovers of natural science. With these, in June, 1848, he undertook a journey to the then little-known region of Lake Superior. In 1850 appeared his well-known work, "Lake Superior: its Physical Character, Vegetation, and Animals," in which Agassiz discussed in detail the erratic phenomena of the lake, its future form and extent, the character of its ichthyological and reptilian fauna, while Cabot, Harris, Gould, and J. Leconte worked out the rest of the collections. In succeeding years he made similar expeditions with his pupils into the interior of the United States, and with the collections brought back laid the foundation of a natural history museum, which, until then, had no existence at Harvard University.

In 1852 Agassiz went to Charleston as Professor of Zoology and Comparative Anatomy, but returned to Cambridge after two years, the warmer climate of the south not agreeing with him. Soon after he visited all the great towns of North America, lecturing in all departments of zoology and geology. Everywhere he was received with enthusiasm, for his expositions were remarkably clear and full of suggestive thought, his language noble and fluent, his knowledge of human science of the widest, his manner so charming and his conversation so full of the highest instruction, that every one felt it a privilege to be near him. From that time Agassiz became the declared pet of the Americans; he was the most popular man in the whole broad land, and in intellectual matters became its greatest benefactor, exercising his influence in improving education and increasing educational establishments.

In 1855 Agassiz began to prepare for the publication of a magnificent work, "Contributions to the Natural History of the United States," he having already published several papers on the subject in American journals. In a short time the necessary means were obtained by subscription, and in 1857 the first volume appeared, dedicated to Döllinger and his generous friend Francis Calley Gray. Unfortunately this work only reached the fourth volume. The first volume contained as introduction the universally-known and much-discussed "Essay on Classification," which latter, as a separate work, was published in London in 1859, and in an enlarged French translation in Paris, 1869. Agassiz treated in this work the questions of the origin, development, and systematic arrangement of the organic world, and developed from these his philosophical views which he had obtained from his own studies and observations, and which stand in direct opposition to the Theory of Descent.

Agassiz's collections had grown so enormously that the accommodation at his disposal was quite inadequate. By the liberality of Mr. F. C. Gray and the State of Massachusetts, as well as Cambridge University, a great Natural History Museum was begun in June 1859, and by December was so far advanced, that the greater part of Agassiz's collections could be transferred to it. From this time the improvement and completion of this museum became the chief object of Agassiz's activity. He aimed at making it in comprehensiveness and suitability for its purpose, a pattern institution for the whole world, and fitted to give the friends of natural history all possible help in their researches. The Museum of Zoology and Comparative Anatomy is much better known to the public of Cambridge and Boston as "Agassiz's Museum."

In 1864-65, Agassiz somewhat broke down from his continued labours, and he was advised to travel. He decided to visit Brazil, the fish of which furnished him with the subject of his first work. With six assistants he left New York in April, 1865, for Rio Janeiro. The party divided to work in various directions, Agassiz, himself, selecting the Amazon as his sphere, sailed up the river to Manaos, at the Mouth of the Rio Negro, and thence to Tabatinga. During the journey from Pará to Manaos 300 species of fish were collected, of which one half were drawn from life by Burkhardt. His headquarters were at Teffe and Manaos, where he studied the habits of the fish in their migrations in the main stream, and several of its tributaries. While he stayed here, his assistants explored some of the other tributaries of the Amazon, while others explored the regions on the Rio Francisco, Rio Doce, Paranahyba, &c. In July of the following year Agassiz returned to the United States with such a collection of booty as would have filled another Museum. With the co-operation of Agassiz, his wife, the true companion of her husband, and full of sympathy for his ideas, brought out the journal of these remarkable travels, which in a short time reached a sixth edition in America, and was translated into French in 1869.

Again, in the end of 1871, Agassiz left for South America, on board the war-ship *Hassler*. Count Pour-

tales, in this well-known expedition, had charge of the deep-sea researches, while Dr. Steindachner with Agassiz, was responsible for the other zoological collections. The results of this expedition are well known to naturalists. The leisurely cruise along the coast of Patagonia and Chili gave Agassiz an opportunity of studying the glacial phenomena of South America. His stay in San Francisco and Sacramento gave an impulse of the greatest importance to education and science in these towns, and in the latter led to the creation of a Natural History Society, which was named after him, the Agassiz Institute.

The history of the Penikese School of Natural History must be so fresh in the memory of our readers that we need not here repeat the details. The success of the school, modelled somewhat after that of Dohrn, at Naples, exceeded all expectation, the accommodation being quite inadequate for the number of students who appeared. At the end of the first summer his pupils bade him a long good-bye in the hope of meeting their much-loved master next year. But the additional burden seems to have been too great for the strength of the never-resting devotee of science. After scarcely eight days' illness, he died at Cambridge, December 14, 1873, in his sixty-third year, in the height of his fame. He has been justly named by his fellow-citizens of the States the "Humboldt of America." Ever amiable and open in intercourse, stimulating and instructive, clear and concise in exposition, was Agassiz; and his numerous pupils, of whom several have developed into important workers in science, as Alex. Agassiz, Stimpson, Putnam, Shaler, Wilder, Morse, &c., clung to him with truly child-like love and respect. The news of his unexpectedly sudden death shocked the whole population deeply, for America had lost in him one of her citizens of whom she had the best right to be proud.

Besides Dr. Steindachner's paper, we would refer the reader who desires further details to a paper in the *Revue des Deux Mondes* for July and August, 1875.

WATERTON'S LIFE AND TRAVELS

Wanderings in South America, the North-West of the United States, and the Antilles, in the Years 1812, 1816, 1820, *and* 1824. With Original Instructions for the Perfect Preservation of Birds, &c., for Cabinets of Natural History. By Charles Waterton. New Edition. Edited, with Biographical Introduction and Explanatory Index, by the Rev. J. G. Wood. With 100 Illustrations. (London: Macmillan and Co., 1879.)

THE reading world will feel grateful to both author and publisher for this handsome edition of one of our classical books of travel and natural history; while those who are already familiar with the work will read with interest and pleasure the excellent biographical notice of Waterton here given. We have first a sketch of his school and college life, when his taste for natural history got him into many scrapes; but we learn that the Jesuit fathers at Stonyhurst wisely utilised his irrepressible love of animals by making him rat-catcher and general vermin-killer to the establishment. We next find him travelling on the Continent, where he had a narrow escape of dying of the plague at Malaga. He visited Gibraltar, and saw a whole colony of the well-known apes which were then far more abundant than now. He speculates on the "tremendous convulsion of nature" which had opened the channel of the Straits, observing that—"if apes had been on Gibraltar when the sudden shock occurred, these unlucky mimickers of man would have seen their late intercourse with Africa quite at an end"—a passage which recalls to us those extreme catastrophist doctrines in geology which are now happily extinct.

When his wanderings in South America were at an end he settled down in his ancestral Yorkshire home, Walton Hall, devoting himself to the management of his estate and the study of nature, and living a life of the most Spartan simplicity. His single room had neither bed nor carpet. He always lay on the bare boards with a blanket wrapped round him, and with an oaken block by way of pillow. He went to bed at eight, and was up, dressed and clean shaven every morning at four, having himself lit a fire and boiled water to shave with. His devotions and reading occupied him till six; his bailiff's report, writing and business till eight, his breakfast hour; so that he had done a fair day's work before most people are out of bed. His room was at the very top of the house; he never touched fermented liquors, and took very little meat.

His great delight was in studying the habits of birds and other wild animals; and he devoted his park of over 250 acres to this purpose. He had moats, and ponds, and swamps, woods and trees of all kinds; and he spent 10,000*l*. in surrounding the whole with a wall nowhere less than eight feet high, in order to keep out poachers and animal intruders. In this domain no gun was ever fired or anything done to disturb the feathered inhabitants. The very year after the wall was finished the herons came and established themselves in the park, where they had never bred before; and, as Mr. Wood remarks, it is strange that they should have known that the wall, which they themselves could so easily pass, would be any protection to them. He constructed a yew fortress for pheasants, built a cat-proof tower for starlings, and a lofty dovecot to secure his pigeons from poachers. Owls and titmice and many other birds had special haunts constructed for them, while rats and other bird-enemies were carefully trapped or poisoned.

Waterton was one of the kindest and most humane of men. He studied the comforts of his horses, his dogs, and even of his pigs, as if they had been human beings. He had his gates specially constructed so that his horses and cows could lean over them and converse together, without inconvenience to themselves or injury to the gates. When he took possession of a deserted country house in Demerara, tenanted by frogs and snakes, owls and vampires, he tells us in his quaint language,—"The frogs, and here and there a snake, received that attention which the weak in this world generally experience from the strong, and which the law commonly denominates an ejectment. But here neither the frogs nor serpents were ill-treated; they sallied forth, without buffet or rebuke, to choose their place of residence; the world was all before them. The owls went away of their own accord, preferring to retire to a hollow tree rather than to associate with their new landlord. The bats and vampires stayed with me, and went in and out as usual." Even when, going down the St. Lawrence, he caught, crawling on his neck, the only bug he saw in North America, he "thought of my uncle Toby and the fly;" and so, instead of killing it, he "quietly chucked it among some baggage that was

close by, and recommended it to get ashore by the first opportunity."

Any wild animals that he does not actually want for specimens he treats in the same way, and it is therefore not surprising that he looks favourably on the Indian and his mode of life. Ignorant travellers and colonists call the Indians a lazy race; "but," he remarks, "man in general will not be active without an object. When an Indian has got plenty to eat, what need has he to work? He has no idea of making pleasure-grounds. Money is of no use to him as there are no markets for him to go to, nor milliners' shops for his wife and daughters. He has no taxes to pay, no highways to keep up, no poor to maintain, no army nor navy to supply. He lies in his hammock both night and day (for he has no chair nor bed, neither does he want them), and in it he forms his bow, and makes his arrows, and repairs his fishing-tackle. But when his provisions are gone he rouses himself, and scours the forest in quest of food. He plunges into the river after the deer and tapir, or passes through swamps and quagmires, and never fails to obtain food. Should the approach of night check him while hunting, he lays him down in the forest and continues the chase the next morning till he is successful. With us the poor or needy man has to work every day and all day long for a maintenance, but should this man acquire a fortune he usually changes his habits." Waterton then amusingly sketches for us the life of an idle man for a single day, and concludes:—"Now, could the Indian in his turn see this, he would call the white men a lazy, indolent set. Perhaps, then, upon due reflection, you would draw this conclusion: that men will always be indolent when there is no object to rouse them."

Not even Gilbert White was a closer or more accurate observer of the habits of animals than was Waterton, and had he recorded all his observations during the forty years he lived at Walton Hall we should have had a work in no way inferior to White's "Selborne." There is one curious observation of his which throws some light on the origin of one of the superstitions of natural history, but which seems to have been entirely overlooked. The name *Caprimulgus*, or "goat-sucker," has its equivalent in many European languages; and the belief that this bird sucked goats or cows has been prevalent since the time of Aristotle. The only foundation for this wide-spread belief, suggested in any ornithological book to which I have access, is, that the goat-sucker is often found near sheep-folds and cattle-pens on account of the abundance of insects in such places. Pliny however says that they enter the folds and fly to the udders of the goats in order to suck the milk. This is a much more definite statement, and, strange to say, Waterton supports the *fact* thus stated by his own observation, and at the same time shows how the erroneous *inference* arose from this fact. At p. 233 of this volume we find the following:—"I am fully persuaded that these innocent little birds never suck the herds; for when they approach them, *and jump up at their udders*, it is to catch the flies and insects there. When the moon shone bright I would frequently go and stand within three yards of a cow, *and distinctly see the caprimulgus catch the flies on its udder*." The passages marked in italics are most remarkable, since they directly confirm Pliny's statement that the birds "fly to the udders of the goats." It is not quite clear by the context whether Waterton made this observation in Demerara or in England. He is describing the habits of the Demerara goat-suckers at the time, but as he has said nothing about there being any cows on the deserted estate where he was staying, he may in this passage be referring to his observations at home.

In another passage at p. 198 this is certainly the case. He says (according to his custom addressing his reader as if speaking to him):—

"When the moon shines bright you may have a fair opportunity of examining the goat-sucker. You will see it close by the cows, goats, and sheep, jumping up every now and then under their bellies. Approach a little nearer,—see how the nocturnal flies are tormenting the herd, and with what dexterity he springs up and catches them, as fast as they alight on the belly, legs, and udder of the animals. Observe how quiet they stand, and how sensible they seem of his good offices, for they neither strike him, nor hit him with their tail, nor tread on him, nor try to drive him away as an uncivil intruder."

There can be no doubt that these are Waterton's own observations at home, though expressed rather generally; but the other passage, at all events, written in the first person, is far too definite a statement to be doubted, coming from such an observer; and it is curious that no modern writer on the subject appears to have referred to it.

As a capturer of snakes Waterton was pre-eminent, his fight with the great boa constrictor, and his capture single-handed of a smaller one, which he allowed to coil round his body while he held its neck in his two hands, are well-known incidents in his "Wanderings;" but Mr. Wood tells us how he coolly manipulated live rattlesnakes in the presence of a number of friends at Leeds, transferring them from one box to another with his bare hands. His secret was, simply, that if a snake is not frightened by noise or sudden movements, its natural sluggishness prevents it from resenting cautious handling.

We quite agree with the editor that few books have ever been written so thoroughly truthful and accurate, and so entirely free from exaggeration as those of Waterton; yet his veracity was often doubted by his reviewers, and he was classed among travellers of the Munchausen type. This however he little cared for, but he did not like to be called eccentric. He thought himself the most ordinary of human beings, though he climbed trees barefoot and never in his life wore a black coat. "Yet," as Mr. Wood well says of him, "had he not been eccentric he would not have been the Charles Waterton so long known and loved. . . . It was eccentric to come into a large estate as a young man, and to have lived to extreme old age without having wasted an hour or a shilling. It was eccentric to give bountifully and never allow his name to appear in a subscription list. It was eccentric to be saturated with the love of nature. It might be eccentric never to give dinner-parties, preferring to keep an always open house for his friends; but it was a very agreeable kind of eccentricity. It was eccentric to be ever childlike but never childish. We might multiply instances of his eccentricity to any extent, and may safely say that the world would be much better than it is if such eccentricity were more common."

So far we have had only praise for this book, and

though we have said nothing yet about the illustrations, they are also worthy of commendation as really illustrating the matter in hand, and being for the most part of excellent quality. But now we have the less pleasant duty of finding fault. Waterton had a strong prejudice against the use of scientific names. He tells us that the Salempenta is excellent eating; that you hear the voice of the Hannaquoi at early dawn; while such words as Conanacouchi, Labarri, and Karabimiti are continually used without any explanation of their meaning. In pursuance of his duty as editor Mr. Wood undertakes to clear up all these points, and to make the path easy both for the general reader and the scientific naturalist; and he does this by means of an "Explanatory Index," which occupies nearly one-third of the volume, and of which he says in his preface that he believes "there is not a single living creature or tree mentioned by Waterton concerning which more or less information cannot be found in this Index."

The index referred to does undoubtedly contain a great deal of useful and interesting information, but it is also full of the most extraordinary and misleading errors, which seem to show that Mr. Wood participates in his old friend's contempt for scientific names, since he evidently thinks accuracy in these names of little importance. First we have several completely obsolete names given, which the reader would in vain look for in any modern book on natural history; such as Champsa for Alligator, and Arapunga instead of Chasmorhynchus as the name of the bell-bird. Then we find misspelt or misplaced names; as *Derotypus coronatus* instead of *Deroptyus accipitrinus* for the name of the sun-parrot, and *Helias eurypyga* instead of *Eurypyga helias* for the sun-bittern. More important are the completely wrong identifications of species, or the mixing together of two quite different animals. The ant-thrushes are said to belong to the genus Pitta, which is eastern, whereas they form a peculiar American family, Formicariidæ. The feathers of the "wild turkey," a bird which does not exist in South America, are said to be used by the Indians of Demerara. The "hannaquoi," or motmot, is said to be named *Ortalida motmot*, and the description mixes up the real motmot (Momotus) and the gallinaceous Ortalida, saying that the eggs are blue and that the bird can be easily tamed and feeds with the poultry; which is certainly not true of the motmot, of which a figure is given, and which is a solitary forest bird whose eggs are white and which never walks on the ground. The "kurumanni" wax is said to be produced by a wild bee named *Ceroxylon audicola*, which is the name of the wax-palm of the Andes. The name of the "coral-snake" is given as *Tortrix scytale*, whereas the species belongs to a quite distinct family, being either an *Elaps* or a *Pliocerus*; while the deadly "labarri" snake is named *Elaps lemniscatus*, though, from the description Waterton gives, it is almost certainly a Craspedocephalus. The red grosbeak, which Waterton mentions as a rarity he was long in search for and gives a recognisable description of, is called *Cardinalis virginianus*, a bird not found in Demerara; whereas it is almost certainly the *Pitylus erythromelas*. The little tiger-bird is said to be a Tigrisoma or tiger-bittern; but Waterton's description shows it to be *Capito cayanensis*, a fruit-eating bird of a totally distinct family. The "yawaraciris" are said to be manakins of the genus Pipra; but the description in the text clearly points to the well-known "blue creepers" of the genus Cœreba. The jay of Guiana described by Waterton, and which Mr. Wood could not determine, is the *Cyanocorax cayanus*, while the "grand gobe-mouche," which is omitted from the index, is easily recognisable as the *Querula rubricollis*. Of the plant identifications I am not prepared to speak, except to remark that the cultivated pineapple is certainly not a species of *Pitcairnea*.

It is to be hoped that this delightful work will come to a second edition, and admit of these blemishes being removed. It would also be a great convenience if references were added to the explanatory index, to avoid the trouble of first going to the index proper and then back to the body of the work. These, however, are matters which, though important to the student who keeps the book for reference, will not much affect the enjoyment of the general reader; and I can therefore cordially recommend all who have not made the acquaintance of the "Wanderer" to do so in the pages of the present volume.

A. R. W.

OUR BOOK SHELF

Ueber ehemalige Strandlinien in anstehendem Fels in Norwegen. Dr. R. Lehmann. (Halle, 1879.)

PROBABLY no feature of Scandinavian geology has been more frequently discussed than the remarkable lines of terrace which have been traced along the slopes of the coast, even up into the far northern fjords. Certainly no stranger, even if ignorant of geology, can visit these regions without being impressed by the freshness and persistence of these "parallel roads," which wind in and out among the intricate navigation of strait and sound, islet and archipelago. From the time of Celsius downwards a continually increasing literature has been devoted to this subject, and now Dr. Lehmann, of the Realschule, in Halle, adds another essay to the pile. He discusses at length and rejects the theories of erosion by glaciers and by floating ice, and adopts that of breaker-action. But probably no exclusive theory is correct. Unquestionably Norway has been overridden by land-ice, scarped and notched by coast-ice, as well as cut into by tides and breakers. That the terraces mark lines of former sea-level seems so self-evident that it hardly deserves more than a simple mention of the fact. But when these lines were cut out of the rock and the land was a hundred feet or more lower than it is now, the coasts were doubtless cumbered with ice, and while the breakers were grinding out a platform from the solid rock, their work was probably expedited by drifting masses of floe-ice. Dr. Lehmann's pamphlet is useful for the collected references it contains to recent literature on the subject. But it is needlessly voluminous.

Die Lust an der Musik. Erklärt von H. Berg. B. Behr's Buchhandlung. (Berlin, 1879.)

THIS is a little pamphlet which we have perused with no small amount of disappointment. After a short chapter treating of the origin of music, in which the author merely recapitulates the theory expounded by Darwin long ago, we come to Chapter II., on the development of music, in which the author states very little that has not before been stated by Darwin, and particularly by Helmholtz, in his "Lehre von den Tonempfindungen." The principal chapter, viz., that on the effects of music, in which we expected to find the explanation promised in the title of the pamphlet, or at least the expression of some new ideas on the subject, occupies but four small

pages, and contains merely a few illustrations of the capacity inherent in music of modulating the pleasant sensation it produces in the mind of man in a number of various ways. An appendix treats of the pleasure man derives from the aspect of colours, certain forms, and the beauty of the human body.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

On the Spectrum of Brorsen's Comet

THE observations of Prof. Young on the present appearance of the spectrum of Brorsen's comet are of great interest, from the circumstance to which he refers in his letter in NATURE, vol. xix. p. 559, namely, that in 1868 I found the positions of the three bright bands of this comet not to agree with those of other comets which I showed to be coincident with the bright bands in the spectrum of flames containing carbon.

The care I bestowed upon the determination of the apparently anomalous character of the spectrum of Brorsen's comet in 1868 gives me great confidence in its approximate accuracy. I wish now to call attention to the fact that a spectrum apparently essentially similar to the peculiar one exhibited by Brorsen's comet in 1868, was observed at Dunecht by Lord Lindsay in the case of Comet C 1877 (Borelly's). It is remarkable that another comet, Comet B (Winnecke's) 1877, observed by Lord Lindsay on the same evening (May 6) presented the ordinary cometary spectrum.

Lord Lindsay's diagram in the *Monthly Notices* R.A.S. (vol. xxxvii. p. 431) of these two spectra agrees as nearly as can be expected in such observations with my diagram in the *Phil. Trans.*, 1868 (Pl. xxxiii.), contrasting the spectrum of Comet B, 1868, with that of Brorsen's comet.

It may be accepted, therefore, as beyond doubt that the unusual form of spectrum of Brorsen's comet in 1868 is occasionally presented by comets. The great interest of Prof. Young's observation lies in the information which it gives us that the *same comet* may present on one occasion one spectrum, and on another the other spectrum.

I regret that the special arrangement of my telescope for photographic work does not permit me to observe the spectrum of Brorsen's comet at its present appearance.

Upper Tulse Hill WILLIAM HUGGINS

The Migration of Birds

IN NATURE, vol. xix. p. 433, there is a notice of my paper "Ueber das Wandern der Vögel," to which I have somewhat to reply.

However agreeable it is to me that my views should be communicated to your readers, and however little I object to their being submitted to rigorous criticism, I must still also desire that this criticism be fair.

I believe it is due to differences of national customs that your reviewer has not quite satisfied this desire. We make, perhaps, in Germany a sharper distinction between a *scientific* treatise and a *popular* work than in England. Of the latter we do not require that it bring forth what is *new*, but only that it should give what it has to give in a clear and easily intelligible manner. Nor do we require completeness of such a work, or even a criticism of the scientific works on which it is based; indeed, it is generally left to the author how far to cite his sources of information and how far not. In the *scientific* treatise it is quite otherwise; here only that is of value which is new; the theme must be treated *exhaustively*; the sources must always be named and dealt with critically, &c.

Now my publication is a lecture, which was delivered *before a company of educated ladies and gentlemen, and so before mere laics*, and a year and a half afterwards was printed in Virchow and Holtzendorff's Collection of Popular Lectures. *It thus belongs unquestionably to the category of popular writings.*

For this reason your severe critic had no occasion to point out that in my lecture there is much that had been long known, that sources are named but rarely, and that no scientific criticism is exercised. That is quite a matter of course in a popular work, at least in Germany. Mr. Newton would have had much better right to feel surprised *that even any new ideas were contained in it.*

My original aim in this lecture was merely to make my hearers acquainted with the new facts and views on the migration of birds, as they have been established by Wallace, Middendorff, and especially by Palmén. As I followed the new facts theoretically to their consequences, there arose perhaps some new ideas, which I should be glad to find verified in the future.

It is further a matter of course that, notwithstanding the *popular* form of my work, I stand by all that I have said; but I must protest against being made responsible *for what I have not said!*

Thus, *e.g.*, I have nowhere said that I hold Palmén's routes of flight for "absolute truths." I am rather quite of Mr. Newton's opinion, that these routes are merely *inferred*, not *directly* observed, and therefore that they are to a certain extent "conjectural." In this sense, however, the routes of birds must ever remain comparatively "conjectural," unless one were to follow the birds in a balloon. But while "conjectural," Palmén's routes are yet inferred by a purely scientific method, and I doubt not that most of them will in the main be confirmed by further observations. Precisely in the application of this method lies Palmén's great merit, and it is only to be hoped that ornithologists will follow further in his footsteps, and correct his mistakes by accumulation of new facts. That Palmén's routes contain some errors I do not doubt; I should rather wonder if it were not so.

Little, however, comes of this with reference to the questions which are treated with special fulness in my lecture, *the origination of the instinct of migration, and the powers by which the bird reaches its distant goal.*

I have, further, nowhere said that birds fly over the sea at a height of 20,000 feet, but have merely cited the fact that birds have been seen at such height; with reference, of course, to explanation of their flight over the sea. I believe that birds, in flight over the sea, do not close their eyes, but exercise their keen eyesight as far as possible. Therewith, however, it is not said (as Mr. Newton imputes to me) that in *all* flights over the sea they always keep the land in sight.

I desist from adducing further misunderstandings by Mr. Newton, and come to what *I have actually said* and am minded to maintain.

In agreement with Palmén, I have expressed the opinion, *that migratory birds have no special sixth sense*, as Middendorff has assumed, but that *they find their way only with the help of their ordinary five senses.*

Mr. Newton seems to be of a different opinion. He does not say, indeed, whether he agrees with Middendorff, but he brings forward observations which appear incapable of harmony with my view.

First, there appear in New Zealand two species of cuckoo (*Chrysococcyx lucidus* and *Eudynamis taitensis*) which regularly fly some 1,000 miles' distance over the ocean. I believe with Mr. Newton that the birds cannot fly so high as to see at once New Zealand and the Norfolk or Kermadec Islands, though on the former is a hill of 1,000 feet. Likewise I will accept the case of *Charadrius pluvialis* as a regular guest of the Bermuda Islands, and a doubtful *Charadrius* species as regular guest of the Sandwich Islands. *All these observations are, indeed, still very imperfect*, inasmuch as it is not known whence the birds come nor whither they go; but so much seems certain, that they do *regularly* fly over large stretches of ocean in which are almost no islands or rocks, and which are so great that they must of course also fly by night.

What then? Are we therefore compelled to make the assumption, with Middendorff, of a sixth sense, which informs the bird which direction is north? Is there no simpler explanation of the fact? Obviously, we should only be warranted in accepting such a purely hypothetical sense, if it were clearly proved, that we could never get to understand the facts without it.

The question had already occupied me, before I knew of Mr. Newton's examples. I omitted it in my lecture, because it seemed to lead me further into the region of hypotheses than I considered I could answer for before my audience.

I do not believe that we are necessitated by the far sea flight of birds, to assume a sixth sense. Is it not conceivable that birds are capable of keeping exactly the same direction of flight for many hours together, and so to fly somewhat like a shot ball or a steamship with rudder bound fast? From the physiological side, it might of course be objected that a very slight difference in the strength of the right and left wing-beats must cause a deflection from the original course, just as in the case of rowing without a steersman, a constant control by sight is necessary, if the right direction is not to be lost. To this might be replied, however, that birds are so accomplished in flight, and that we may assume they have *an extremely fine muscular sense.* Besides, they migrate mostly in company, and an error in flight of one bird will be easily corrected by the others.

But how do they hit the direction in flying away from the coast? They must be able to exactly measure the angle at which they ought to leave the land. Therein, of course, a quite small error would involve great deflections from the proper course, *but do we know that this does not actually occur often enough? and may it not be supposed that in many cases corrections are made in the flight, as soon as any point of orientation again emerges in the circle of vision? So* much we at least know, that even on land birds wander not infrequently. And it is at least not demonstrated *in any one of the cases cited by Mr. Newton,* that the birds referred to appeared on those islands *every year,* nearly *at the same time and in the same number.*

Mr. Newton adduces a second series of "facts" which seem to be against the sufficiency of the five senses; *but are these really facts?*

The young, scarcely three months old, of many of our birds, are said to pursue their flight southwards in autumn *alone.* Is that certain? and have we not here, perhaps, a too ready deduction of general rules from a few well-observed cases? Mr. Newton even says: "This seems to happen with nearly all the accipitres," &c. He quotes a letter from M. Gätke, stating that in July "Young starlings pass over Heligoland by hundreds of thousands *without a single old bird* accompanying them." I confess that I cannot regard this as a *fact*, but as a more or less probable *conjecture;* for M. Gätke, though an excellent ornithologist, could not possibly have inspected a hundredth part of these "hundreds of thousands" of starlings flying about.

I do not mean to assert that these or the other data are false; they may well be correct. I merely hold that we must guard against building far-reaching theoretical inferences on observations the general validity of which is not in the least demonstrated.

But even supposing that all these data are correct; further, supposing it certain, that these young birds, which go forth alone, also actually find the route of the species with the same certainty as if they had known it long before, would these facts be explained by the supposition of a magnetic sense? I think not. For in that case, what must have been born with the young bird? Merely this magnetic sense? *i.e.*, the power of directly perceiving external direction in its own body? By no means. There must also be born with the young bird the consciousness of what angle to the magnetic meridian it must shape its flight at.

But much more than this. It has been long known that birds, so long as they are migrating over land, frequently alter their direction; hence, supposing the young bird to be guided by a magnetic sense, there must be born with it the tendency to fly (say) twenty miles at an angle of 45° to the magnetic meridian, then 100 miles at an angle of 27°, and so on. That this is a physiological absurdity, no one would deny.

For these reasons I hold that *a special sense for direction does not exist in birds*, and that the phenomena of migration, however wonderful they appear, yet cannot ultimately depend on magic (*Zauberei*), and in this Mr. Newton no doubt agrees with me. Hence, nothing remains but to try to explain these phenomena by the known physical and mental properties of birds; *for there is no third course.*

I shall be rejoiced if Mr. Newton succeed with this better than I. AUGUST WEISMANN

Freiburg im Breisgau, March 31

THE editor having afforded me the opportunity of seeing the foregoing remarks, it will, perhaps, be convenient to the readers of NATURE that I should here add the comments I have to make upon them.

I deeply regret if my criticism of Dr. Weismann's treatise or lecture be open to the charge of unfairness. I had no wish to misrepresent him, and I cannot see that I have been guilty of such an act—indeed, the wide publication of his theory would render any attempt to do so futile. As to his acceptance of Dr. Palmén's conjecture for "absolute truths," I must urge that he took no exception to any of them, while, in the case of his Bernacle or Brent Goose, he especially adopted (p. 27) that route X which I had particular reason to consider unfounded. I did not assert that Dr. Weismann spoke of birds flying over the sea at the height of 20,000 feet, though there seems no reason why some might not, if they can do so over the land; nor did I impute to him that they always keep land in sight. I had no need to declare my disbelief in Dr. von Middendorff's magnetic hypothesis, for I never met with any man that held it. I had spoken of it already elsewhere (*Encycl. Brit.* Ed. 9, iii., p. 769), and I considered it had been set at rest for ever by Prof. Baird in the article I cited. In like manner it seemed useless to disclaim any belief in the possession by birds of a "sixth sense" which is not common to ourselves and other animals. My only object was to show that Dr. Weismann's theory was inconsistent with certain facts, and nothing he has since adduced makes me think it otherwise. As to some of these "facts" he is incredulous, and I have no fault to find with his caution in this respect, but I am sure that the more he investigates them, the less he will be inclined to demur to them. I shall leave to the ornithologists of New Zealand the defence of those that relate to their cuckoos. Dr. Weismann will find in Mr. Jones's "Naturalist in Bermuda" (London, 1859) more than enough to justify my allegations in regard to the passage of *Charadrius virginicus* (not *pluvialis*) over those islands; indeed it has long been notorious; and as to the plovers of the Sandwich group, I have not only to thank Capt. Long, R.N., for his confirmation (*suprà*, p. 460) of my statements, but also Prof. George Forbes, who kindly informs me that when there, on the occasion of the transit of Venus, he shot scores of these birds, and that his friend Capt. Cator, R.N., of H.M.S. *Scout*, having sailed thence, was overtaken in mid-ocean by them, flying in a direct line for Vancouver's Island, on arriving at which he found they had already reached it. Concerning the "facts" relating to some young birds preceding their parents in migration, the more inquiries I make of well-placed observers the more satisfactory are the answers. For want of space I cannot here give the details, but I may just say that Mr. Cordeaux, who has been for many years a watchful observer of migratory birds on the Lincolnshire coast, has named to me nine species of *Limicolæ*, of which he has personally assured himself that the young migrate apart from, and invariably arrive earlier than, the old—thus fully bearing out Temminck's assertion, made nearly forty years ago. The case of our cuckoos, which I cited, is incontestable, and M. Gätke, I doubt not, will satisfy any scruples about his starlings in that book which we are expecting from his hands.

I will also take this opportunity of replying to Mr. Pringle's note (*suprà*, p. 481). My chief reason for not referring to the matter of temperature was that we know too little of the power of birds to resist extreme cold to depend much upon it, and I thought I would not take up room by bringing in that question. Doubtless there is something in what he says touching the loom of land, but I fail to see how it will help very far, and especially in nocturnal flights. ALFRED NEWTON

Magdalene College, Cambridge, April 20

Colour in Nature

I WISH to offer a few remarks upon Mr. Wallace's kind and appreciative review of my work on the "Colour-Sense" in NATURE, vol. xix. p. 501. Mr. Wallace attributes to me "many errors" and inaccuracy as to matters of fact; but I do not think the instances he alleges are sufficient to justify the statement. Had I said in every case what Mr. Wallace makes me say, I should, doubtless, have been misrepresenting facts; but it seems to me that in most of the passages to which he refers he has slightly misconceived my meaning. I should not attempt to oppose so distinguished a naturalist on points of biological inference, but I venture to defend the accuracy of my statements of fact.

1. "*Scissirostrum Pagei* does not 'belong to a family generally dull,' while it is itself decidedly dull-coloured." The first statement will be correct if we place *Scissirostrum* among the brilliant starlings; but Mr. Wallace himself, following Prince

Lucien Bonaparte, puts it next to the West African *Buphagæ*. Now the *Buphagæ* are certainly dull birds, while *Scissirostrum* is described in the "Malay Archipelago" as "almost entirely of a slaty colour, with yellow bill and feet, but the feathers of the rump and upper tail-coverts each terminate in a rigid glossy pencil or tuft of a vivid crimson" (i. 430). I wrote with this passage of Mr. Wallace's under my eyes, and refer in a footnote to his volume for the vivid crimson. I did not say the bird was brilliant, I merely noticed the colour of its tail and beak. The case really stands thus: If *Scissirostrum* was differentiated from a generic ancestor generally resembling *Buphaga*, we have to inquire, why did it develop these ornamental adjuncts? and my answer is, because while *Buphaga* pecks the parasites of the backs of mammals, *Scissirostrum* feeds off "grains and fruits."

2. "Santarem, of which it is said 'the pastures are destitute of flowers, and also of animal life, with the exception of a few small plain-coloured birds,' is one of the richest localities for flowering shrubs in South America." Now, this passage to which Mr. Wallace takes exception is not mine, but is a textual quotation from Mr. Bates ("Naturalist on the Amazons," p. 183). It is given in inverted commas in my text, with reference to the original in a footnote. I was, of course, aware that the Brazilian woods generally were full of brilliant birds, and that "the butterflies in the adjacent forests were gorgeous in the extreme." What I wished to point out was that in particular spots like these meadows, where the general aspect of the flora was not bright, the purely local fauna was likewise dull. We may find great varieties in this respect nearer home in a meadow, an adjacent warren, and a moor or swamp behind it. Moreover, the passage was professedly quoted, simply as showing the general impression left upon my mind by reading various books of travel. May I add a sentence from a private letter of Mr. Darwin's, which helps out the same view on a larger scale? "The contrast," he says, "in the colour of the birds in Patagonia" (where he had just noticed "the sombre aspect of nature"), "and on the bright green flower-decked plains of La Plata is very striking."

3. About a certain squirrel, described in the "Malay Archipelago" as having a tail "ringed with gray, yellow, and brown," and as looking "exceedingly pretty," Mr. Wallace now says it "is one of the dullest of the group," while he did not "say a word about its feeding on 'bright-coloured fruits.'" But he *did* say that it would eat "any fruit" (i. 192), and I presume, therefore, that it sometimes eats "bright-coloured food."

4. "So far from the colours of caterpillars being 'mostly protective,' every entomologist knows that a large number of caterpillars in every part of the world are conspicuously coloured." True; but Mr. Wallace himself was the first to suggest that these conspicuous colours were themselves protective by giving warning of inedibility; and I am at a loss to understand what he means by thus going back upon his own words. I took my statement from Sir John Lubbock's lecture "On Certain Relations between Plants and Insects," pp. 23–24, where this fact of universal protective colouring in larvæ is very clearly brought out.

5. "Again, the ground-feeding pheasant family are passed over as containing only one brilliant bird, the peacock, whereas it abounds in species of the most gorgeous colour." But my words are very different from this—"Even among the pheasants themselves," I say on p. 176, "many species are far from brilliant; and when we come to compare the whole family with that of the parrots or the humming-birds, we shall find that the peacock alone can fairly come into competition with the typical fruit-eaters and flower-feeders." Mr. Wallace goes on to mention (amongst others) the "Impeyan pheasant of the Himalayas," and "the intensely-brilliant fire-backed pheasants of the Malay countries," as among the most brightly-coloured species. Any one would suppose from his review that I had totally overlooked these cases; but in the very same paragraph with the sentence which Mr. Wallace blames the following passage occurs:—"The forests of the Himalayas and the Malay Archipelago, with their great brilliant fruits and flowers, and their exquisite insects, form the haunts of the most beautiful species of pheasants" (p. 177). As a matter of fact, before writing that paragraph I had carefully compared all the living *phasianidæ* in the Zoological Gardens, and all the preserved specimens in the British and Oxford Museums; and I feel sure that any one who does the same will agree with me that the peacock alone can be placed in the very first rank of brilliant colouration.

6. How much the subjective element enters into these questions may be seen from the following remark of Mr. Wallace:—"The tigers, the zebras, the beautifully-marked antelopes, and the spotted deer and giraffes, *which are really among the most brightly-coloured of all mammals*, are passed over as less beautifully coloured than the squirrels and monkeys." Now I confess myself simply astounded at the statement that the zebra, of all animals in the world, is brightly coloured—a creature without a tinge of anything but creamy white and black about its body. Quite apart from the nature of food or surroundings, I call a panda a brightly-coloured mammal; or a mandrill; or a Rhesus monkey; or a Canadian chipmonk; but certainly not a tiger, a zebra, or a giraffe, none of which has a single tinge of scarlet, blue, green, or bright yellow.

No one who knows anything of Mr. Wallace could for one moment imagine him capable of intentionally misrepresenting the humblest opponent in the smallest particular; and I owe him many thanks for much kind and appreciative criticism both on this and several previous occasions. Yet I cannot help thinking that in these instances, and others with which I will not burden your space, he has unconsciously permitted mere differences of opinion unduly to assume the appearance of positive errors in fact. GRANT ALLEN

Remarks by the Reviewer

1. *Scissirostrum Pagei* is universally placed in the starling family. Its affinity to *Buphaga* is very doubtful, while its *crimson-tipped tail-coverts* are very different from "*a tail of vivid crimson*" which Mr. Allen gives it (p. 184).

2. I object altogether to founding theories on chance expressions of travellers. It is curious, that in my "Travels on the Amazon" (p. 157) I refer to these same Santarem pastures as follows:—"There were some boggy meadows here, more like those of Europe than one often sees so near the equator, on which were growing pretty, small *Melastomas* and other flowers. The paths and campos were covered with flowering myrtles, tall *Melastomas*, and numbers of passion-flowers, convolvuluses, and bignonias." These open meadows and campos really exhibited more conspicuous flowers than the woods and forests which swarmed with brilliant butterflies and birds.

3. I referred to the squirrel, because it was the only example given by Mr. Allen which I could at the moment test.

4. My argument is, that the colours of caterpillars are often as varied, as vivid, and as beautifully arranged as in birds and winged insects. This is not necessary for protection by *conspicuousness*, for which purpose any tint contrasted with foliage, such as black, or white, or ringed with black-and-white, would have sufficed.

5. The "pheasant" question I leave, as Mr. Allen has placed it, for the consideration of naturalists.

6. Here it seems to me Mr. Allen is himself changing his ground. His main argument is that the æsthetic tastes of the higher animals are the same as ours, yet he objects to the elegantly-marked and intensely-contrasted zebra and tiger being called "brightly-coloured." Surely they are more beautiful than the mandrill or the Rhesus; while among animals *white* is as much a colour as among flowers. ALFRED R. WALLACE

Nitric Acid Batteries

I INCLOSE the results of some experiments I have lately made to ascertain if the cost of working the nitric acid batteries of Grove and Bunsen could be reduced. I find that the nitric acid can be replaced by a mixture of half nitric and half dilute sulphuric. And the latter gives a higher force for nearly three hours. The experiments were made with a large-surface voltameter, and the gases were collected during one minute every half-hour; four pint-size cells were used. The experiments were repeated, and every care taken to avoid any error. I have also used the mixed acids very successfully with twenty-eight cells for the electric light. I presume the increased power is due to the internal resistance of the battery being slightly lowered by the addition of the dilute sulphuric acid in the porous cell. I may add that the fumes were much less than when nitric acid alone is used. JOHN HENRY KNIGHT

Farnham, April 19

The Black Rat

IN regard to the distribubution of the black rat (*Mus rattus*), your correspondent may be glad to know that this animal, spread

over the States in early days, but has since been supplanted, as in England, by the brown rat (*M. decumanus*). Forty years ago the black rat was the only rat in South-west Ohio. About thirty years ago the brown rat drove him out. Some years later the same occurred in Illinois. I have been informed by one of my students living in Minnesota, that neither rat is known in and about the town of St. Cloud in that state, only one having ever been seen there, and that was killed on landing from a steamer. I have seen it stated that the black rat is still to be found in some localities in England, among them the Whitechapel Docks. E. W. CLAYPOLE

Antioch College, Ohio, April 7

Did Flowers Exist during the Carboniferous Epoch?

I CANNOT accept Mr. McLachlan's reference of the interesting *Breyeria borinensis* to the Ephemeridæ, even though he has "examined the fossil," and "has no doubt" about it. The photograph which I possess is so beautifully sharp that it brings out the minutest details, and a careful examination and comparison of it with specimens and drawings leads me to the conclusion, that in the general character of the wing-neuration it is strictly lepidopterous and of the Bombycine type, having the costal, subcostal, and median nervures, with their branches and bifurcations, arranged precisely as in that group, but differing in the much greater length of the wing and the increased number of the branches of the subcostal vein—seven instead of four. In some of the Chalcosiidæ, however, there are often six branches to this vein, but crowded together and sometimes anastomosing, owing to the much shorter apical portion of the wing. In this family also we often have an intermediate false vein, which is distinctly visible in the fossil. Until, therefore, I am referred to some group of insects with which it more nearly agrees, I must believe it to be an ancestral moth, even though, according to Prof. Haeckel and Mr. Scudder, moths ought not to have existed in the carboniferous epoch.

After a careful comparison of the photograph with specimens and figures of Ephemeridæ, I can see *no resemblance whatever* to the neuration of the family with which Mr. McLachlan so confidently associates it; while the "dense transverse reticulation" to which he refers seems to me to be merely due to crumpling of the membrane, and certainly bears no close resemblance to the strong reticulation of the veining of the Ephemeridæ, and it is, moreover, only visible at all at the base of the wing. The general form of the wing and arrangement of the veins are, however, so different, as, to me, to be conclusive against this view. ALFRED R. WALLACE

Blue Flame from Common Salt

At the present time any spectroscopic observations of coloured flames are peculiarly interesting, and I am glad to see the origin of the blue or violet flame produced by common salt and other chlorides again discussed in your pages.

In the letter of Mr. Percy Smith (NATURE, vol. xix. p. 483), he considers the only feasible explanation to be "that it is due simply to hydrochloric acid," but he gives no proof, and admits that a spark between carbon points in a bottle of this gas does not give the violet bands. In a short paper on the subject in the *Philosophical Magazine* of December, 1862, I considered "this supposition is negatived by the fact that anhydrous chloride of copper emits these rays equally whether it be placed in a flame of hydrogen or of pure bisulphide of carbon." Neither does this characteristic flame seem due to any carbon compound, inasmuch as several chlorides will give it in a hydrogen flame. I also found that "a stream of chlorine or hydrochloric acid passed into a flame never gives the violet light, nor does Dutch liquid, muriatic ether, or chloroform mixed with alcohol and burnt in a spirit lamp," though chloride of platinum or gold give a flash of it at that temperature.

Would Mr. Smith favour us with any details of his experiments which may support his conclusion?

17, Pembridge Square, April 10 J. H. GLADSTONE

Cape Diamonds

AT the Croydon County Court a lady sought to recover 36*l*. 15*s*. paid for a ring, the stone in which had been represented to be *a diamond*, and which was indeed admitted to be a Cape diamond.

Judgment was given for the plaintiff, because several diamond *dealers* gave evidence which, the judge stated, clearly showed that what were described as "Cape diamonds" were not at all *to be regarded as* ordinary diamonds, and the receipt showed that the ring was sold *as a diamond* ring. The "several diamond dealers" stated that so-called Cape diamonds were comparatively valueless and lacked the essential qualities of the Brazilian stones, viz., lustre, hardness, and colour.

Now all this is beside the question, which was not as to the value of Cape diamonds, nor yet what they lacked of the qualities of the Brazilian stone, but simply whether this stone was a diamond or not, not even whether it was or was not an ordinary diamond, and I am surprised that any judge could be thus led away from the legal point.

I see that notice of appeal has been given, and it is to be hoped for the credit of elementary science that the court above will require some *scientific* evidence, such as specific gravity or chemical composition, about Cape diamonds. If, for instance, it can be shown that they are a form of carbon, the point is settled.

It would be just as absurd for a person to object to Derby coal as not coal because it lacked the good qualities of Wallsend. The ring was sold as a diamond ring; the question is: Is the stone a diamond?

I have no personal interest whatever in the matter. I know nothing of the case except as it appears in the report. I possess no diamonds, not even a "Cape"; but I am interested in seeing justice administered with some regard to the scientific knowledge of the day. B. G. JENKINS

April 14

Sense of Temperature

YOUR correspondent J. T. B. asks for further instances of the cultivation of the sense of temperature. None can be more striking than that of the caste of egg-hatchers in Egypt, who determine the temperature in their ovens entirely without the aid of instruments, and maintain it at 100° to 103° Fahr. during the requisite three weeks. How successful they are is shown by the official return for 1831, given by Lane ("Modern Egyptians," London, 1842, vol. 2, p. 5, *et seq.*) from whom I take these particulars. Out of a total of 26,204,500 eggs artificially incubated, 17,418,973 were successfully hatched.

April 19 ALFRED H. HUTH

Tides at Chepstow

THE highest tides in the Wye and in the Severn for the present year were on Tuesday, April 8. On that day, up the Wye, at Llandogo, the tidal rise was 13 feet; at Tintern Abbey, 21 feet 5 inches; at Chepstow Railway Bridge, 44 feet. Up the Severn, at Newnham, the tidal rise was 20 feet; at Portskewitt, 46 feet 6 inches; at Cardiff, 44 feet; at Clevedon Pier, 52 feet.

Reference to Coxe's "Historical Tour in Monmouthshire," 4to Edition, 1801, p. 358, containing his own *soundings* at high tide, on September 4, proves that there has been no perceptible change in the depth of the Wye at high tide this century.

The Severn has been confined within narrower limits by the South Wales Railway embankment, on the Monmouthshire side, since 1850, and by Lord Fitzhardinge's breast-works on the Gloucestershire side, from about same date, but the height of the tide and the depth of the river have not been sensibly affected by these slight alterations. One fact further may be worth mention, however: a gun-boat or armed sloop, commanded by Capt. White, came up the "Pill," below St. Pierre, in 1827, on a surveying expedition, remained at anchor some days, and re-entered the Severn without difficulty, piloted by W. Wheeler, a thing that would now be impossible on account of the embankments. The Pill is a mere creek—the "anchorage," dry ground. JOHN YEATS

OUR ASTRONOMICAL COLUMN

BRORSEN'S COMET.—The following ephemeris of this comet for May is deduced from Dr. Schulze's elements, with the time of perihelion passage corrected so as to accord better with the observations in March at Florence and Kremsmunster. The heliocentric co-ordinates, referred to apparent equinox of May 1, for combination

with the X, Y, Z of the *Nautical Almanac*, have been found from—

$$x = r[9{\cdot}94281], \text{ sin. } (v + 207^\circ\ 51'{\cdot}8),$$
$$y = r[9{\cdot}98498], \text{ sin. } (v + 126\ 18{\cdot}6),$$
$$z = r[9{\cdot}73737], \text{ sin. } (v + 60\ 32{\cdot}2).$$

Ephemeris for Greenwich Midnight

1879.	Right Ascension. h. m. s.	North Declination. ° ′	Log. distance from Earth.	Log. distance from Sun.
May 1	5 45 30	59 42·0		
2	5 57 36	60 38·4	9·8459	9·9366
3	6 10 31	61 30·5		
4	6 24 10	62 17·8	9·8422	9·9494
5	6 38 31	63 0·1		
6	6 53 37	63 36·4	9·8395	9·9621
7	7 9 19	64 6·7		
8	7 25 30	64 30·5	9·8381	9·9745
9	7 42 5	64 47·3		
10	7 58 55	64 57·1	9·8379	9·9866
11	8 15 50	64 59·5		
12	8 32 41	64 54·9	9·8388	9·9984
13	8 49 16	64 43·3		
14	9 5 28	64 24·9	9·8409	0·0100
15	9 21 11	64 0·2		
16	9 36 19	63 29·6	9·8441	0·0213
17	9 50 46	62 53·5		
18	10 4 30	62 12·4	9·8484	0·0323
19	10 17 28	61 27·0		
20	10 29 39	60 37·8	9·8538	0·0430
21	10 41 5	59 45·2		
22	10 51 49	58 49·7	9·8602	0·0535
23	11 1 52	57 51·9		
24	11 11 16	56 52·1	9·8675	0·0637
25	11 20 5	55 50·6		
26	11 28 20	54 48·1	9·8757	0·0736
27	11 36 4	53 44·9		
28	11 43 18	52 40·6	9·8848	0·0833
29	11 50 5	51 36·2		
30	11 56 28	50 31·8	9·8942	0·0927
31	12 2 28	49 27·6		

THE DOUBLE-STAR, SOUTH 190.—Interest attaches to this object for more than one reason. The principal star possesses a large proper motion in which the companion participates, while there is a much slower change of relative position in the same way that we observe in 61 Cygni. Further, there would appear to be some evidence of variability of light in the principal star. Argelander in his memoir on the proper motions of 250 stars, assigns +0·0691s. in right ascension, and – 1″·766 in declination, or 2″·015 annually in arc of great circle, in the direction 151° 14′. If we compare Lalande's observation on May 22, 1798, with the observations made at Bonn in 1864, and at Washington 1867–69, almost identical values with those given by Argelander will result. The following figures will sufficiently indicate the variation in relative position that has occurred since Piazzi observed the star early in he present century:—

Piazzi	1806·7	Position	251°·4	Distance	9′·4c
Herschel and South	1823·32	,,	270·1	,,	10·82
Herschel (Cape Obs.).	1836·46	,,	277·4	,,	12·08
Jacob	1856·37	,,	284·0	,,	13·35
Stone (Cincinnati)	1877·37	,,	290·3	,,	14·92

The star forms one of Sir W. Herschel's catalogue of 145 new double stars, where the duplicity is stated to have been discovered in 1785; at the epoch 1791·39 the angle was estimated 270° —, distance IV.; an observation not easily reconciled with more recent ones.

As regards variability the principal star was rated 4m. in Argelander's zone No. 295, on May 20, 1850; it is 5·9m. in the second Radcliffe catalogue, while the Washington observers call it 6·6m.; Lalande and Piazzi estimated it 6m. Argelander calls the companion 8·4. The position of South 190 for 1880 is in R.A. 14h. 50m. 27s., N.P.D. 110° 52′·3. It is No. 1186 in the Greenwich catalogue for 1860.

THE MINOR PLANET HILDA.—This small planet, the most distant member of the group, which approaches the orbit of Jupiter within 0·85 of the earth's mean distance from the sun, has been sought for unsuccessfully at Berlin, near the calculated position; there may now probably be a difficulty in recovering it.

GEOGRAPHICAL NOTES

WE hear that Sir Walter C. Trevelyan, who died lately at Wallington, Northumberland, has bequeathed to the Royal Geographical Society, of which he had been for many years a trustee in conjunction with Lord Houghton, the sum of 500*l.*, in addition to a valuable collection of books relating to the Faroe Islands, maps, &c.

THE geographical haze in which some of our daily contemporaries persist in enveloping themselves, appears to be growing denser. The "War at the *Cape*" is bad enough, but the telegram received last week from a special correspondent at Baku, informing a wondering public that "Krasnovodsk has returned with General Lazareff, and Lomakine's reconnaissance to the confluence of the Attrek and Sumbir [*sic*], &c.," fills the cup to overflowing. Krasnovodsk, we thought, was the name of a town and bay on the eastern shore of the Caspian, but the tangle is above our powers to unravel.

THE new part of the *Transactions* of the Asiatic Society of Japan is wholly occupied with Mr. John Milne's narrative of his journey across Europe and Asia to the Land of the Rising Sun. From some singular statements which he makes, we suspect that Mr. Milne was not sufficiently careful in making himself acquainted with the literature of Chinese travel before leaving; otherwise he would hardly venture to assert that the journey from Peking to Tientsin and overland to Shanghai has but seldom been made by Europeans. Mr. Milne's views on the subject of the rendering Chinese sounds are very remarkable.

THE Paris Society of Geography held its annual meeting for the election of officials on Friday, April 18. Admiral Laroncière le Nourry was returned president almost without opposition. The great gold medallist is Lieut. de Brazza, the Ogowé explorer. A gold medal was also awarded to Lieut. Wyse, of the French Navy, for his exploration of the Isthmus of Darien, for the construction of an inter-oceanic canal. The gold medal for Polar exploration was awarded to Sir George Nares, Commander of the last English Arctic Expedition. The Cross of the Legion of Honour was also given to M. Brazza and his fellow-explorer, Dr. Ballay. Lieut. Wyse and Lieut. Reeks received a similar honour for the Darien explorations. An address was given by Commander Perrier on the determination of longitudes by electricity. A map was distributed amongst members showing all the European and African towns whose longitudes have been determined by that process. They number about one hundred, extending from Oural to Valentia, and from Lapland to Sahara.

NO. 3 of this year's *Mittheilungen* of the Vienna Geographical Society contains an important paper, with map, on the sources of the Dniester and the valley-structure of the region of the Upper Dniester and Strwcaz. The first number of this year's *Boletin* of the Madrid Geographical Society contains, among other things, the first part of an account of an excursion in the La Plata Republics, by Capt. Carrasco y Guisasola.

THE just published *Bulletin* of the Antwerp Geographical Society contains, amidst a considerable variety of matter, a paper by Mme. Dumas de Baiglie, entitled "Les Voyageuses illustres." The Society about a year ago resolved to admit ladies, and the author of this paper is a *membre associé*, who seems very grateful for this recognition of the rights of women.

AMONG the new bills introduced into the first session of the Forty-sixth U.S. Congress is one authorising the president to establish a temporary colony at some point north

of the eighty-first degree of north latitude, on or near the shore of Lady Franklin Bay, for the purpose of scientific observation and exploration, and to develop or discover new whaling grounds; such officers as may be necessary to be detailed to take part in the same, and with permission to use any public vessel or vessels in connection therewith. This is essentially Capt. Howgate's plan, and probably introduced by his request.

THE last number of the *Indian Antiquary* contains a note by Major J. S. F. Mackenzie on some curious customs current among the Komti caste in regard to marriage, &c. "A Folklore Parallel," by Prof. C. H. Tawney, of Calcutta, is also worthy of notice.

MGR. LAVIGERIE, Archbishop of Algiers, communicates to *Les Missions Catholiques* intelligence respecting the portion of the French missionary expedition in East Africa, which, under the leadership of Père Livinhac, was gradually making its way towards Lake Victoria. At the date of the letter (December) the five Europeans were all in good health, and were then in Mirambo's country, on the way to Uganda. Père Livinhac writes that they had been three months in Unyanyembe, and that they were then twenty or thirty days' march from the lake. In the same number of *Les Missions Catholiques* Mgr. Ridel continues the account of his recent captivity in Corea, in which he gives a terrible picture of the prisons of the country.

A TELEGRAM from Malmö states that the steamer *Nordenskjöld*, built for M. Sibiriakoff, to go to the assistance of Prof. Nordenskjöld's expedition, was launched on the 17th inst.

A VERY interesting narrative of travel has just been commenced in the *Tour du Monde*, entitled "Voyage en Nouvelle Guinée," by M. Achille Raffray. The first instalment deals with the Moluccas, which M. Raffray visited *en route*, but in the second he commences his work in New Guinea. The illustrations are unusually good.

BIOLOGICAL NOTES

THE EARLY TYPES OF INSECTS.—Samuel H. Scudder has published a memoir on the early types of insects (*Memoirs* of the Boston Society of Natural History, vol. iii. Part i. No. 11, March, 1879). He concludes that the hexapods, arachnids, and myriapods appeared together in the carboniferous strata. That the hexapod insects may be divided into a higher group (Metabola), and a lower group (Heterometabola), that the latter are Devonian and carboniferous, the former just appearing in the Jurassic period. The Devonian forms were in the early stages of their life, undoubtedly aquatic. Nearly all the palæozoic orthoptera belong to the lower Saltatorial families. It would seem that the earlier types were of inferior organisation, and that the general type of wing structure in insects has remained unaltered from the earliest times.

HALOSPHÆRA, A NEW GENUS OF UNICELLULAR ALGÆ.—Under this name Dr. F. Schmitz describes, in the first "Heft" of the first volume of the *Mittheilungen aus der zoologischen Station zu Neapel*, an organism which is found abundantly between the middle of January and the middle of April, floating on the surface of the water in the Bay of Naples. Hitherto known to collectors simply as *punti verdi*, Dr. Schmitz gives it the name *Halosphæra viridis*. It presents to the naked eye the appearance of minute just visible pale green globules, the largest having a diameter of from 0·5 to 0·6 mm., but with no independent power of motion like that of *Volvox*. Each globule consists of a tolerably thick perfectly smooth and colourless cell-wall, coated on the inside with a thin layer of pale green protoplasm, which incloses a single very large central vacuole filled with a colourless cell-sap. The green colour of the protoplasm is due to its being interspersed with a small number of minute grains of chlorophyll; and there is also, at an early stage, a single globular nucleus with a somewhat darker nucleolus. As the cell increases somewhat slowly in size, the process of cell-division commences. The single nucleus divides into two nuclei, which gradually separate from one another; and this process is repeated time after time, until a very large number of nuclei, which the author reckons to average from 200 to 300, come to be tolerably regularly distributed through the parietal protoplasm of the mother-cell, which has by this time attained its full size. The layer of protoplasm then breaks up into a number of primordial daughter-cells, each surrounding one of the nuclei, and having the form of a hemispherical ball, the flat surface of which is in contact with the cell-wall of the mother-cell. They are of a uniform bright green colour, without apparently containing any distinct grains of chlorophyll. The external cell-wall of the mother-cell has now become differentiated into two distinct layers, the outer one of which bursts into two nearly equal halves, and becomes completely detached from the inner one, which now itself consists distinctly of two layers. The hemispherical green daughter-cells then become transformed into zoospores of a very peculiar shape. They begin gradually to detach themselves from the outer cell-wall, and to take up positions in the interior of the cell. In most cases each of them contracts in the centre into somewhat the shape of an hour-glass, but pointed at the two ends, ultimately dividing in the middle into two zoospores of conical shape, with a nearly flat base, but toothed at the edges, and a pointed apex. To a colourless protuberance in the centre of the nearly flat base are attached two very long vibratile cilia. Sometimes only a single zoospore is formed from each of the primordial cells, and occasionally more than two. The remaining cell-wall of the mother-cell has, in the meantime, been gradually swelling up and deliquescing, and has now become completely converted into mucilage, so that the zoospores escape free into the surrounding water. After moving about for some time with a rather slow swarming motion, they fall to the bottom; but their further development has not been followed up. Until its complete life-history is known, it is impossible to assign a systematic position to *Halosphæra*. It may possibly come near *Eremosphæra*, a genus of Conjugatæ; its resemblance to *Volvox* is clearly only superficial.

A NEW ALGA.—In the first Heft of the 1st vol. of the *Mittheilungen aus der zool. Station zu Neapel*, Dr. Falkenberg describes a new genus of Phæosporeæ under the name *Discosporangium*, with the following characters:—Thallus, an irregularly branched filament, consisting of a single row of cells, and growing by an apical cell. Reproduction by zoospores, which are formed singly in the compartments of multilocular zoosporangia. The zoosporangia are placed singly near the middle of the cells of the thallus, forming a square unilamellar plate, the compartments of which open separately when ripe on the upper side of the sporangium.

IN the second Heft of the same publication Dr. Falkenberg gives a complete list of the marine Algæ of the Bay of Naples.

MARINE FLOWERING PLANTS.—Dr. I. B. Balfour has just published (*Transactions* Bot. Soc. Edinburgh, Session 1877-78) a most valuable and interesting memoir on two species of the genus Halophila, found very abundantly in widely extended patches on the reefs surrounding the island of Rodriguez. The island was visited in 1874 by Dr. I. Balfour as naturalist accompanying the "Transit of Venus" expedition. Of the two species one, *H. ovalis*, grows on spots which are just uncovered at low tides. The other, *H. stipulacea*, grows in places where it is always submerged. Specimens

collected both in flower and fruit were preserved in alcohol, and were most painstakingly investigated at Prof. de Bary's botanical laboratory at Strassburg. The only portion of the life-history of these plants left for future investigators is the germination of their seeds, which, probably, does not take place until the first quarter of the year. The stem structure is simple. Of the presence of sieve-tubes in the bundles there appears to be no doubt. The mode of the tissue formation at the tips of the roots is peculiar; from an initial group of cells underneath the root-cap, there issues three distinct tissues. This corresponds to the third type of Janczewski, who, among the monocotyledons, found it only in Elodea. The scale and foliage leaves are described in detail. The epidermal layer is peculiar; stomates are to be found in neither of the species. The floral axis is short and axillary; there is a double-leaved spathe. The author is inclined with Ascherson to consider the plants diœcious. The anther cavities are filled with a mass of confervoid pollen. These pollen cells are found to be united in long strings, each string apparently continuous through the greater part of the length of the cylinder. The partition walls between adjacent cells in a string are transverse. The ovary is inferior and contains many ovules. The author suggests the morphological identity of the stamens and carpels, "the same phyllomes (or the phyllomes from the same nodal regions), which, in the male form stamens, in the female form carpels." A technical and emended character to the genus and of the two species concludes this paper.

AMERICAN APHIDES.—Dr. Riley gives a detailed account of the life-history of some species of gall-making Pemphiginæ (Art. 1, vol. v. *Bulletin* of the United States Geological and Geographical Survey of the Territories, 1879). The facts concerning these Aphides have a special interest on account of the close relationship between the insects of this group and the now notorious grape vine Phylloxera. The special history of *Schizoneura americana*, n. sp., is given. It is to be found curling and gnarling the leaves of the White Elm (*Ulmus americana*), and passes from the egg state through no less than seven stages, in some winged, in some wingless, but in all agamic until the seventh, when, as the result of fertilisation, the true egg state is again reached. Another very common gall described is that formed by *Colopha ulmicola*, and the diagnoses of five new species of Pemphigus are given. In a second part of this paper Mr. Monell describes several new species, and gives detailed synonymy of several already described. Two excellent plates accompany Dr. Riley's notes on the gall-making forms.

NEW BIRDS FROM THE PORTUGUESE POSSESSIONS IN WESTERN AFRICA.—Prof. Barboza du Bocage publishes (*Journ. de Scien. Math. Phys. Natur.*, Nos. xxiii. and xxiv., Lisboa, 1878) his sixteenth and seventeenth lists of birds from Angola. A new genus and species (*Hylypsornis salvadori*) is established for a creeper, and a pretty sun-bird is called after M. d'Anchieta, who has added so much to our knowledge of the birds of Angola (*Nectarinia anchietæ*). Several other new species are described in the sixteenth list. In the seventeenth list a new genus and many additional new species are also established, the more remarkable being a sun-bird (*Nectarinia oustaleti*) and a unique bird from Caconda (*Sharpia angolensis*), called after Mr. Sharpe, of the British Museum, and having affinities with Hyphantornis.

A UNIVERSAL CATALOGUE

THE Council of the Society of Arts, probably the most practically useful body in the kingdom, has taken a positive step towards the accomplishment of a task which certainly deserves to be called gigantic. We need not moralise once more on the extent to which the making of books has been carried; many a modern Solomon has no doubt been appalled into silence in the effort even to realise, far less to express, the extent of this manufacture. To attempt to begin *ab initio* to catalogue the works published during the past century, or even since the beginning of the present century, would be a task which to us would seem to be hopelessly endless. Any one whose business it is to work with books, and even the most thorough-going scientific worker must refer to them occasionally, must recognise the immense advantage, however, of having in one properly arranged catalogue, as complete a list as possible of printed books, and the farther back it went, the more valuable it would be. It is, then, certainly a fortunate thing that there exists ready to hand, though unprinted, a catalogue which for all practical purposes may be regarded as a universal catalogue of printed books, and that not only for the past century, but the past four centuries and more; for the British Museum Catalogue begins as far back as 1450. Some time ago the Society of Arts considered the advisability and practicability of constructing a catalogue coming down to the year 1600. The Council addressed a series of questions to them likely to give useful answers, and afterwards met to hear evidence on the subject. Mr. Bullen and other authorities were thus examined, and it seems to have been Mr. Bullen who happily suggested that the best and only sure method of laying a solid foundation for the Universal Catalogue of English printed literature would be to print the Catalogue of the Printed Books in the British Museum, from A.D. 1450 to the present time, say, the end of the year 1878, representing about 1,250,000 vols., and comprising between 2,000,000 and 3,000,000 entries, *i.e.*, main titles and cross references. He considered the work might be ready for printing, "in a rough and ready way," in two years, and in less time if more force were employed, and that it would take five years to print. All the witnesses agreed that the printing of the British Museum Catalogue would be highly desirable, and the Committee are of the same opinion.

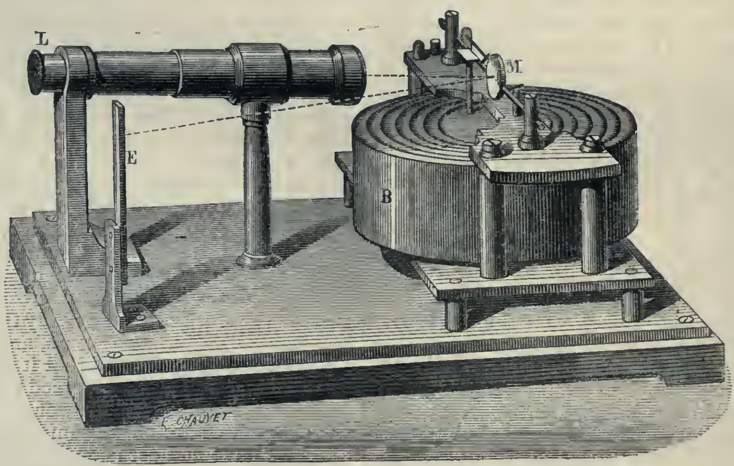
As we have had occasion to point out in these pages, the British Museum Catalogue is by no means perfect, and it is specially difficult for a man in search of a scientific serial or paper to get at it without much roundabout hunting from one cross reference to another, much waste of time, and loss of temper. Still considering all the difficulties in the way of constructing a perfectly new catalogue, we do not think a better course could be followed than that suggested by the Society of Arts' Council. It might be possible to introduce some improvement in arrangement during the process of printing, and especially with reference to the arrangement of the publications of scientific societies, which at present is so completely unscientific. It must also be borne in mind that the Catalogue is only one of authors, and that for many purposes of research such a catalogue is of little use without an equally complete one of subjects. Still the want of the latter is no argument against the publication of the former, though we should hope that the one would be followed by the other.

Of course such a stupendous, and, at its cheapest, costly undertaking could hardly be accomplished by any private body, and it is natural that the Society of Arts should look to government for help in the matter. As the scheme has the approval of the President of the Society, the Prince of Wales, we should think that the Government is not likely to hesitate in granting such aid as might be required. Of course the printing and paper need not be luxurious nor expensive, and the specimen-page issued by the Society seems to us satisfactory. It is calculated that the British Museum Catalogue would thus occupy about forty-five volumes of 1,000 pages each, and could be issued through the Stationery Office at about 16*s*. per vol., and even less if the edition were of 2,000 copies. No doubt a fair sale would be obtained for such a publica-

tion both at home and abroad, for take it all in all, as Mr. Bullen says, "no catalogue in the world, whether in print or in manuscript, is equal to that of the British Museum." We hope, therefore, that the proposal of the Society will speedily meet with a favourable response from Government, and that should it be decided to print the British Museum Catalogue, some plan will be formed by which proofs may be revised not only by qualified bibliographers, but that the various departments of literature, science, and art will be represented on the staff of revisers.

A MIRROR BAROMETER

M. LÉON TEISSERENC DE BORT has invented an aneroid mirror barometer, which is described in a recent number of *La Nature*. It is based on a method analogous to that well-known since the researches of Gauss for the reading of small rotations. M. Teisserenc de Bort has sought to obtain an aneroid barometer which will give precise observations at sea, especially in rough weather, when it is impossible to read the mercury barometer. The principle of this barometer is very simple. The elastic tub or box B carries, as in most aneroids, a metallic point, which follows its movements. In the ordinary aneroid the transformation of the vertical movement into a rotating movement necessitates either a chain or a curb, or a sort of fork which works in a spiral furrow cut in the axis which supports the needle. These various systems have the inconvenience of producing frictions; some of them are liable to dust and rust. In the mirror barometer, the transformation of the movement is obtained by the simple contact of a small palette supported on the axis of the mirror and of the point spoken of above. As the angle which the plane of the mirror may describe does not exceed 12° on each side of the vertical, it follows that the contact of the point in the palette is always precise.



Teisserenc de Bort's mirror barometer.

As to the amplification of the movements necessary to enable us to appreciate millimetres and their fractions, this is obtained by reading with the aid of a small reticled telescope, L, the image of a graduated scale E which is reflected in the mirror M. By combining the enlargement of the telescope with the distance of the scale from the mirror, we succeed in giving to the apparatus a length of less than 20 cm. by 12, which renders it quite portable. It is important to remark that the amplification of the movements of the box, which, in ordinary barometers, is obtained by means of several levers, is obtained here by an optical process; it follows that the numerous frictions and the time lost in contacts are mostly eliminated. There remains only a single movement, that of the axis which bears the mirror; in the barometer figured the pivots are of steel and the cap of platinum, and in order to avoid rust, the whole is nickel-plated.

M. Teisserenc de Bort proposes to construct others, in which the axis will be mounted on rubies. This garniture will not sensibly increase the price of the apparatus. This instrument is too new to allow us to appreciate the full degree of precision which it can attain. In a trial in a captive balloon by Capt. Perrier of several aneroids as compared with the mirror, the latter showed a great sensibility, and it quickly resumed its original position on landing.

BUTTERFLIES WITH DISSIMILAR SEXES

NATURALISTS have long been familiar with the fact that the two sexes of certain species of lepidoptera often differed from each other in colour and marking, and sometimes in form and size to a very considerable extent. For this phenomenon the convenient term "Antigeny" has been proposed by Mr. S. H. Scudder.[1] In accordance with Darwin's theory of sexual selection we find that when the sexes of a butterfly differ to any marked extent in colour, it is generally the male which is the more gaudily coloured, although there are certain genera in which the reverse obtains; but, as I pointed out in NATURE (vol. iii. p. 508), there is reason to believe that in these exceptional cases the males may be the selecting sex. Mr. Charles Darwin having recently called my attention to a paper on this subject in *Kosmos*,[1] by that most philosophical entomologist, Fritz Müller, I have thought that an abstract might interest readers of NATURE.

The species of which the author treats, *Epicalia acontius*, has such very dissimilar sexes that Fabricius described them as distinct species, calling the male *Antiochus* and the female *Medea*, while in Doubleday and Westwood's "Genera of Diurnal Lepidoptera" the two sexes are placed in different genera, the male in *Epicalia* and the female in *Myscelia*. It is not known with certainty who first pointed out that *Antiochus* and *Medea* were the sexes of the same species; but this fact is now

[1] *Proc.* Amer. Acad., xii. 150.

[1] "Epicalia acontius. Ein ungleiches Ehepaar," *Kosmos*, January, 1879, p. 285.

established beyond doubt. Were this not the case *Antiochus* would be without a female and *Medea* without a male. Indeed Fritz Müller has reared from larvæ both sexes of an allied species, *Epicalia numilia*, which differ from one another to the same extent as do the sexes of *E. acontius*. In both sexes of this latter species the general ground colour of the wings is black, the male having a broad oblique bar of a bright orange colour extending from about the middle of the inner margin to about the middle of the fore-wing in the direction of the apex. There is a corresponding blotch near the middle of the hind-wing, so that when the wings are extended the bar on the fore-wing is continuous with the blotch on the hind-wing, the whole forming one oblique orange bar. The female (*Medea*) has two oblique rows of pale yellow spots across the fore-wings running nearly parallel with the costal margin, and two similar rows across the hind-wings; when the insect rests with outstretched wings, the fore- and hind-wings overlap so that the spots of all four wings form three straight parallel rows which are continued on the body by spots of the same colour. The sexes of *E. numilia* differ in a similar manner.

Further, in the female of *E. acontius* (as in both sexes of *E. numilia*), the inner margin of the fore-wing is nearly straight, while it is markedly curved in the male. Both wings in this latter sex are also much broader in proportion to their length than is the case with the female, and in consequence of this, the wings of *Antiochus* overlap each other to such an extent that nearly half the hind-wing is hidden beneath the fore-wing, the space thus concealed being fully twicc as broad as in *Medea*. The curvature of the inner margin of the fore-wing of a butterfly when exaggerated on the over-lapping portions of the two wings, is, according to the author, a never-failing indication of the presence of a scent-secreting organ at this spot. Thus, having read in Doubleday and Westwood's "Genera" that in the fore-wing of *Ageronia* "the inner margin in the male is occasionally dilated," Fritz Müller caught a male specimen of *A. arethusa*, and found a strong odour to be emitted by a scent organ concealed between the wings. Now in *Antiochus* a similar organ exists, while it is absent in the male of *E. numilia*, and in this latter the fore and hind wings overlap only to the same extent as in the female.

When in lepidoptera the sexes of a species differ from one another to any great extent in colour and marking, the female is generally inconspicuous or is coloured gaudily in imitation of some other species (mimicry). Thus in *Thecla hemon* the male is bright blue, while the female is dull brown, while in *Dyschema amphissa* the male is white, and the female is one of the numerous mimickers of *Acræa thalia*. This explanation, however, does not apply to the female of *E. acontius*, since there is no species marked in a similar manner which might serve as a model for mimicry. On the other hand, the *Medea* type of marking is to be found in a large number of species of the same and of allied genera (the female of *Myscelia orsis*, for example). Neither can the coloration of Medea be considered protective, since it is very conspicuous, and the insect has a habit of sitting with wings fully expanded.

According to Darwin's theory of sexual selection,[1] the ancestor of the present genus *Epicalia* was probably of the *Medea* type—the present form of the male having resulted from selection by the female. The author then asks whether *Medea* has preserved the form of marking common to both sexes of the progenitor, and whether this marking has any present significance; also: "Is the colour ornamental, or for offensive or defensive purposes, or both?—for the one does not exclude the other." In reply to the latter part of this question, dissent is expressed from Prof. Gustav Jaeger's view, that yellow is as a rule an offensive or defensive colour. It is next pointed out that in the female of *E. numilia*, the row of spots is replaced by a broad oblique yellow bar, this alteration of pattern being attributed to sexual selection by the males, which must have thus preserved but at the same time slightly modified, the taste of the common ancestor of the genera *Epicalia* and *Myscelia*, the females of a few species of which have been made to depart to a much greater extent from their congeners by a greater divergence of taste on the part of their mates. The females of most of the species of these genera had, however, "set the fashion" in a completely new direction, and thus brought about the dissimilarity of the males.

[1] "Descent of Man." i. 388.

In support of this view the author remarks, that although sexual selection is generally regarded as being exerted by the females, yet, as Haeckel has maintained,[1] the selection by the males must have an equal influence on the opposite sex. That such a choice is exerted by butterflies the author has already pointed out.[2] In the present case we must believe that the two sexes manifested completely different tastes,[3] just in the same manner as much that we consider physically or intellectually superior in woman would be considered unfitting for men.

The acquisition and modification of the *Medea* type of marking may have occurred at a time when both sexes of the ancestral form were alike. Such peculiar marking could not have been produced by the direct action of external conditions nor by any innate "laws of growth," neither can it be considered as a protective colouring produced by natural selection. Sexual selection is thus the only explanation left open.

It has been shown by Weismann that the colour and marking of butterflies' wings are undoubtedly affected by external conditions, and in the case of larvæ markings, which, through such conditions, make their appearance on one segment, not unfrequently extend to other segments (by correlation of growth?). The same appears to hold good for the wings of butterflies: markings which through any cause appear in any one wing cell tend to be repeated on corresponding places in the other wing-cells. When such markings serve as signs of distastefulness or for other protective purposes, they would be preserved, and even increased in brilliancy and size by the action of natural selection. Thus a striped butterfly might be produced from a simple grey or brown one, and the markings regularly repeated on the corresponding places of the wing-cells would not fail to give us a pleasing impression, although no selection with special regard to beauty had taken place. In such cases, however, it is obviously immaterial whether the markings of the fore- and hind-wings harmonise or not. When, however, we have an unbroken bar across both fore- and hind-wings so arranged that the pattern is only complete when the insect sits with outstretched wings, or is in an attitude of flight, while in every other position the bar is broken, it may be safely assumed that the ever-vigilant eye of selection had brought about this result.

The markings of *Medea* are then considered from this point of view. The two rows of yellow spots on each wing, as already described, form three straight rows when the wings are spread out as in flight; in any other position—if, for instance, the fore-wings are pushed too forward or too far backwards—the symmetry is broken. Special attention is called to the fact that the hindmost rows of spots on the hind-wings have been distorted so as to form a straight bar parallel with the other rows; this results from the displacement of the spots, each of which, although situated in one wing-cell, does not appear on the *corresponding place* in each cell; were this the case, the row would be curved instead of straight. That it was the sense of beauty of a critical eye which straight-

[1] "Generelle Morphologie," 1866, ii. 244. [2] *Kosmos*, ii. 42

[3] The term "reciprocal sexual selection" might be advantageously applied to such classes of cases.—R. M.

ened the original curved row of spots to a straight bar, is most strikingly shown by the two foremost spots of the row which are unsymmetrical with regard to the corresponding row on the front wings, and which really form the commencement of a curved bar, *but these are hidden by the overlapping of the front wings.*

Thus it was perhaps the selection of males by the females that first perfected the *Medea* type among the progenitors of the genus. Later on the males of some of the species may have been completely modified (as with *E. acontius*), while the females retained their peculiar pattern (by reciprocal selection or by sexually limited inheritance?) down to the present time.

In conclusion, attention is directed to the scent-secreting organ of *Epicalia acontius* as compared with that of another butterfly belonging to a quite different group, viz., *Antirrhæa archæa*, the organ being almost identical in these two widely-separated species, and thus affording a striking instance of what is well known to evolutionists as "analogy," in contradistinction to "homology."

R. MELDOLA

SUN-SPOTS AND COMMERCIAL CRISES

I HAVE been repeatedly told by men who have good opportunity of hearing current opinions, that they who theorise about the relations of sun-spots, rainfall, famines, and commercial crises are supposed to be jesting, or at the best romancing. I am, of course, responsible only for a small part of what has been put forth on this subject, but so far as I am concerned in the matter, I beg leave to affirm that I never was more in earnest, and that after some further careful inquiry, I am perfectly convinced that these decennial crises do depend upon meteorological variations of like period, which again depend, in all probability, upon cosmical variations of which we have evidence in the frequency of sun-spots, auroras, and magnetic perturbations. I believe that I have, in fact, found the missing link required to complete the first outline of the evidence.

About ten years ago it was carefully explained by Mr. J. C. Ollerenshaw, in a communication to the Manchester Statistical Society (*Transactions*, 1869-70, p. 109), that the secret of good trade in Lancashire is the low price of rice and other grain in India.[1] Here again some may jest at the folly of those who theorise about such incongruous things as the cotton-mills of Manchester and the paddy-fields of Hindostan. But to those who look a little below the surface the connection is obvious. Cheapness of food leaves the poor Hindoo ryot a small margin of earnings, which he can spend on new clothes; and a small margin multiplied by the vast population of British India, not to mention China, produces a marked change in the demand for Lancashire goods. Now, it has been lately argued by Dr. Hunter, the Government statist of India, that the famines of India do recur at intervals of about ten or eleven years. The idea of the periodicity of Indian famines is far from being a new one; it is discussed in various previous publications, as, for instance, "The Companion to the British Almanack for 1857," p. 76. The principal scarcities in the North-Western and Upper Provinces of Bengal are there assigned to the years 1782-3, 1792-3, 1802-3, 1812-13, 1819-20, 1826, 1832-3. Here we notice precise periodicity up to 1812-13, which, after being broken for a time, seems to recur in 1832-3.

Partly through the kind assistance of Mr. Garnett, the Superintendent of the British Museum Reading Room, I have now succeeded in finding the data so much wanted to confirm these views—namely, a long series of prices of grain in Bengal (Delhi). These data are found in a publication so accessible as the *Journal* of the London Statistical Society for 1843, vol. 6, pp. 246-8, where is printed a very brief but important paper by the Rev. Robert Everest, chaplain to the East India Company, "On the Famines that have devastated India, and on the Probability of their being Periodical."

[1] This view is confirmed by the opinion of Mr. E. Helm, as given in the *Transactions* of the same society for 1868–9, p. 76.

Efforts have, I believe, been made by Dr. Hunter, Mr. J. H. Twigg, and probably others, to obtain facts of this kind, which would confirm or controvert prevailing theories; but this little paper, which seems to contain almost the only available table of prices, has hitherto escaped the notice of all inquirers, except, indeed, Mr. Cornelius Walford. The last number of the *Journal* of the London Statistical Society contains the second portion of Mr. Walford's marvellously complete account of "The Famines of the World, Past and Present," a kind of digest of the facts and literature of the subject. At pp. 260-1 we find Everest's paper duly noticed. In this latter paper we have a list of prices of wheat at Delhi for seventy-three years, ending with 1835, stated in terms of the numbers of seers of wheat—a seer is equal to about 2lb. avoirdupois—to be purchased with one rupee. As this mode of quotation is confusing, I have calculated the prices in rupees per 1,000 seers of wheat, and have thus obtained the following remarkable table:—

Price of Wheat at Delhi

1763	50 M.C.	1800	22
1764	35	1801	23
1765	27	1802	25
1766	24	1803	65 M.
1767	23	1804	48 C.
1768	21	1805	33
1769	24	1806	31
1770	28	1807	28
1771	33	1808	36
1772	38 C.	1809	40
1773	100 M.C.	1810	25 C.
1774	53	1811	28
1775	40	1812	44
1776	25	1813	43
1777	17	1814	30
1778	25	1815	23 C
1779	33	1816	28
1780	45	1817	41
1781	55	1818	39
1782	91	1819	42
1783	167 M.C.	1820	46
1784	40	1821	38
1785	25	1822	35
1786	23	1823	33
1787	22	1824	39
1788	23	1825	39 C.
1789	24	1826	48 M.C
1790	26	1827	30
1791	33	1828	22
1792	81 M.	1829	21
1793	54 C.	1830	21
1794	32	1831	26
1795	14	1832	22
1796	14	1833	33
1797	15	1834	40 M.
1798	8	1835	25
1799	17	1836	— C.

The letter M indicates the maxima attained by the price, and we see that up to 1803, at least, the maxima occur with great regularity at intervals of ten years. Referring to Mr. Macleod's "Dictionary of Political Economy," pp. 627-8, we learn that commercial crises occurred in the years 1763, 1772-3, 1783, and 1793, in almost perfect coincidence with scarcity at Delhi. M. Clément Juglar, in his work, "Des Crises commerciales, et de leur Retour périodique," also assigns one to the year 1804. After this date the variation of prices becomes for a time much less marked and regular, and there also occurs a serious crisis about the year 1810, which appears to be exceptional; but in 1825 and 1836 the decennial periodicity again manifests itself, both in the prices of wheat at Delhi and in the state of English trade. The years of crisis are marked with the letter C.

When the above numbers are plotted out in the form of a curve, the earlier part of the series presents the appearance of a saw, with four or five high, sharp-pointed teeth at almost exactly equal distances of ten years. The first maximum, that of 1763, is perhaps imperfectly represented, and were the table extended backwards, the true maximum might fall in 1762. It is remarkable that after about the year 1807 the character of the curve suddenly and entirely changes, the oscillations becoming comparatively small, irregular, and rounded, although the periodicity, as already remarked, seems to recur in a less intense degree after 1823. This change in the curve may be due to some local causes, such as the opening of new roads and markets, and it is obviously important that we should learn whether this is the case, or whether some important meteorological variation is here manifested. This is not the only instance in which a well-marked decennial oscillation appears to be for a time suddenly arrested or thrown into confusion.

One difficulty which presents itself in connection with the above table is that the commercial crises in England occur *simultaneously* with the high prices in Delhi, or even in anticipation of the latter; now the effect cannot precede its cause, and in commercial matters we should expect an interval of a year or two to elapse before bad seasons in India make their effects felt here. The fact, however, is that the famines in Bengal appear to follow similar events in Madras. Thus it is well known that the great famine occurred in the year 1770, or even began in 1769, though it seems not to have made its mark at Delhi until 1773. This quite explains the fact that the English crisis was in 1772-3. Mr. F. C. Danvers, of the India Office (*Journal of Science*, N.S., vol. viii. p. 436), assigns famines in the Madras Presidency to the years 1781-3 and 1790-2. In fact Mr. Danvers explicitly points out this tendency of famines to travel northward, saying (p. 441): "It is a point worthy of remark that severe droughts in Northern India have, on several occasions, followed closely upon distress similarly caused in the Peninsula of India; thus the Madras famine of 1781 to 1783 was followed by one which affected Bengal, the north-western provinces, and the Punjab in 1783-4; the failure of rains which resulted in scarcity in many of the provinces of the Madras Presidency in 1824-5, was followed by a similar calamity in the North-western Provinces in the succeeding years. The "Guntoor" famine of 1833 preceded only by a few years one which affected the north-western and lower provinces of Bengal in 1837-8, and the Madras famine of 1866 was very closely followed by one in the North-western Provinces and the Punjab in 1868 to 1870." We see, then, that in looking for periodicity, we must confine each comparison to events of the same locality. It must also be allowed that the *commencement* of famine in India precedes by about two years the occurrence of commercial collapse in England.

It ought to be added that Everest refers to a journal published at Calcutta, called *Gleanings of Science*, which contains (vol. i. p. 368) a table of the prices of various kinds of grain at Chinsurah in Bengal, from 1700 to 1813. The volume is to be found in the British Museum; but on referring to it and plotting out the curve for the price of rice, it was very disappointing to find the series broken by gaps of several years every here and there, which renders it impossible to draw any safe inference, affirmative or negative. The table is said to have been drawn up by G. Herklots, the fiscal of Chinsurah, from authentic documents. Now, if such documents existed half a century ago, it is indispensable that minute inquiry should be made for any local records of the kind which may still exist.

Returning to the prices at Delhi, and taking the above table in connection with a mass of considerations of which I have given a mere outline at the last meeting of the British Association (see *Journal* of the Statistical and Social Inquiry Society of Ireland, August, 1878, pp 334-42; NATURE, vol. xix. pp. 33-37). I hold it to be established with a high degree of probability that the recurrence of manias and crises among the principal trading nations depends upon commerce with the east. This conclusion is confirmed by the fact that these fluctuations are but slightly felt by the non-trading nations, and that what these nations do feel is easily accounted for as an indirect effect.

It has been objected by the *Economist* that this explanation cannot be applied to the earlier crises in the years 1711,1721, and 1732, because trade with India was then of insignificant dimensions. But the reading of many old books and tracts of the seventeenth and eighteenth centuries has convinced me that trade with India was always looked upon as of the highest importance. A large part of the political literature of the time was devoted to the subject, and under the Mercantile Theory the financial system of the country was framed mainly with an eye to Indian trade. The published returns of exports and imports probably give us little idea of the real amount of trade, as smuggling was very common in those days, and much of the Indian trade went on secretly in private ships or indirectly through Holland.

Dr. George Birdwood has lately been studying the records of the India Office, and he gives as the result of his extensive reading "that the history of modern Europe, and emphatically of England, has been the quest of the aromatic gum-resins, and balsams and condiments, and spices of India and the Indian Archipelago" (*Journal* of the Society of Arts, February 7, 1879, vol. xxvii. p. 192). This closely corresponds with the view which I have been gradually led to adopt of the cause of decennial crises.

While India is, no doubt, together with China, the principal source of disturbance, there is no reason to suppose that it is the only source. A nearly exhaustive analysis which I have made of the trade of England with various parts of the world during the last century, as given in Whitworth's valuable tables, fails to disclose any clear periodicity as regards European trade. The investigation of various long series of prices of agricultural produce in Europe also leads me to believe that the decennial periodicity, if felt in Europe at all, is over-borne by disturbing causes, or involved in too great complication to admit of discovery. On the other hand, I have fallen upon the very interesting and significant fact that the export trade from Maryland and Virginia exhibits what seems to me an unquestionable periodicity, with maxima in the years 1701, 1711-13, 1720, 1742, 1753, 1764, and 1774. The same tendency is not apparent in the trade of New England. Thus it is likely that crises may have an independent meteorological origin in the semi-tropical States of the Union; and, if so, it is probable that there are other tropical parts of the world where the meteorological conditions allow the cycle to manifest itself. This subject, so far as it has yet been studied, is full of important and mysterious facts, which stimulate the interest of the inquirer in a high degree. At the same time it is plain that sound conclusions can be reached only by most extensive analyses and comparisons of large series of facts. The search for the facts, too, among the records of the last two centuries, the suitable part of which has in too many cases probably perished, is so tedious and disappointing that it taxes the patience of the inquirer very severely. It is no jest at all.

But whatever be the area of the tropical and semi-tropical regions from which the decennial impulse comes, mainly India and China, no doubt, it does not follow that the extent of the commercial mania or crisis here is bounded by the variation of the foreign trade. The impulse from abroad is like the match which fires the inflammable spirits of the speculative classes. The history of many bubbles shows that there is no proportion be-

tween the stimulating cause and the height of folly to which the inflation of credit and prices may be carried. A mania is, in short, a kind of explosion of commercial folly followed by the natural collapse. The difficulty is to explain why this collapse so often comes at intervals of ten or eleven years, and I feel sure the explanation will be found in the cessation of demand from India and China occasioned by the failure of harvests there, ultimately due to changes of solar activity. Certainly the events of the last few years, as too well known to many sufferers, entirely coincide with this view, which is, nevertheless, made the subject of inconsiderate ridicule.

Hampstead, April 23 W. STANLEY JEVONS

JAMES NICOL, F.R.S.E., F.G.S.

ANOTHER of the links connecting us with the early days of geology has been severed by the death of the Professor of Natural History in the University of Aberdeen. For some years past Prof. Nicol's failing health prevented him from undertaking more work than his college duties required, so that he had somewhat fallen behind the crowd of younger aspirants to scientific reputation. It is a pleasant duty to recall his early services to geology. As far back as the year 1843 we find him contributing to the series of prize essays of the Highland Society a memoir on the geology of his native county, Peebleshire. Devoting himself with energy to the prosecution of his favourite pursuits, he prepared a useful little Guide to the Geology of Scotland, illustrated with maps and sections, and giving, from his own observations and the researches of previous writers, a compendious account of Scottish geognosy, so far as then known. Many years afterwards he published another compilation of Scottish geology in the form of a Geological Map of that country. He specially took up the mineralogical and petrographical department of geology, and showed his capacity for these subjects by publishing a text-book of mineralogy, which has kept its place as a work of reference. Appointed Assistant Secretary of the Geological Society, he in that capacity edited the Society's Journal, and had an opportunity of coming personally in contact with the foremost geologists of his time. Among those whose friendship he formed, one of the kindest and most serviceable was Murchison. Through the assistance of that active and powerful friend Nicol was appointed to the Chair of Geology at Cork, and a few years afterwards to the more lucrative post at Aberdeen, which he resigned only last year. During these years of official work he found time for a number of original papers chiefly on the geology of different parts of Scotland. Thus he returned once more to the study of the rocks of his own Tweed Valley to which he had been the first definitely to apply the term silurian. In company with his friend and benefactor Murchison, he extended these observations into Ayrshire and the west of Scotland. With the same companion he visited the north-west of Scotland, and after a long journey through these regions produced an independent memoir, in which he suggested that much of the metamorphic rocks of the north-west Highlands consisted of altered Carboniferous formations. When the fossils found in the Assynt limestones proved to be unquestionably Lower Silurian he was of course compelled to retract his published suggestion. He then adopted a completely opposite view and endeavoured to prove that the rocks which he had thought might be altered Carboniferous were really the most ancient or fundamental masses of the west coast brought up everywhere to the surface again by a vast dislocation and inversion. In this view, no less than in that for which it was substituted, he was opposed by Murchison, who proved by many sections that the rocks in question really lay upon the fossiliferous limestones and could not therefore be older than the Lower Silurian period. From the time of this dispute the late professor devoted himself chiefly to his duties at Mareschal College, where his capacity for business made him a most useful colleague. From summer to summer, however, he could resume the hammer and renew his acquaintance with old haunts or make himself familiar with new ones. In these excursions he was sometimes accompanied by an old geological friend to whom he could communicate the views he no longer cared to publish. With a kindly nature he united a certain timidity which made him shrink from publicity and led to his being less widely known than his personal qualities deserved that he should be.

NOTES

THE International Meteorological Congress was opened at Rome on Tuesday last week, nearly all the Countries of Europe being represented, as well as the United States. Prof. H. P. S. Smith and Mr. Scott represented this country. Prof. Cantoni was elected president, M. Wild, of St. Petersburg, vice-president, Dr. Hoffmeyer, of Copenhagen, and Mr. Scott, secretaries. The introductory address was given by M. Depretis, who spoke of the great influence exercised by the physical sciences on the progress of the other sciences, and consequently on the moral and economical development of nations. He referred to the important place of meteorology among the physical sciences, and concluded by welcoming the strangers to Italy. Dr. Buys Ballot was unable to be present, but Prof. Mascart read an address sent by him, full of scientific data and statistics, passing in review all the discoveries recently made in America and Europe in meteorological science. The report on the work of the permanent committee was read by the secretary of the committee, Mr. Scott. The congress then divided into sections for work.

THE annual meeting of the French Sociétés Savantes commenced on April 16 at the Sorbonne. The general sessions of the Section of Sciences were held under the presidency of M. Milne Edwards, on April 16, 17, and 18. MM. Faye and Wurtz were vice-presidents, and M. Blanchard the secretary. M. Faye delivered a lecture on the 18th in the large hall, on the Great Movements of the Atmosphere. General Nansouty, the Director of the Pic du Midi Obsevatory, gave an address, in which he complained of the interruptions in the telegraphic communications with Toulouse, caused by the snows during winter, and insisted upon the necessity of placing the wire under ground. M. Ferry, the Minister of Public Instruction, who is president, said that he should take the measures which were asked for by the gallant observer, whose devotion to science was so widely admired in France and abroad. M. Alluard, Director of Pry de Dôme Observatory, presented a series of maps tabulating the readings taken at Clermont Ferrand and on the top of the mountain. An intermediate station has been established. The final meeting of the Congress took place in the large hall of the Sorbonne, under the presidency of M. Ferry, who was assisted by a large number of officials. Five reports were read on the works of the Sociétés Savantes. The Minister, as usual, delivered a speech stating the projects of his administration. The number of learned societies in France is now 360. He stated that the Government spent 11,000,000 frs. in 1870 for the Faculties; the sum was now 30,000,000 frs. The list of rewards granted was then read over. The four gold medalists in science are M. Combercure, of Montpellier, for mathematical disquisitions, M. Dieulafait, of Marseilles, for geology, M. Coquillon, for determining the quantity of inflammable gas contained in the air of coal-mines, and M. Schrader, for explorations in the Pyrenees.

THE French Physical Society held its annual meeting the other day at the Hotel of the Societé d'Encouragement, rue Bonaparte. The *façade* of the hotel was illuminated by a series of twenty-four lamps of the Reynier system fed by three large gramme machines, each of them consuming from three to four horse-power. The effect was regular for several hours, but the illuminating power was very low as compared with ordinary regulators. Among the apparatus exhibited we noticed a rotation machine exhibited by M. Antoine Breguet, to demonstrate that the Gramme machine may be considered as a form of the Barlow rotating wheel. M. Nodot, *preparateur* of the Dijon Faculty of Sciences, exhibited a Barlow apparatus, in which the rotating part is formed by a series of copper wires radially implanted in the centre. M. Deprez exhibited a new motor worked by six Bunsen elements, and which gives about five kilogrammetres per second. This apparatus is analogous to a Wilde electro-magnetic machine. All the principal opticians of Paris, Breguet, Ducretet, Carpentier (formerly Ruhmkorff), Dubosc, Dumontin, Froment, Deleuil, Sauter and Lemonnier, sent in an improved or enlarged form the instruments which have attracted the attention of physicists in recent years. The Faber speaking-machine, which has been attacked by one of the French scientific papers as being worked by a ventriloquist, was exhibited and explained by M. Garrel. The display was considered as one of the most successful that has been offered to the public since the Society was established. The large halls of the hotel were crowded up to a late hour.

SINCE the commencement of the present year, the well-known weekly German botanical journal, the *Botanische Zeitung*, has passed into the sole editorship of Prof. De Bary, of Strassburg.

THOSE interested in British botany will be glad to hear that the threatened extinction of the Botanical Exchange Club, to which we recently alluded, has been averted. Mr. Charles Bailey, of Manchester, has offered to undertake the main responsibility of the curatorship, although the scope of the Club will in future be somewhat restricted. There is a proposal for issuing, in connection with the Exchange Club, a small number of copies of a reference herbarium of British plants, the difficult and critical species being especially kept in view, on the plan of Reichenbach's "Flora Germanica Exsicata."

THE *Times* Paris correspondent telegraphs that at the sitting of the Academy of Sciences on Monday, it was announced that Lavoisier's chemical apparatus, still preserved by his heirs, but hitherto left unnoticed, had been minutely inspected by Prof. Truchot, of Claremont Ferrand. It is in excellent preservation, and the accompanying documents show that Lavoisier was the author of the work on sea-water distillation published anonymously in England.

AN exhibition of an interesting kind is to be held in Dresden in the summer of this year. This is a general exhibition of objects of art, science, and industry, connected with the education and training of youth. The following are the various classes into which the exhibition will be divided:—1. Teaching material for schools, home, Kindergärten, &c. 2. Printed works, as schoolbooks, children's books, illustrations. 3. Gymnastic and similar apparatus. 4. Musical instruments. 5. Toys. 6. Articles required for children in all departments of industry, as furniture, linen, clothes, orthopædic instruments, &c. A systematic exhibition of the development of various school materials will be connected with the above, and historical objects connected with training and education are therefore desired. A similar exhibition on a small scale in 1877 had a great success. The Committee of the Exhibition consists of one merchant and three teachers. Inquiries should be addressed to Herr Kaufmann C. Heinze, Dresden. The Exhibition will be open from July 1 to August 31.

SECONDARY Technical Education forms the subject of the address delivered before the American Institute of Mining Engineers by their president, Mr. Eckley B. Coxe, at their Baltimore meeting. Great stress is laid on the necessity of educating workmen, and the maxim, a little learning is a dangerous thing, is combated by the statement that false learning mingling with the truth causes the danger. If the truth of the maxim were admitted we fail to see that it provides a very strong argument against education. A little dynamite is a dangerous thing, but it is of great use in mining work. The author states that they have already five good schools at their works, and proposed to establish another to carry on the education of those boys who have left them to enter the works. While admitting that an average boy cannot work all day and study all the evening, and foreseeing the possibility of making Jack a dull boy, the programme of studies is sketched out "as far as we have been able to arrange it." It comprehends instruction in algebra, geometry, trigonometry, free-hand and mechanical drawing, with descriptive geometry, physics, chemistry, mineralogy, and geology, mechanics and the construction of machines, framing, mining, and mine surveying, English composition, book-keeping, and, last of all, writing. Our astonishment is that not only is arithmetic omitted but lunar and planetary theory have no place assigned to them.

The *Madras Times* writes:—The necessity of a scientific training for coffee planters is now being recognised. Men of intelligence, industry, and steady habits can alone hope to succeed as coffee planters. We would urge upon estate owners the expediency of insisting upon their superintendents and assistants possessing a knowledge of chemistry, sufficient at least for the purposes of coffee planting. They should be able to make analyses of the coffee tree, soil, manures, &c. Planters should also be able to take correct observations of the weather, gauge the rainfall, take notes of the nature and progress of the various diseases the coffee tree is subject to, and so forth. The paper of questions submitted by Mr. Harman to the various coffee planters in Coorg will put their capabilities to the test, and though we are aware that there are many educated and intelligent planters in that province, we fancy some of them will find it no easy task to answer the last of Mr. Harman's questions: Can you give analyses of your rock soil and sub-soil?

ON January 15, 1880, an International Exhibition of products of agriculture, industry, science, and fine arts will be opened at Mexico. The Exhibition will remain open for three months.

THE Anthropological Exhibition at Moscow was opened on the 16th inst. The International Anthropological Congress, connected with this exhibition, will however not meet until August 7 next.

SLIGHT shocks of earthquake were noticed at Montmarault and Chantelle, in the French Department of the Allier, on March 27th. A slight shock of earthquake, lasting about fifteen seconds, and travelling from east to west, was felt at Darjiling at 8.15 on the morning of the 11th ult.

THE death is announced of Mr. William Mudd, the curator of the Botanical Gardens, Cambridge, after a brief illness. The stipend attached to the office is about 100*l*. a year; it is in the gift of the Botanical Garden Syndicate.

MARK W. HARRINGTON, M.A., F.L.S., lately Professor of Astronomy at the Imperial University of Peking, and formerly assistant Professor of Botany at the University of Michigan, has just been appointed Professor of Astronomy and Director of the Observatory at the last-named institution, the chair rendered vacant by the resignation of Dr. James C. Watson, now Professor of Astronomy at the Wisconsin University.

A COURSE of three lectures will be delivered in the Galleries of Natural History and Antiquities, British Museum, on the 24th, 28th, and 30th inst., by Dr. Carter Blake, of Westminster Hospital. Some of the keepers of departments will also give *vivâ voce* explanations of the specimens under their care.

MR. ARTEMAS MARTIN, a well-known American mathematician, has lately commenced the publication of a serial work, entitled *The Mathematical Visitor*, appearing occasionally at intervals of several months. It is published by him at Erie, Pennsylvania, and shows a creditable spirit of enterprise in entering a field which is not generally considered remunerative.

WE have received the first number of the *American Chemical Journal*, which promises to take a high place in scientific literature. Its first object is to collect the good original papers written by American chemists, and to make them the basis of a purely chemical journal, while papers from other journals, and notes in all departments of chemistry will find a place. It is expected that six numbers will appear yearly. The principal articles in the first number are "On the Complex Inorganic Acids," by Dr. W. Gibbs, "On Nitrogen Iodide," by Mr. J. W. Mallet, a paper on Lockyer's hypothesis that the so-called elements are compound bodies, by Mr. C. S. Hastings, of Johns Hopkins University, and "On the Oxidation of Substitution Products of Aromatic Hydrocarbons," by Messrs. Remsen and Iles. There are, besides, several reviews and a number of notes.

THE scientific journals of Pennsylvania express much regret at the possibility of the failure of the Legislature of that State to make appropriations for the continuance of the work of the Geological Survey. This work has been carried on for several years past under Prof. Lesley with great success, and it is so near completion that its cessation now might be considered almost a national calamity.

THE War Department is on the point of at length adopting war balloons into the land and sea services. Movable apparatus for inflating and manipulating military balloons in the field has just been completed in the Royal Arsenal, Woolwich, and been tried with two new balloons, specially constructed for military purposes. The appliances consist of a portable tank, weighing 400 lb., containing iron shavings, together with a portable boiler and furnace. These appliances can be moved about with troops on the field or on vessels at sea. Hydrogen is generated by passing steam through the iron turnings. As soon as the necessary arrangements can be made it is in contemplation to send a few war balloons out to Zululand.

THE boring of an artesian well for the purpose of investigating the nature of the chalk layers through which the submarine tunnel between England and France is to pass was resumed on the French coast at Sandgatte on March 1 last. The depth of the well, which at the end of last year was at 34·35 metres, was extended to 38·50 m.—that is, to a depth corresponding to 8·67 m. below the low-water level. At this depth the flow into the well amounted to 1,300 litres per minute, and the exhaust machines became insufficient. They are to be replaced by more powerful ones.

WE are glad to see that Messrs. Kegan Paul and Co. have published in a handy form a complete translation of Prof. Haeckel's "Freedom in Science and Teaching," first reproduced in this country in our own columns. There is an interesting prefatory note by Prof. Huxley.

A NEW monthly electrical paper has been started in Paris, the *Lumière electrique*, intended as a general organ of electricity.

WE have received from Messrs. Cole and Sons, of Notting Hill, several specimens of pathological, physiological, and educational preparations for the microscope, which for cutting and mounting surpass anything we have seen. They are really beautiful preparations, and deserve to be widely used.

THE Report of the Marlborough College Natural History Society for the past year is an unusually satisfactory one. Several modifications in the rules have been attended with good results, and the Society seems in a fair way to become a real working one. The papers by the boys and others are highly creditable; perhaps the most generally interesting is that by Mr. Rodwell, on Iceland. The Report of the Winchester and Hampshire Scientific and Literary Society is not quite so satisfactory as could be wished; the *dilettante* and absolutely idle elements seem large, and the Report complains of the indifference to the less popular subjects. We trust that the next Report will be more satisfactory.

Apropos of electric perforation of glass, Prof Waltenhofen, of Prague, has recently described the following experiments:—A thin glass plate, having on it a small drop of stearine, is introduced into the spark-path of an electric machine. It is perforated at the part where the drop is, and more easily so when the drop-side is turned to the positive electrode. A glass plate, hung bifilarly between the electrodes of a Holtz machine, is driven by the discharge towards the negative electrode, and more strongly, if the side turned towards the positive electrode be partly covered with stearine. Prof. Waltenhofen considers that the rapidly-moving air-molecules in the spark-path are ruled by a component of velocity directed from the positive to the negative electrode.

THE following subjects in natural science have been proposed by the Society of Arts and Sciences of Utrecht, for prize competition:—1. Researches on the development of one or several species of invertebrates whose history is not yet known. 2. Researches on the influence of small variations in exterior circumstances on the evolution of the embryo of one or several species of vertebrates. 3. Exact anatomical description of the larva and nymph of the common cockchafer (*Melolontha vulgaris*). 4. By what means may the water of rivers which traverse Holland be purified so as to become potable, without any injury to health? What would be the expense of applying them on a large scale? 5. A memoir on the results of experiments made in recent times on the motion of liquids and the resistance they offer to moving bodies; with an exposition (*a*) of the general or special laws which may be deduced; (*b*) of the principal points on which some data are still wanting, and the nature of the experiments necessary to obtain them. 6. Critical and experimental study of the functions of the semicircular canals of the ear. 7. Critical and historical study of the theories of electric phenomena observed in muscles and nerves. 8. Critical *aperçu* of the methods employed to determine the place which substituted atoms and groups of atoms occupy in bodies of the aromatic series, according to the theory of the constitution of benzol given by Kekulé and Ladenburg. 9. Determine rigorously the quantities of heat liberated or absorbed in the allotropic change of two or several simple substances. Each prize consists of an honorary diploma and about 25*l*. Papers must be sent in to the Secretary before December 1, 1879.

THE additions to the Zoological Society's Gardens during the past week include three Red Brockets (*Cervus rufus*) from Brazil, presented by Mr. W. H. Lacy; a Blue-faced Green Amazon (*Chrysotis bouqueti*) from St. Lucia, West Indies, a Yellow-fronted Amazon (*Chrysotis ochrocephala*) from Guiana, presented by Mr. Neville Holland; a Black-faced Kangaroo (*Macropus melanops*) from South Australia, three White-eared Conures (*Conuras leucotis*) from Brazil, an Upland Goose (*Bernicla magellanica*) from the Falkland Islands, deposited; a Reeve's Muntjac (*Cervulus reevesi*) born in the Gardens.

ON THE DEVELOPMENT OF THE SKULL AND ITS NERVES IN THE GREEN TURTLE (CHELONE MIDAS), WITH REMARKS ON THE SEGMENTATION SEEN IN THE SKULL OF VARIOUS TYPES[1]

FOR these embryos the author is indebted to Sir Wyville Thomson and Mr. H. N. Moseley, the latter having sent him the smaller specimens, and the former the ripe and nearly ripe young. There are in all five stages.

1st Stage, ½-in. long.—The embryo is already fairly formed, for there are rudiments of all the principal organs. About fifty-two somatomes may be counted behind the head, and there are evidently seven clefts—four post-oral, two pre-oral, and one oral. The body of the embryo is tolerably distinct from the yolk-sac, the mesocephalic flexure is well marked, and the tail is coiled upon itself.

The regions of the body, viz., cervical, dorsal, and caudal, are plain. A fold lying between the fore and hind limbs shows the commencing carapace, which is at present the only mark to distinguish it from any other Sauropsidan embryo.

Half the ventral region is now taken up by the heart and its pericardium, and behind it the ventral laminæ have not closed below, thus showing the Wolffian bodies within. Posteriorly the umbilical vessels are seen emerging

The limb-buds grow out from a continuous ridge, due to a thickening of the mesoblast at the upper part of the somatopleure.

The body-cavity extends into the head, thus corroborating

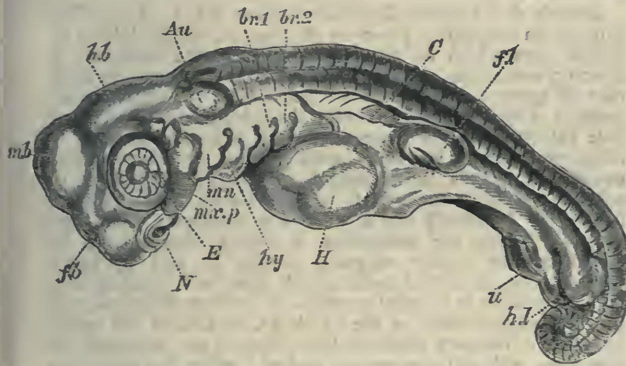


FIG. 1.—*Chelone midas*, 1st stage. *Au*, auditory capsule; *br.* 1 and 2, branchial arches; C, carapace; E, eye; *f.b*, fore-brain; *f.l*, fore-limb; H, heart; *h.b*, hind brain; *h.l*, hind limb; *hy*, hyoid; *m.b*, mid brain; *mn*, mandible; *mx.p*, maxillo-palatine; N, nostril; *u*, umbilicus.

Mr. Balfour's account of the same thing taking place in the Selachians.[2]

As far as possible, the head has formed a coil similar to that of the tail, from which it never more than partially recovers. This bending of the head, which imprisons the elements of the face, sets the dorsal region free, and the neural canal expands to form the three vesicles of the brain. Buds are already seen on the forebrain which give rise to the hemispheres.

The axial tissues, below the nervous structures, are thickening into embryonic cartilage.

A sectional view shows the mid-brain to be bent like a horseshoe, forming the "middle trabecula" of Rathke, which is occupied by the apex of the notochord, its investing structures, and the third nerves.

Three pairs of chambers, for the organs of special sense, are built in the sides of the cranium.

The mouth is formed as in other types, by the extension of the third pair of clefts into one another; the fourth or mandibulohyoid cleft, being what is usually known as the first cleft.

The first post-oral visceral arch, forms most of the framework and machinery of the mouth, but as a rule, rudiments of pre-oral arches, supplemented by sub-cutaneous bones, finish the upper jaw.

On the whole, the series of clefts and folds along the face of the embryo are at this stage very regular, and the sense capsules are intimately connected with those which lie below them.

[1] Abstract of a paper by Prof. Parker, F.R.S., read at the Royal Society on Februnry 13.

[2] This extension of the body-cavity is also seen in the lizard.

The slit-like opening of the nasal sac and the space between the eyeball and maxillo-palatine fold are very probably openings of the same nature as those behind them.

The tubular cartilage that forms round the external nostril is homologous with the "labial" that serves the same purpose in the Icthyopsida.

The floor of the skull is open under the fore-brain, and the double maxillo-palatine fold is sharply severed from the nasal fold. The fronto-nasal process is but little freed from the inferior cranial wall and the nasal folds.

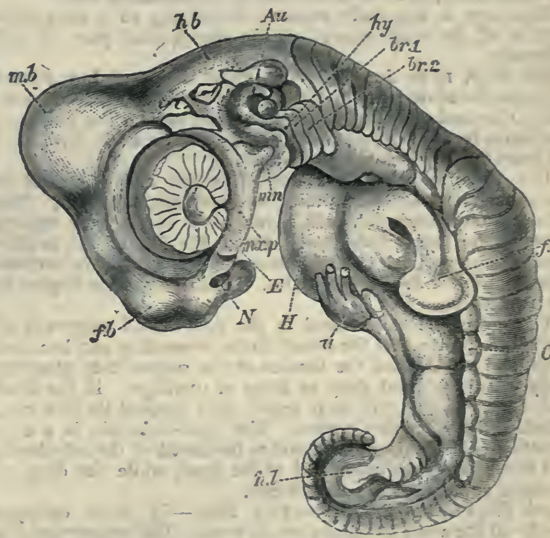


FIG. 2.—*Chelone midas*, 2nd stage. Letters as before.

2nd Stage, ¾-in. long.—The proximal part of each limb now lies adherent to the infero-lateral region of the body in a manner very similar to what is seen in the osseous fishes. The edge above the deep sulcus between the marginal row of cutaneous folds and the ingrowing abdominal part of the body-wall is ultimately somewhat bevelled down, but it shows well that the structure in which the plastron is formed is not originally flat but trough-shaped.

The upper region of the mandible is already assuming the very image of the quadrate with its tympanic cavity and its

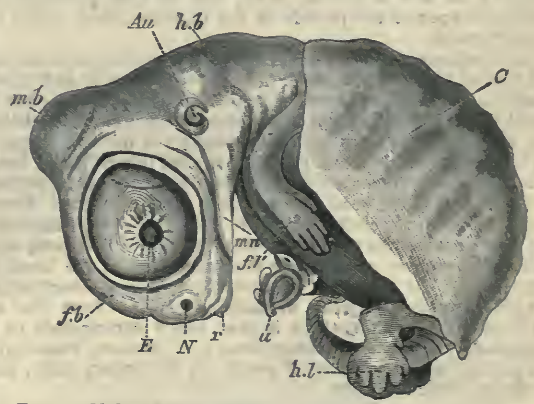


FIG. 3.—*Chelone midas*, 3rd stage. Letters as before. *r*, rostrum.

condyles. In the cavity a discoid body—the "extra-stapedial" has developed. Between the second post-oral (hyoid) fold and the hollow of the quadrate elevation, the beginning of the membrana tympani is seen.

The eyeball is now at its relatively largest size, exceeding the mid-brain in bulk.

The maxillo-palatine fold is somewhat hour-glass shaped, and has a head-cavity in its fore part.

In the middle of the palate an open space appears into which the oral lining has grown. This diverticulum is the rudiment of the pituitary body. It lies where the hypoblast and epiblast

meet, but is probably formed from the latter. The infundibulum is beginning to grow towards the pituitary body, and close in front of it are seen the optic nerves which are still hollow. Rudimentary olfactory lobes are seen where the solid olfactory nerves are given off.

Cartilage is forming in the base and sides of the skull, as well as in the ear-capsules. The notochord ascends high into the head and is slightly curved over at the end. The azygous prochordal element, or inter-trabecular bar, is of equal size to the trabeculæ, which are now articulated with the hind part of the basis cranii, in front of the base of the ascending wall.

This stage is especially valuable in helping to a clear conception of the true nature of the prochordal part of the trabeculæ.

3rd Stage, 1¼-in. long.—In this stage the head has almost acquired the adult form, and the carapace is well marked out. The abdominal region is flattened to give rise to the plastron. The limbs have also practically acquired their adult form, and the heart is fairly inclosed in the thorax.

The post-oral clefts are now filled in, and the skull is thoroughly chondrified, forming a cartilaginous trough. The trabeculæ and inter-trabecula have grown into a high septum between the eyes and nose. From the former, the orbito-sphenoids grow, and the alisphenoids extend from the orbito-sphenoids to the auditory capsules. Ossification now begins in the palate.

The notochord turns round in the post-clinoid upgrowth of of the basal plate, and the sheath in its descending part becomes solid, and ends behind the lobules of the rudimentary pituitary body as a tear-shaped drop or lump of cartilage. If the head had been straight, this drop *might* have reached its fore end, directly below the first nerves. The inter-trabecula is a continuation of the same skeletal tract as the sheath of the notochord, and it reaches to the *actual* end of the head, while the drop of cartilage approaches the *organic* end.

4th Stage—two-thirds ripe—3-in. long.—In these embryos nearly all the adult structures can be seen. The epipterygoid is still, however, a cartilaginous hook hanging down from the quadrate. The columella is well developed, and its shaft is ossified.

The parietals have grown down the sides of the skull causing the alisphenoids to be absorbed to a great extent. The investing bones are now rapidly developed, but much of the endocranium is still soft.

5th Stage—ripe—4in. long.—The processes of development and ossification have now gone so far that little can be remarked upon as differing from the adult. The epipterygoid, however, which is wedged in between the descending parietal and the pterygoid, is now a distinct bone, but its apex permanently touches the apex of the pedicle of the quadrate, from which it was segmented.

The development of *Chelone midas* corresponds in all essentials with that of the common snake (*Tropidonotus natrix*) and lizard (*Lacerta agilis*) which the author has recently worked out; but it is well worth remarking that that which distinguishes the chelonian from other reptiles is already manifest in the first stage.

The author considers that there are several things in the head of the vertebrate embryo which are evidently of a segmental nature. Firstly, nerves in the head corresponding to spinal nerves. These constantly fork over clefts, which are also signs of segmentation. The number of inferior arches, whether pre-cral or post-oral, also indicate the number of segments that may exist in the head of a vertebrate. At any rate, wherever there is any diverticulum of a pleuro-peritoneal cavity, although divided off from that of the body by the clefts, there is that which corresponds to a somatome. By this last evidence there is at least one homologue of a pre-oral somatome, and if we go by the nerves, clefts, and cartilages, there are more.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Council of King's College have decided to give the name "Wheatstone Laboratory" to the physical laboratory of the College, in honour of Sir Charles Wheatstone, who was for some years Professor of Experimental Philosophy in the College, to which he also bequeathed his valuable collection of physical apparatus. The report of the laboratory work shows that the physical laboratory was established in the year 1868, and that during the eleven years of its existence about 250 students have been trained in it in the various branches of practical physics. The average number of occasional students—*i.e.*, students who are engaged in research and do not attend with any special class, has been nine a year during the last five years. Among these are graduates of the older universities, who come to reside in London after they have completed their term of residence at the University. Engineering students in their third year's course have the privilege of working in the laboratory free of charge. There are also special practical classes which have been well attended, for the Bachelor of Science and the Preliminary Scientific M.B. Examinations of the University of London, and also special classes for evening class students who are engaged in business during the day-time. In all there are not less than forty students now engaged in practical work in physics in "the Wheatstone Laboratory" in King's College. The Laboratory is greatly in need of endowment, in order that an additional Demonstrator may be appointed, and the usefulness of the laboratory still further extended.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, March.—We note the following papers in this number:—Concerning $\frac{T_1 - T_0}{T_1}$, or the limit of efficacy of steam-engines, by Mr. Klein.—Gauging- and measuring-implements, by Mr. Richards.—A new engine-governor, by Prof. d'Auria.—Conical arches at South Street Bridge, Philadelphia, Pa., by Mr. Stauffer.—Graphic freight diagrams, by Mr. Dudley.

Bulletin de l'Académie Royale de Belgique, No. 1, 1879.—This contains an interesting paper by M. Niesten, on the colours of double stars, which he was led to study by the variations in intensity and colour of planets in relation to the sun. He finds that in systems which allow of connecting the colours with position of the satellite in its orbit, the principal star is white or pale yellow when the companion is at periaster, whereas in other positions it is yellow, gold yellow, or orange. The companion follows the principal star in its fluctuations of colour, and often exceeds it as it removes from periaster (where it is mostly white, like the principal). In perspective groups, the companion is nearly always blue, by an effect (the author suggests) similar to that by which mountains on the distant horizon look blue (and pointing to a gaseous medium in celestial space).—M. Delarge describes some instructive experiments on the telephone, applied in the neighbourhood of ordinary telegraph lines. Secrecy can be insured for telegrams, with dial-apparatus or that of Hughes, but the former is objectionable as leaving no trace, and the latter is very expensive and delicate. Hence recourse should generally be had to cipher.—M. Marchal contributes a revision of American Hederaceæ, describing eighteen new species and a genus.—M. Chevron is led to deny the inalterability of tricalcic phosphate by citrate of ammonia; but the use of this solvent for separation of the phosphate may give sufficiently exact results if a too great excess of the citrate solution be avoided.—We further note an analysis of, and reports on, the second part of M. Lagrange's work on the origin and establishment of astronomical movements, wherein is assumed that the material atoms were originally diffused through space in a state of rest and at the absolute zero of temperature, and endowed simply with reciprocal attraction.—M. Malaise writes on arsenopyrite, or mispickel, and on the arsenical water of Court Saint-Etienne.

No. 2. We have here a paper by M. Saltel on a mathematical paradox, and on a new character of decomposition due to the presence of multiple lines.—M. van Beneden records the receipt of some interesting fossils of cetacea from marls of the tertiary epoch in Croatia.

THE *Revue Internationale des Sciences* (January-March, 1879), contains the following papers of interest: On the cell soul and soul cells, by Ernst Haeckel.—On the nutrition of plants, by J. L. de Lanessan.—Analysis of two memoirs on *Noctilucæ*, by G. Carlet.—On a monstrous skeleton of a batrachian, by F. Lataste.—Researches on *Bacteria*, by Dr. Koch.—On vascular innervation, by MM. Grutzner and Heidenhain.—On the action of light and heat upon moving spores, by E. Strassburger and E. Stahl.—On contagious diseases and disinfecting agents, by Prof. Naegeli.—General observations on fertilisation, by E. Strassburger.—On a technical process for the study of fish embryos, by F. Henneguy.—On the retina red and its relation to vision, by

W. S. S.—On the movements of diatoms and *Oscillatoria*, by Th. W. Engelmann.—On the preparation and conservation of inferior organisms, by R. Blanchard.—On the influence of motion and rest upon life, by Dr. A. Horwath.

SOCIETIES AND ACADEMIES

London

Chemical Society, April 17.—Dr. Roscoe in the chair.—The following communications were made:—On heptane, from *Pinus sabiniana*, by T. E. Thorpe. Wenzell, in 1872, described, under the name of abietene a hydrocarbon obtained by distilling the exudation of the Californian "nut pine." The author has subjected the crude oil (which occurs in commerce in San Francisco) to an exhaustive chemical and physical examination, and finds that it consists of nearly pure heptane. This discovery, that a paraffin is playing the part of oil of turpentine in a tree now living is exceedingly interesting, as our only natural sources of this hydrocarbon are petroleum and fossil fish oil.—On the determination of tartaric acid in lees and inferior argol, by B. J. Grosjean. The author suggests several improvements in the well-known oxalate process of Warington. The employment of the method of filtration suggested by Casamajor, the addition of potassium chloride to render the precipitation of the potassium bitartrate complete, precipitation of the latter salt by stirring, &c. By these improvements the author has shortened the time required for an estimation to four hours.—Conditions affecting the equilibrium of certain chemical systems, by M. M. P. Muir. The author has carefully studied the influence of time, temperature, and mass on certain reactions: 1. Bismuthous chloride, hydrochloric acid, and water. 2. Calcium chloride and potassium or sodium carbonate.—On the action of oxides on salts, Part II., by E. J. Mills and J. W. Pratt. The authors have examined the actions of aluminic, ferric, and stannic oxides on potassic carbonate at a temperature of 735°.—Examination of substances by the time method, by J. B. Hannay. The author has arrived at the following conclusion:—Two hydrated salts, in forming a double salt containing the normal amount of water, expend one-half of the affinity of the anhydrous salt for its water of crystallisation, in combining with each other, showing that the formation of double salts is comparable with other forms of chemical action.—Preliminary note on certain compounds of naphthalene and benzene with antimony chloride, &c., by Watson Smith. The author has obtained white needles, which he believes to be trinaphthylstibine or naphthyloxystibine. He has obtained other crystalline compounds, which have not yet been examined.

Anthropological Institute, April 8.—Mr. Hyde Clarke, vice-president, in the chair.—Mr. Coutts Trotter, of the Bengal Civil Service, was announced a Member.—Prof. W. H. Flower, LL.D., F.R.S., read a paper entitled "Illustrations of the Method of Preserving the Dead in Darnley Island and South Australia." A mummy from Erroob or Darnley Island, in Torres Strait, inhabited by a Papuan race, was first described. It was fastened in an extended position upon a framework made of pieces of wood, joined together with native cords, and kept in an upright position in the house of the relatives. The surface was covered with red ochre, and a piece of the large Indian volute shell (*Melo indica*), fashioned into the shape of a shield, was suspended in front of the body, as worn by the warriors in battle. The whole of the viscera had been removed through an aperture in the right flank, which had been carefully closed by an interrupted suture. Pieces of light wood filled the abdominal and thoracic cavities. The tongue, larynx, &c., had been removed through the mouth; the lips were not closed, but the jaw was kept from falling by a piece of cord passing close to the bone, through the nostril, and round the ramus of the mandible. The orbits were filled with a resinous substance, and imitation eyes of mother-of-pearl introduced. The second specimen described was a dried mummy from near Adelaide, in South Australia, presented in 1845 to the museum of the Royal College of Surgeons by Sir George Grey. In this case the limbs were bent jointly, and fixed by a band of native netting close to the side of the body, the knees being behind the shoulders, and the feet close to the hips. The internal organs had not been removed, but the mouth had been filled with emu's feathers, and carefully sewn up, a tassel of feathers hanging from one corner. Both cases showed a considerable amount of care and trouble bestowed in what was considered the decent and proper care of the body after death; but, as might be expected, a more elaborate development of art was attained in the Papuan than in the Australian.—A paper by Mr. M. J. Walhouse was read, on rag-bushes and kindred observances. The author, referring to the custom of tying pieces of rag to the bushes near springs of healing repute and by the tombs of holy men, once common in England, and still observed on the Continent, adduced evidence of its antiquity, and instances of its occurrence in Europe, Africa, throughout Asia, and all over America from the north to Patagonia. He also described some apparent varieties of custom, when other objects than rags were used, but with the same motive, and thought that they, as well as the rags, were offered as symbols of sacrifice or gifts, sometimes to deities, sometimes to ghosts, and often as thank-offerings for cures of sickness and other benefits. The worthless form of such offerings might be owing to the sacred spots being frequently in remote and desert regions, where travellers and pilgrims were not likely to have things of value to spare, and would leave trivial scraps and shreds ready at hand rather than nothing at all. Or they might be substitutes for more valuable offerings, once generally made, but which have a tendency to decrease in value, and at last exist only nominally as survivals. The Chinese custom of offering mock food and gilt-paper ornaments at tombs, where costly gifts were anciently made, was referred to in illustration of this. It was further suggested that the *ex voto* offerings, so commonly hung in Roman Catholic churches, are a form and development of the rags and shreds tied to bushes, and that may-poles and even Christmas-trees may have had a similar origin.—A number of antiquities from the United States of Colombia were exhibited by Mr. W. D. Powles.

Meteorological Society, April 16.—Mr. C. Greaves, F.G.S., president, in the chair.—The following were elected Fellows of the Society:—R. W. Abbotts, Rev. S. Allen, D.D., E. H. Banks, F. J. Bramwell, F.R.S., J. A. Caird, E. H. Cardwell, the Earl of Durham, J. Farquharson, W. Garnett, Rev. C. W. Harvey, W. Inskip, the Earl of Powis, and D. Robie. The papers read were: On the results of comparisons of Goldschmid's aneroids, by G. M. Whipple, F.R.A.S.—Observations on the temperature of the Atlantic during the month of March, by P. F. Reinsch.

Entomological Society, April 2.—J. W. Dunning, M.A., F.L.S., vice-president, in the chair.—Mr. McLachlan exhibited the cases of a number of species of Brazilian caddis-flies with the insects bred from the larvæ that manufactured some of them, sent to him by Dr. Fritz Müller from Santa Catharina, and read extracts (with notes) from Dr. Müller's letters on the subject. In reference to the habits of Mantidæ, which had been recently brought under the notice of the Society, Mr. Stainton referred to a larval form of probably *Mantis religiosa*, which had been forwarded to him in 1866 by Mr. Moggridge, jun., and which, from its saltatorial habits, that gentleman had described as a "curious grasshopper." De Geer had also drawn attention to the apparent similarity between these insects belonging to different orders, and Mr. Stainton considered that the peculiar motion of the young Mantis was an illustration of the remark of Mr. Darwin, that the relationships and affinities of animals are often more expressed in the embryonic than in the adult form.—Sir Sydney Saunders exhibited a bag-like fabrication, said to be the production of a large species of spider inhabiting the Fiji Islands.—The Secretary read a note from Mr. J. W. Sclater, on insects destroyed by flowers.—Miss E. A. Ormerod communicated a paper entitled "Observations on the Effects of Low Temperature on Larvæ." From an examination of many species belonging to different orders, during the severe frosts of the past winter, none were found materially injured by the low temperature to which they were subjected.—Mr. Distant communicated a paper containing descriptions of new species of hemiptera collected by Dr. Stoliczka during the Forsyth expedition to Kashgar in 1873–74, to form portion of the general work on the scientific results of the expedition now in course of publication at Calcutta.

Geological Society, April 9.—Henry Clifton Sorby, F.R.S., president, in the chair.—Rev. Joseph Finnemore, Thomas James Slatter, William H. Twelvetrees, Arthur Pendarves Vivian, and Ernest Westlake, were elected Fellows; Prof. Bernhard von Cotta, Freiberg, Dr. Nicolai von Kokscharow, St. Petersburg, and Dr. J. J. S. Steenstrup, Copenhagen, were elected Foreign Members; and Prof. P. J. van Beneden, Louvain, Prof. Guglielmo Guiscardi, Naples, and Prof. Gerhard von Rath, Bonn, Foreign Correspondents of the Society.—The following communications were read:—On the geological age of the rocks of the southern highlands of Ireland, generally known as "the

Dingle Beds" and "Glengariff Grits," by Prof. E. Hull, F.R.S. The author has arrived at the following results:—First, that "the Dingle Beds" are perfectly conformable to, and continuous with, the upper silurian beds of the Dingle promontory. Secondly, that they are the representatives of "the Mweelrea Beds and Salrock Slates," of West Galway and Mayo, the age of which, as shown by the fossils, is upper silurian, and that "the Dingle Beds" may therefore be regarded as of the age of the Ludlow Rocks, but unusually developed—the view adopted as far back as 1839 by Sir Richard Griffiths. Thirdly, that throughout the south of Ireland "the Dingle and Glengariff Beds" are disconnected from the succeeding conformable series, consisting of (*c*) lower carboniferous slate; (*b*) the upper old red sandstone with *Anadonta jukesii*; (*a*) the lower old red sandstones and conglomerate, as these three conformable formations are found resting upon, and against, the Glengariff beds successively in a direction either from south to north, or from south-west to north-east, owing to a conformable overlap against the flanks of an old shelving shore formed of the Glengariff beds. Fourthly, that at the close of the upper silurian period, and after the deposition of "the Dingle and Glengariff Beds," these strata were disturbed, upraised, and denuded, and were not again submerged till the commencement of the old red sandstone (*a*), when they were successively overlain by the beds of that formation with the succeeding ones of the lower carboniferous period, probably including the carboniferous limestone in some places. Lastly, that it was during this period of upheaval that, as the author believes, the marine Devonian beds (Ilfracombe and Morte series) were deposited, which accounts for their absence in the Irish area, which was either a land surface or only partially submerged. To this part of the subject the author hoped to call the attention of the Society on a future occasion.—On some three-toed footprints from the triassic conglomerate of South Wales, by W. J. Sollas, F.G.S.—On the silurian district of Rhymney and Pen-y-lan, Cardiff, by W. J. Sollas, F.G.S.

Statistical Society, April 15.—Sir R. W. Rawson, vice-president, in the chair.—Mr. E. G. Ravenstein, F.R.G.S., read a paper on the geographical distribution of the Celtic speaking population of the British Isles. He stated that four Celtic languages are at present spoken in the British Isles, three of which belonged to the northern Gaelic or Gadhelic, and one to the southern or Cymraig branch. The former are Irish Gaelic, Scotch Gaelic, and Manx; the Cymraig branch, since the extinction of Cornish, being now represented only by the Welsh. *Ireland*.—The localities where Irish Gaelic is the language of the majority, are comparatively limited and remote areas, where the population is less dense than in the more fertile and English speaking districts of the island. In 1851, 23·3 per cent. of the population spoke Irish, and in 1871 15·3 per cent. The success of the labours of the "Society for the Preservation of the Irish Language" was referred to, although it cannot be doubted that Irish is on the decrease. Opinions differ as to the agencies to which this decrease must be ascribed. The census on the whole presented a very fair picture of the linguistic condition of Ireland. *Scotland*.—Mr. Ravenstein said that not quite 9 per cent. of the population could speak Scotch Gaelic, and that there was no doubt it was dying out, although in the more remote parts of the Highlands, and in the Hebrides, it still maintains its ground. In the Isle of Man 25·6 per cent. of the population still understood Manx. *Wales* (Cymraig).—Of all the Celtic speaking races in the United Kingdom, the Welsh were the most important, and in the maintenance of their own language they showed by far the greatest amount of vitality. Including 60,000 Welsh in England, there are 1,006,100 Welsh speaking people in Great Britain. The total number of persons in the United Kingdom still speaking a Celtic tongue was:—

Irish Gaelic ...	867,600
Scotch ,, ...	309,250
Manx ...	12,500
Welsh ...	1,006,100
Total ...	2,195,450

or nearly 7 per cent. of the population of the British Isles.

PARIS

Academy of Sciences, April 14.—M. Daubrée in the chair.—The following papers were read:—Law of propagation of expressive nervous affections and phenomena, by M. Rambosson. A movement purely physical may be transformed into one physiological, and into one psychic or cerebral, being transmitted to these different media; and reciprocally, a psychical movement may be transformed with a physiological and a physical; and that without altering in nature, the same phenomena being reproduced after all these transmissions and transformations, on repassing into the same medium.—Studies on Collioure and its environs, by M. Seriziat.—On the curve-place of positions of centres of curvature of a left curve after its development on a straight line, by M. Aoust.—On various experiments with an oscillating pendulum having large amplitudes, by M. Dejean de Fonroque. The pendulum being free to oscillate in all directions, the plane of oscillation becomes rapidly oriented in a particular direction; which the author thinks is nothing but the horizontal projection of the earth's trajectory, or the resultant of the two great motions of translation of the earth, towards Hercules and round the sun. The trajectory in question does not change sensibly in direction, in the course of a day; but in this time the inclination of the horizontal plane (passing through the point of suspension) to this trajectory varies incessantly according to a law easily determined, consequently its projection on this plane must vary, also the direction of the pendulum. M. Cornu, while not accepting the causes assigned, thought the phenomena worthy of attention.—Anomaly of magnetic observations of Paris, by M. Flammarion. He does not allow M. Marié-Davy's explanation.—Fossil fauna of the environs of Castres, by M. Caraven-Cochin. He has discovered several carapaces of tortoises in the eocene sandstone of the place, also jaws and teeth of Lophiodon, scales and teeth of crocodiles (apparently three new species), remains of various mammalia, &c.—On an alteration of the cells of renal epithelium, at the commencement of Bright's disease, by M. Cornil. He describes vacuoles in the cells of the uriniferous tubes, filled with a ball or drop of granular albuminoid matter.—Researches on the Pyrenomycetes of St. Paul and Amsterdam Islands, by M. Crié.—Considerations on the Echinida of the Cenomanian formation in Algeria, by M. Cotteau. He finds remarkable relations of the system in Algeria to that in France.

GÖTTINGEN

Royal Society of Sciences, February 12.—On the constant batteries of Grove and Bunsen, by Herr Fromme.—Report on ear diseases, by Dr. Burkner.

March 1.—On the reduction of Abel integrals to elliptical and hyper-elliptical, by Herr Königsberger.

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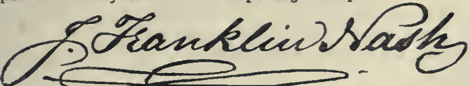

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THURSDAY, November 7.

Linnean Society, at 8.—On the Existence of *Carpesium* (*C. cernuum*?) in Queensland: Lewis A. Bernays.—Notes on Cleistogamic Flowers, chiefly of *Viola, Oxalis*, and *Impatiens*: Alfred W. Bennett.—Descriptions of New Hemiptera: Dr. F. Buchanan White.—On the Absorption of Rain and Dew by the Green Parts of Plants: Rev. George Henslow.

Chemical Society, at 8.—On the Red Colouring Matter of the Lithospermum Erythrorhizon: M. Kahara.—On the Occurrence of Certain Nitrogen Acids amongst the Products of Combustion of Coal Gas and Hydrogen Flames: L. T. Wright.—Second Report on Researches on some points in Chemical Dynamics; Dr. Wright and Mr. Luff.—On the Constitution of the Olefine produced by the Action of Zinc on Ethylic Iodide: Dr. Frankland.

SATURDAY, November 9.

Physical Society, at 3.

SUNDAY, November 10.

Sunday Lecture Society, at 4.—Voltaire: Moncure D. Conway.

MONDAY, November 11.

Royal Geographical Society, at 8.30.

TUESDAY, November 12.

Anthropological Institute, at 8.—Report on Anthropological Proceedings at the Oriental Congress: R. Cust.—On some Characters Tattooed on a Motu Woman: J. Park Harrison, M.A.—On some American Illustrations of the Evolution of New Varieties of Men: Dr. Daniel Wilson.

West London Scientific Association, at 8.

WEDNESDAY, November 13.

Royal Microscopical Society, at 8.—Further Inquiry into the Limits of Microscopic Vision and the Delusive Application of Fraunhofer's Optical Law of Vision: Dr. R. Pigott.—On the Measurement of the Angle of Aperture of Objectives: F. H. Wenham.

THURSDAY, November 14.

Mathematical Society, at 8.—Anniversary.—Instability of Jets: Lord Rayleigh, F.R.S.—Self-strained Frames of Six Joints: Prof. M. W. Crofton, F.R.S.

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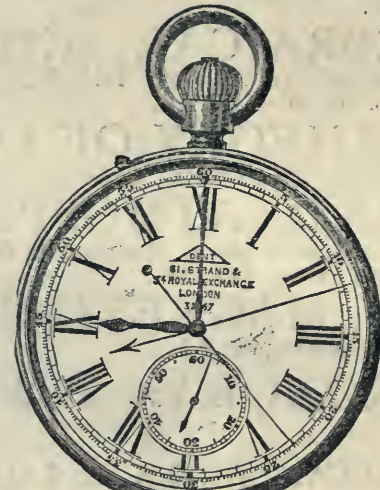

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DIARY OF SOCIETIES.

LONDON

THURSDAY, NOVEMBER 14.

MATHEMATICAL SOCIETY, at 8.—Anniversary.—Instability of Jets: Lord Rayleigh, F.R.S.—Self-strained Frames of Six Joints: Prof. M. W. Crofton, F.R.S.

SUNDAY, NOVEMBER 17.

SUNDAY LECTURE SOCIETY, at 4.—The Philosophy of Heraclitus: Dr. C. J. Grece.

TUESDAY, NOVEMBER 19.

ZOOLOGICAL SOCIETY, at 8.30.—On the Relative Positions to their Construction of the Chambered Shells of Cephalopods: Prof. Owen, C.B.—On the Classification of the Cervidæ, with a Synoptical List of the Existing Species: Sir Victor Brooke, Bart.—On a New Species of Gazelle: Sir V. Brooke, Bart.

STATISTICAL SOCIETY, at 7.45.—Opening Address.

WEDNESDAY, NOVEMBER 20.

GEOLOGICAL SOCIETY, at 8.—On the Upper Greensand Coral Fauna of Haldon, Devonshire: Prof. P. M. Duncan, M.B., F.R.S.—Notes on *Pleurodus affinis*, sp. ined. Agassiz, and Description of the Spine of Cestraciont from the Lower Coal Measures: J. W. Davis.—On the Distribution of Boulders by other Agencies than that of Icebergs: C. E. Austen.

METEOROLOGICAL SOCIETY, at 7.

SOCIETY OF ARTS, at 8.—Opening Meeting.

CROYDON MICROSCOPICAL CLUB, at 8.

THURSDAY, NOVEMBER 21.

ROYAL SOCIETY, at 8.30.

CHEMICAL SOCIETY, at 8.—The Processes and their Comparative Value for Determining the Quantity of Organic Matter in Potable Waters: Dr. C. M. Tidy.—A Chemical Study of Vegetable Albinism: Prof. A. H. Church.—Researches on the Action of the Copper-Zinc Couple on Organic Compounds: Dr. Gladstone and Mr. Tribe.—On a New Gravimetric Method for the Estimation of Minute Quantities of Carbon: Dr. Dupré and Dr. Hake.

LINNEAN SOCIETY, at 8.—Preliminary Report on the Mollusca of the *Challenger* Expedition (viz., the Genera Dentalium, Siphodentalium, and Cadulus): Rev. R. Boog Watson.—On the Symplocaceæ: John Miers.—On Tuberous Branch Terminations in *Vitis gongylodes*: R. Irwin Lynch

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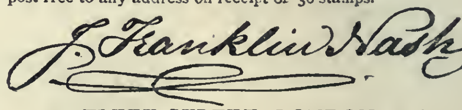

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THURSDAY, NOVEMBER 21.

ROYAL SOCIETY, at 8.30.—On a Method of Using the Balance with great Delicacy, and on its Employment to Determine the Mean Density of the Earth: J. H. Poynting.—On Repulsion Resulting from Radiation, Part VI.: W. Crookes, F.R.S.

CHEMICAL SOCIETY, at 8.—The Processes and their Comparative Value for Determining the Quantity of Organic Matter in Potable Waters: Dr. C. M. Tidy.—A Chemical Study of Vegetable Albinism: Prof. A. H. Church.—Researches on the Action of the Copper-Zinc Couple on Organic Compounds: Dr. Gladstone and Mr. Tribe.—On a New Gravimetric Method for the Estimation of Minute Quantities of Carbon: Dr. Dupré and Dr. Hake.

LINNEAN SOCIETY, at 8.—Preliminary Report on the Mollusca of the *Challenger* Expedition (viz., the Genera Dentalium, Siphodentalium, and Cadulus): Rev. R. Boog Watson.—On the Symplocaceæ: John Miers.—On Tuberous Branch Terminations in *Vitis gongylodes*: R. Irwin Lynch

FRIDAY, NOVEMBER 22.

QUEKETT MICROSCOPICAL CLUB, at 8.

SATURDAY, NOVEMBER 23.

PHYSICAL SOCIETY, at 3.—The Music of Colour and of Visible Motion; Professors W. E. Ayrton and J. Perry.—On a Condenser of Variable Capacity: C. Boys.

CRYSTALLOLOGICAL SOCIETY, at 8.30.—Crystals of Terpene and some of its Derivatives obtained by Dr. Tilden; Amm. and Potass. Teriodides obtained by Mr. Johnson; the Spontaneous Production of Tenorite on Copper Glance: Prof. Maskelyne.—On Cinnabar and Copper Pyrites: Elliot Steel.—The Rhombohedral System: Mr. Lewis.—A Note on the Forms of Sulphur: Mr. Fletcher.—On Rhabdophane: Mr. Lettsom.

SUNDAY, NOVEMBER 24.

SUNDAY LECTURE SOCIETY, at 4.—Long and Short Chronologists: Dr. Zerffi.

MONDAY, NOVEMBER 25.

SOCIETY OF ARTS, at 8.—Cantor Lecture; Mathematical Instruments; W. M. Williams.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

MEDICAL SOCIETY OF LONDON, at 8.30.

TUESDAY, NOVEMBER 26.

ANTHROPOLOGICAL INSTITUTE, at 8.—On the Evils arising from the Use of Historical National Names as Scientific Terms: A. L. Lewis.—On some American Illustrations of the Evolution of New Varieties of Men: Prof. Daniel Wilson, LL.D.—On Left-Handedness: Dr. Henry Muirhead.

WEST LONDON SCIENTIFIC ASSOCIATION, at 8.

METROPOLITAN SCIENTIFIC ASSOCIATION, at 7.

WEDNESDAY, NOVEMBER 27.

ROYAL SOCIETY OF LITERATURE, at 8.—The Earthly Paradise of European Mythology: C. F. Keary.

SOCIETY OF ARTS, at 8.—The Land of Midian: Capt. Burton.

SATURDAY, NOVEMBER 30.

ROYAL SOCIETY, at 4.—Anniversary.

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THURSDAY, NOVEMBER 28.

SOCIETY OF ANTIQUARIES, at 8.30.—The Roof of St. Alban's Abbey: James Neale.

SATURDAY, NOVEMBER 30.

ROYAL SOCIETY, at 4.—Anniversary.

SUNDAY, DECEMBER 1.

SUNDAY LECTURE SOCIETY, at 4.—English Law of Heresy: Prof. W. A. Hunter, M.A.

MONDAY, DECEMBER 2.

SOCIETY OF ARTS, at 8.—Cantor Lecture; Mathematical Instruments; W. M. Williams.

ROYAL INSTITUTION, at 5.—General Monthly Meeting.

LONDON INSTITUTION, at 5.—The Elements of Psychology: Prof. Huxley.

VICTORIA INSTITUTE, at 8.—On Science and Man: Prof. Noah Porter.

MEDICAL SOCIETY OF LONDON, at 8.30.

TUESDAY, DECEMBER 3.

ZOOLOGICAL SOCIETY, at 8.30.—On the Conformation of the Thoracic Extremity of the Trachea, in the Class Aves. Part I. The Gallinæ: Prof. A. H. Garrod, F.R.S.—On Reptiles from Midian, Collected by Major Burton: Dr. A. Günther, F.R.S.—On a New Sylvia from Abyssinia: H. Seebohm, F.Z.S.

WEDNESDAY, DECEMBER 4.

GEOLOGICAL SOCIETY, at 8.—On some Mica-Traps from the Kendal and Sedbergh Districts: Prof. T. G. Bonney, F.R.S., and F. T. S. Haughton, B.A.—Pleistocene Notes on the Cornish Coast near Padstow: W. A. E. Ussher.—The Pleistocene History of Cornwall: W. A. E. Ussher.—On Remains of Mastodon and other Vertebrata of the Miocene Beds of the Maltese Islands: Prof. A. Leith Adams, F.R.S.

ENTOMOLOGICAL SOCIETY, at 7.

SOCIETY OF ARTS, at 8.

THURSDAY, DECEMBER 5.

ROYAL SOCIETY, at 8.30.

LINNEAN SOCIETY, at 8.—Note on *Gardenia turgida*, Roxb.: Chas. B. Clarke.—Geographical Distribution of Indian Freshwater Fishes. Part III. Conclusion: Dr. Francis Day.—Memorandum respecting the Growth of *Melocactus communis?* Dr. W. B. Kesteven.—On some Coleoptera Collected by Chas. Darwin, of Interest as regards Insular Fauna: F. H. Waterhouse.—Remarks on a New British Moss: Ed. Morell Holmes.—Mollusca of *Challenger* Expedition. No. II. (the Genera Seguinzia, Bassillissa, and Gaza): Rev. R. Boog Watson.

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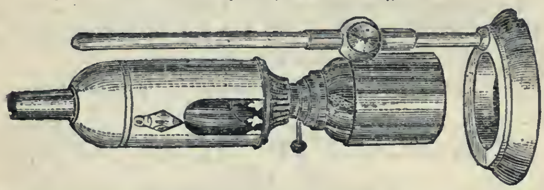

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SUNDAY LECTURE SOCIETY, at 4.—Socialism: Miss Helen Taylor.

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MEDICAL SOCIETY, at 8.30.

TUESDAY, DECEMBER 17.

STATISTICAL SOCIETY, at 7.45.—Reports on the Demographic Congress, the International Statistical Congress, and the International Penitentiary Congress: Dr. Mouat.—Discussion on the Best Form of Statistical Annual for International Purposes.

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SUNDAY LECTURE SOCIETY, at 4.—Sacred Poetry of the Hebrews, Hindoos, &c.: C. J. Plumptre.

MONDAY, DECEMBER 23.

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TUESDAY, DECEMBER 24.

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SATURDAY, JANUARY 4.

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SUNDAY, JANUARY 5.

SUNDAY LECTURE SOCIETY, at 4.—On Anæsthetic Sleep, and the Temporary Abolition of Pain: Dr. B. W. Richardson, F.R.S.

MONDAY, JANUARY 6.

LONDON INSTITUTION, at 5.—Organisation and Moral Feeling: Prof. Maudsley.
MEDICAL SOCIETY, at 8.—First Lettsomian Lecture: Dr. Thorowgood.
INSTITUTE OF ACTUARIES, at 7.—The Rates of Mortality in New South Wales, with a Note on the Formation of Commutation Tables: Prof. Pell.
VICTORIA INSTITUTE, at 8.—The Glacial Epoch: J. C. Southall.

TUESDAY, JANUARY 7.

ANTHROPOLOGICAL INSTITUTE, at 8.—Revised Nomenclature of the Inter-Oceanic Races of Men: S. J. Whitmee.—Ethnological Notes on Koitapu, Motu, and Neighbouring Tribes of New Guinea: W. G. Lawes.
ROYAL INSTITUTION, at 3.—Juvenile Lecture; A Soap Bubble: Prof. Dewar.

WEDNESDAY, JANUARY 8.

GEOLOGICAL SOCIETY, at 8.
ROYAL SOCIETY OF LITERATURE, at 8.—On Rubens and the Antwerp Art Congress: Mr. Carmichael.—On an Unrecorded Event in the Life of Sir Thomas More: Mr. Brabrook, F.S.A.
ROYAL MICROSCOPICAL SOCIETY, at 8.—Observations on *Dactylocalyx pumiceus*, with a Description of a New Variety, *D. stutchburyi*: W. J. Sollas, M.A.—Note on a Revolver Immersion Prism for Sub-stage Illumination: Dr. Edmunds.—An Appliance for Sub-stage Illumination: J. Mayall.—The Thallus of Diatoms: F. Kitton.
SOCIETY OF ARTS, at 7.—Juvenile Lecture.

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ROYAL SOCIETY, at 8.30.—Researches on the Action of Organic Substances on the Ultra-violet Rays of the Spectrum: W. N. Hartley.—On the Electro-magnetic Theory of the Reflection and Refraction of Light: G. F. Fitzgerald.—On Dry Fog: Dr. Frankland.—Note on the Inequalities of the Diurnal Range of the Declination Magnet as recorded at Kew Observatory: Prof. B. Stewart and W. Dodgson.—Some Experiments on Metallic Reflection: Sir John Conroy.
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NATURE

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NO. 480, VOL. 19] THURSDAY, JANUARY 9, 1879 [PRICE SIXPENCE

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TURES at ST. GEORGE'S HALL, Langham Place, each SUNDAY AFTERNOON, commencing at Four o'clock precisely. Sunday, January 5.—BENJAMIN W. RICHARDSON, Esq., M.D., LL.D., F.R.S., on "Anæsthetic Sleep, and the Temporary Abolition of Pain." Second Lecture. With Oxyhydrogen Lantern Illustrations.—Members' Annual Subscription, £1. Payment at the Door—One Penny, Sixpence, and (reserved seats) One Shilling.

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PROFESSOR E. A. SCHÄFER, F.R.S.—Twelve Lectures on Animal Development; Tuesday next, January 14 to April 1.—One Guinea the Course.

J. H. GORDON, Esq.—Four Lectures on Electric Induction; Thursday next, January 16 to February 6.—Half-a-Guinea.

PROFESSOR H. G. SEELEY, F.L.S., F.G.S.—Three Lectures on Reptilian Life; Saturday, January 18, 25, February 1.—Half-a-Guinea.

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Friday, January 17th, at 9 P.M., PROFESSOR TYNDALL on the Electric Light.

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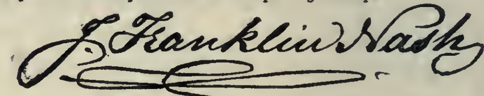

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GEOLOGY.—In the Preface to the Student's

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DIARY OF SOCIETIES.

LONDON

THURSDAY, JANUARY 9.

ROYAL SOCIETY, at 8.30.—Researches on the Action of Organic Substances on the Ultra-violet Rays of the Spectrum: W. N. Hartley.—On the Electro-magnetic Theory of the Reflection and Refraction of Light: G. F. Fitzgerald.—On Dry Fog: Dr. Frankland.—Note on the Inequalities of the Diurnal Range of the Declination Magnet as recorded at Kew Observatory: Prof. B. Stewart and W. Dodgson.—Some Experiments on Metallic Reflection: Sir John Conroy.

MATHEMATICAL SOCIETY, at 8.—On a Theorem in Elliptic Functions: Prof. Cayley, F.R.S.—On a New Modular Equation: Prof. H. J. S. Smith, F.R.S.

LONDON INSTITUTION, at 7.—The English Stage: Prof. H. Morley.

ROYAL INSTITUTION, at 3.—Juvenile Lecture; A Soap Bubble: Prof. Dewar.

FRIDAY, JANUARY 10.

QUEKETT MICROSCOPICAL CLUB, at 8.

SUNDAY, JANUARY 12.

SUNDAY LECTURE SOCIETY, at 4.—Anæsthetic Sleep (Second Lecture): Dr. B. W. Richardson.

MONDAY, JANUARY 13.

LONDON INSTITUTION, at 5.—Good and Bad Etymology: E. B. Tylor.

MEDICAL SOCIETY, at 8.30

TUESDAY, JANUARY 14.

ROYAL INSTITUTION, at 3.—Animal Development: Prof. Schäfer.

ZOOLOGICAL SOCIETY, at 8.30.—On the Habits and Changes of Plumage of Humboldt's Penguin: A. D. Bartlett.—On the Anatomy of *Hyæna crocuta*: Dr. Morrison Watson and Dr. Alfred H. Young.—On a Collection of Birds made by Mr. Huebner, on Duke of York Island and New Britain: Dr. O. Finsch, C.M.Z.S.

WEST LONDON SCIENTIFIC ASSOCIATION, at 8.

WEDNESDAY, JANUARY 15.

SOCIETY OF ARTS, at 8.—Automatic Couplings: T. A. Brocklebank.

METEOROLOGICAL SOCIETY, at 7.—Annual Meeting.

CROYDON MICROSCOPICAL CLUB, at 8.30.—Annual Meeting.

THURSDAY, JANUARY 16.

ROYAL SOCIETY, at 8.30.

LINNEAN SOCIETY, at 8.—On the Colchicaceæ and Aberrant Tribes of Liliaceæ: J. G. Baker.

LONDON INSTITUTION, at 7.—Man's Power of Modifying External Nature: Prof. Rolleston.

ROYAL INSTITUTION, at 3.—Electric Induction: J. E. H. Gordon.

FRIDAY, JANUARY 17.

ROYAL INSTITUTION, at 9.—Electric Light: Prof. Tyndall.

SATURDAY, JANUARY 18.

ROYAL INSTITUTION, at 3.—Reptilian Life: Prof. Seeley.

Printed by R. CLAY, SONS, and TAYLOR, at 7 and 8, Bread Street Hill, Queen Victoria Street, in the City of London, and published by MACMILLAN AND CO., at the Office, 29 and 30, Bedford Street, Covent Garden.—THURSDAY, January 9, 1879.

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DIARY OF SOCIETIES.

London

THURSDAY, January 16.

Royal Society, at 8.30.—On some Points connected with the Anatomy of the Skin; and, On Hyaline Cartilage and Deceptive Appearances Produced by Reagents, as Observed in the Examination of a Cartilaginous Tumour of the Lower Jaw: Dr. G. Thin.—Volumetric Estimation of Sugar by an Ammoniated Cupric Test giving Reduction without Precipitation: Dr. Pavy, F.R.S.—On the Effect of Strong Induction Currents upon the Structure of the Spinal Cord: Dr. W. M. Ord.—Concluding Observations on the Locomotor System of Medusæ: G. J. Romanes.

Society of Antiquaries, at 8.30.—Sepulchral Remains on the Coast of Algeria: Major Heales.—Office of King's Advocate-General: E. W. Brabrook.

Linnean Society, at 8.—On the Colchicaceæ and Aberrant Tribes of Liliaceæ: J. G. Baker.

London Institution, at 7.—Man's Power of Modifying External Nature: Prof. Rolleston.

Royal Institution, at 3.—Electric Induction: J. E. H. Gordon.

FRIDAY, January 17.

Royal Institution, at 9.—Electric Light: Prof. Tyndall.

SATURDAY, January 18.

Royal Institution, at 3—Reptilian Life: Prof. Seeley.

SUNDAY, January 19.

Sunday Lecture Society, at 4.—Is Life Worth Having? T. W. Rhys Davids.

MONDAY, January 20.

London Institution, at 5.—Health and Recreation: Dr. B. W. Richardson.

Society of Arts, at 8.—Mathematical Instruments: W. M. Williams.

Medical Society, at 8.30.—Second Lettsomian Lecture.

Victoria Institute, at 8.—The Argument from Design: Dr. I. P. Thompson.—Genesis and Geology: Rev. E. Duke.

TUESDAY, January 21.

Royal Institution, at 3.—Animal Development: Prof. Schäfer.

Anthropological Institute, at 8.—On Circumcision, its Significance, its Origin, and its Kindred Rites: Elie Reclus.

Society of Arts, at 8.—Retrospect and Prospect in Egypt: B. F. Cobb.

WEDNESDAY, January 22.

Geological Society, at 8.—On Community of Structure in Rocks of Dissimilar Origin: Frank Rutley.—Distribution of the Serpentine and Associated Rocks with their Metallic Ores in Newfoundland: A. Murray, C.M.G.—The Gold Leads of Nova Scotia: H. S. Poole, M.A.

Royal Society of Literature, at 8.—On the Ogham Inscriptions and on the Mushajjar Characters: Capt. R. F. Burton.

Society of Arts, at 8.—Modern Science of Economics: H. D. Macleod.

THURSDAY, January 23.

Royal Society, at 8.30.—Researches on Chemical Equivalence, Part 1: Dr. Mills and T. W. Walton; Part 2: Dr. Mills and J. Hogarth.—Researches on Lactin: Dr. Mills and J. Hogarth.—On the Microrheometer: J. B. Hannay.—Limestone as an Index of Geological Time: T. M. Reade.—Preliminary Note on the Substances which produce Chromospheric Lines: J. N. Lockyer.

Royal Institution, at 3.—Electric Induction: J. E. H. Gordon.

London Institution, at 7.—Fauna and Flora of Prehistoric Periods: Prof. Rolleston.

FRIDAY, January 24.

Royal Institution, at 9.—The Mirror of Japan: Prof. Ayrton.

Quekett Microscopical Club, at 8.—On the Urticating Threads of *Actinia parasitica*: F. A. Bedwell, M.A., F.R.M.S.—On a New Method of Preparing a Dissected Model of an Insect's Brain, from Microscopic Sections: E. T. Newton, F.G.S.

SATURDAY, January 25.

Royal Institution, at 3.—Reptilian Life: Prof. Seeley.

Physical Society, at 3.

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DIARY OF SOCIETIES.

LONDON

THURSDAY, JANUARY 23.

ROYAL SOCIETY, at 8.30.—Researches on Chemical Equivalence, Part 1: Dr. Mills and T. W. Walton; Part 2: Dr. Mills and J. Hogarth.—Researches on Lactin: Dr. Mills and J. Hogarth.—On the Microrheometer: J. B. Hannay.—Limestone as an Index of Geological Time: T. M. Reade.—Preliminary Note on the Substances which produce Chromospheric Lines: J. N. Lockyer.
ROYAL INSTITUTION, at 3.—Electric Induction: J. E. H. Gordon.
LONDON INSTITUTION, at 7.—Fauna and Flora of Prehistoric Periods: Prof. Rolleston.

FRIDAY, JANUARY 24.

ROYAL INSTITUTION, at 9.—The Mirror of Japan: Prof. Ayrton.
QUEKETT MICROSCOPICAL CLUB, at 8.—On the Urticating Threads of *Actinia parasitica*: F. A. Bedwell, M.A., F.R.M.S.—On a New Method of Preparing a Dissected Model of an Insect's Brain, from Microscopic Sections: E. T. Newton, F.G.S.

SATURDAY, JANUARY 25.

ROYAL INSTITUTION, at 3.—Reptilian Life: Prof. Seeley.
PHYSICAL SOCIETY, at 3.—Studies in Vibration: Dr. F. Guthrie, F.R.S.

SUNDAY, JANUARY 26.

SUNDAY LECTURE SOCIETY, at 4.—The Microscope in Geology: Rev. J. F. Blake.

MONDAY, JANUARY 27.

MEDICAL SOCIETY, at 8.30.
LONDON INSTITUTION, at 5.—Health and Recreation: Dr. B. W. Richardson.
SOCIETY OF ARTS, at 8.—Mathematical Instruments: W. M. Williams.
INSTITUTE OF ACTUARIES, at 7.—The Messenger Prize Essay; on Surrender Values: G. S. Crisford.
ROYAL GEOGRAPHICAL SOCIETY, at 8.30.—The Road to Merv: Maj.-Gen. Sir Henry C. Rawlinson, K.C.B.—The Recent Overflow of the Lower Oxus.

TUESDAY, JANUARY 28.

ROYAL INSTITUTION, at 3.—Animal Development: Prof. Schäfer.
WEST LONDON SCIENTIFIC ASSOCIATION, at 8.
METROPOLITAN SCIENTIFIC ASSOCIATION, at 7.

WEDNESDAY, JANUARY 29.

SOCIETY OF ARTS, at 8.—The Distribution of Disease: A. Haviland.

THURSDAY, JANUARY 30.

ROYAL SOCIETY, at 8.30.—On the Effect of Heat on the Di-Iodide of Mercury, HgI_2: G. F. Rodwell and H. M. Elder.—A Comparison of the Variations of the Diurnal Range of Magnetic Declination as Recorded at the Observatories of Kew and Trevandrum: B. Stewart and Morisabro Hiraoka.—On the Determination of the Rate of Vibration of Tuning-Forks: H. McLeod and Lieut. G. S. Clarke.—On Certain Means of Measuring and Regulating Electric Currents: C. W. Siemens.
SOCIETY OF ARTS, at 8.—Gas Illumination; Dr. W. Wallace.
ROYAL INSTITUTION, at 3.—Electric Induction: J. E. H. Gordon.
LONDON INSTITUTION, at 7.—Meaning and Origin of the Laws of Nature: Sir Edmund Beckett.

FRIDAY, JANUARY 31.

ROYAL INSTITUTION, at 9.—Logic of Architectural Design: H. H. Statham.
SOCIETY OF ARTS, at 8.—Quest and Early European Settlement of India: Dr. G. Birdwood.

SATURDAY, FEBRUARY 1.

ROYAL INSTITUTION, at 3.—Reptilian Life: Prof. H. G. Seeley.

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DIARY OF SOCIETIES.

LONDON

THURSDAY, JANUARY 30.

ROYAL SOCIETY, at 8.30.—On the Effect of Heat on the Di-Iodide of Mercury, HgI_2: G. F. Rodwell and H. M. Elder.—A Comparison of the Variations of the Diurnal Range of Magnetic Declination as Recorded at the Observatories of Kew and Trevandrum: B. Stewart and Morisabro Hiraoka.—On the Determination of the Rate of Vibration of Tuning-Forks: H. McLeod and Lieut. G. S. Clarke.—On Certain Means of Measuring and Regulating Electric Currents: C. W. Siemens.
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SATURDAY, FEBRUARY 1.

ROYAL INSTITUTION, at 3.—Reptilian Life: Prof. H. G. Seeley.

SUNDAY, FEBRUARY 2.

SUNDAY LECTURE SOCIETY, at 4.—Religious Parallelisms and Symbolisms: Matthew Macfie.

MONDAY, FEBRUARY 3.

ROYAL INSTITUTION, at 5.—Monthly Meeting.
LONDON INSTITUTION, at 5.—Birth, Life, and Death of a Storm: R. H. Scott.
MEDICAL SOCIETY, at 8.30.—Lettsomian Lecture.
VICTORIA INSTITUTE, at 8.—The Torquay Caves: J. E. Howard.

TUESDAY, FEBRUARY 4.

ZOOLOGICAL SOCIETY, at 8.30.—Notes on Points in the Anatomy of the Hoatzin (*Opisthocomus cristatus*): Prof. A. H. Garrod, F.R.S.—On the Breeding of the Argus Pheasant and other Phasianidæ in the Society's Gardens: Mr. Sclater.—On a New Genus and Species of Salticides: Rev. O. P. Cambridge.
SOCIETY OF ARTS, at 8.—The District to North of Lake Nyassa: H. B. Cotterell.
ROYAL INSTITUTION, at 3.—Animal Development: Prof. Schäfer.

WEDNESDAY, FEBRUARY 5.

GEOLOGICAL SOCIETY, at 8.—On the Occurrence of Pebbles with Upper-Ludlow Fossils in the Lower Carboniferous Conglomerates of North Wales: A. Strahan, M A., and A. O. Walker, F.L.S.—On the Metamorphic Series between Twt Hill and Port Dinorwie: Prof. T. G. Bonney, F.R.S., and F. T. S. Houghton, B.A.—On the Quartz-felsite and Associated Rocks at the Base of the Cambrian Series in North-Western Carnarvonshire: Prof. T. G. Bonney, F.R.S.—On a New Group of Pre-Cambrian Rocks (the Arvonian) in Pembrokeshire: Henry Hicks, M.D. With an Appendix by T. Davis.—On the Pre-Cambrian (Dimetian, Arvonian, and Pebidian) Rocks of Carnarvonshire and Anglesea: Henry Hicks, M.D. With an Appendix by Prof. T. G. Bonney, F.R.S.
SOCIETY OF ARTS, at 8.—Best Methods for Improving Condition of Blind: Dr. T. R. Armitage.

THURSDAY, FEBRUARY 6.

ROYAL SOCIETY, at 8.30.—On Certain Dimensional Properties of Matter in the Gaseous State: Prof. Osborne Reynolds.
CHEMICAL SOCIETY, at 8.—Discussion on Dr. Tidy's Paper on the Processes for Determining the Organic Purity of Potable Waters.
LINNEAN SOCIETY, at 8.—Anatomy of Ants: Sir John Lubbock, Bart.—Bulls-thorn Acacia (*A. sphærocephala*): R. Irwin Lynch.—Habits of Ants, Bees, and Wasps: Sir John Lubbock, Bart.—Position of the Genus *Seguenzia* among the Gastropoda: Dr. J. Gwyn Jeffreys.—Note on the Genus *Oudneya*, Brown: Dr. Henry Trimen.
ROYAL INSTITUTION, at 3.—Electric Induction: J. E. H. Gordon.
LONDON INSTITUTION, at 7.—Britain in the Later Stone-Age: Prof. Boyd Dawkins.

FRIDAY, FEBRUARY 7.

ROYAL INSTITUTION, at 9.—Bells: Rev. H. R. Haweis.
GEOLOGISTS' ASSOCIATION, at 7.30.—Annual Meeting.

SATURDAY, FEBRUARY 8.

ROYAL INSTITUTION, at 3.—Lessing: R. W. Macan.
PHYSICAL SOCIETY, at 3.—Annual General Meeting.

Printed by R. Clay, Sons, and Taylor, at 7 and 8, Bread Street Hill, Queen Victoria Street, in the City of London, and published by Macmillan and Co., at the Office, 29 and 30, Bedford Street, Covent Garden.—Thursday, January 30, 1879.

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No. 484, Vol. 19] THURSDAY, FEBRUARY 6, 1879 [Price Sixpence

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DIARY OF SOCIETIES.

LONDON

THURSDAY, FEBRUARY 6.

ROYAL SOCIETY, at 8.30.—On Certain Dimensional Properties of Matter in the Gaseous State: Prof. Osborne Reynolds.—Absorption of Gases by Charcoal. No. 2. A New Series of Equivalents or Molecules: Dr. Angus Smith.
CHEMICAL SOCIETY, at 8.—Discussion on Dr. Tidy's Paper on the Processes for Determining the Organic Purity of Potable Waters.
LINNEAN SOCIETY, at 8.—Anatomy of Ants: Sir John Lubbock, Bart.—Bulls-thorn Acacia (*A. sphærocephala*): R. Irwin Lynch.—Habits of Ants, Bees, and Wasps: Sir John Lubbock, Bart.—Position of the Genus *Seguenzia* among the Gastropoda: Dr. J. Gwyn Jeffreys.—Note on the Genus *Oudneya*, Brown: Dr. Henry Trimen.
ROYAL INSTITUTION, at 3.—Electric Induction: J. E. H. Gordon.
LONDON INSTITUTION, at 7.—Britain in the Later Stone-Age: Prof. Boyd Dawkins.

FRIDAY, FEBRUARY 7.

ROYAL INSTITUTION, at 9.—Bells: Rev. H. R. Haweis.
GEOLOGISTS' ASSOCIATION, at 7.30.—Annual Meeting.

SATURDAY, FEBRUARY 8.

ROYAL INSTITUTION, at 3.—Lessing: R. W. Macan.
PHYSICAL SOCIETY, at 3.—Annual General Meeting.—On the Variation of the Thermal Conductivity of a Rod with Temperature: Dr. O. J. Lodge.

SUNDAY, FEBRUARY 9.

SUNDAY LECTURE SOCIETY, at 4.—Fertilisation of Flowers: Lawson Tait.

MONDAY, FEBRUARY 10.

ROYAL COLLEGE OF SURGEONS, at 4.—Evolution of the Vertebrata: Prof. Parker.
LONDON INSTITUTION, at 5.—Indian Home Life: Prof. Monier Williams.
SOCIETY OF ARTS, at 8.—Putrefactive Changes; Dr. B. W. Richardson.
MEDICAL SOCIETY, at 8.30.

TUESDAY, FEBRUARY 11.

ANTHROPOLOGICAL INSTITUTE, at 8.—Customs of Australian Aborigines: Capt. W. E. Armit, R.N.—The Australian Aborigines: D. Macallister.
ROYAL INSTITUTION, at 3.—Animal Development: Prof. Schäfer.
GRESHAM COLLEGE, at 6.—The Recent Solar Eclipse: Rev. E. Ledger.
WEST LONDON SCIENTIFIC ASSOCIATION, at 8.

WEDNESDAY, FEBRUARY 12.

ROYAL SOCIETY OF LITERATURE, at 8.—On the History, System, and Varieties of Turkish Poetry; Illustrated by Selections in the Original and in English Paraphrase: J. W. Redhouse, M.R.A.S.
SOCIETY OF ARTS, at 8.—Application of Bessemer Process to the Reduction of Metallic Sulphides: J. Hollway.
ROYAL COLLEGE OF SURGEONS, at 4.—Evolntion of the Vertebrata: Prof. Parker.
ROYAL MICROSCOPICAL SOCIETY, at 8.—Annual Meeting.
GRESHAM COLLEGE, at 6.—Recent Solar Eclipse: Rev. E. Ledger.

THURSDAY, FEBRUARY 13.

ROYAL SOCIETY, at 8.30.—Note on the Development of the Olfactory Nerve and Olfactory Organ of Vertebrates: Dr. Milnes Marshall.—On the Development of the Skull and its Nerves in the Green Turtle (*Chelone midas*), with Remarks on the Segmentation seen in the Skull of Various Types: Prof. Parker.
MATHEMATICAL SOCIETY, at 8.—On a Modular Equation, on Prof. Cayley's Formula, and on the Formula for Four Abelian Functions answering to the Formula for Four Theta Functions: Prof. H. J. S. Smith, F.R.S.—On the Number of Conics which satisfy Five Independent Conditions: M. Halphen.—Construction of Magic Squares: Sir J. Cockle, F.R.S.—Quaternion Proof of Minding's Theorem: J. J. Walker.—Notes on Frames: Prof. Henrici, F.R.S.
ROYAL INSTITUTION, at 3.—Sound: Prof. Tyndall.
SOCIETY OF ARTS, at 8.—Noxious Vapours: A. G. Phillip.
LONDON INSTITUTION, at 7.
GRESHAM COLLEGE, at 6.—Recent Solar Eclipse: Rev. E. Ledger.

FRIDAY, FEBRUARY 14.

ROYAL INSTITUTION, at 9.—November Meteors: Prof. Johnstone Stoney.
QUEKETT MICROSCOPICAL CLUB, at 8.
GRESHAM COLLEGE, at 6.—Recent Solar Eclipse: Rev. E. Ledger.
ROYAL COLLEGE OF SURGEONS, at 4.—Evolution of the Vertebrata: Prof. Parker.

SATURDAY, FEBRUARY 15.

ROYAL INSTITUTION, at 3.—Lessing: R. W. Macan.

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MATHEMATICAL SOCIETY, at 8.—On a Modular Equation, on Prof. Cayley's Formula, and on the Formula for Four Abelian Functions answering to the Formula for Four Theta Functions: Prof. H. J. S. Smith, F.R.S.—On the Number of Conics which satisfy Five Independent Conditions: M. Halphen.—Construction of Magic Squares: Sir J. Cockle, F.R.S.—Quaternion Proof of Minding's Theorem: J. J. Walker.—Notes on Frames: Prof. Henrici, F.R.S.

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LONDON INSTITUTION, at 7.

GRESHAM COLLEGE, at 6.—Recent Solar Eclipse: Rev. E. Ledger.

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ROYAL INSTITUTION, at 9.—November Meteors: Prof. Johnstone Stoney.

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ROYAL COLLEGE OF SURGEONS, at 4.—Evolution of the Vertebrata: Prof. Parker.

SATURDAY, FEBRUARY 15.

ROYAL INSTITUTION, at 3.—Lessing: R. W. Macan.

SUNDAY, FEBRUARY 16.

SUNDAY LECTURE SOCIETY, at 4.—English Freethinkers of the Eighteenth Century: A. Elley Finch.

MONDAY, FEBRUARY 17.

SOCIETY OF ARTS, at 8.—Dwelling Houses; their Sanitary Construction, &c.: Dr. Corfield.

LONDON INSTITUTION, at 5.—History of the Ironclad: F. J. Palmer.

ROYAL COLLEGE OF SURGEONS, at 4.—Evolution of the Vertebrata: Prof. Parker.

MEDICAL SOCIETY, at 8.30.

VICTORIA INSTITUTE, at 8.—Topography of the Sinaitic Peninsula; Rev. F. W. Holland.

TUESDAY, FEBRUARY 18.

ZOOLOGICAL SOCIETY, at 8.30.—Note on *Pachycephala icteroides*, Peale, with Description of a supposed New Species; E. L. Layard, C.M.G.—Descriptions of Four New Species of Chamæleons from Madagascar: Dr. A. Günther, F.R.S.—On a Collection of Mollusca from Japan: Edgar A. Smith.

ROYAL INSTITUTION, at 3.—Animal Development: Prof. Schäfer.

WEDNESDAY, FEBRUARY 19.

METEOROLOGICAL SOCIETY, at 7.—Diurnal Variations of the Barometric Pressure in the British Isles: Frederick Chambers.—On a Standard Cistern Siphon Barometer: Frederick Bogen.—On the Relation existing between the Duration of Sunshine, the Amount of Solar Radiation, and the Temperature indicated by the Black Bulb Thermometer *in vacuo*: G. M. Whipple, B.Sc., F.R.A.S.—Results of Meteorological Observations made at Buenos Ayres: William B. Tripp, Assoc. Inst. C.E.

CROYDON MICROSCOPICAL CLUB, at 8.30.

SOCIETY OF ARTS, at 8.—Turkish Resources: J. L. Haddan.

ROYAL COLLEGE OF SURGEONS, at 4.—Evolution of the Vertebrata: Prof. Parker.

THURSDAY, FEBRUARY 20.

ROYAL SOCIETY, at 8.30.—On Electrical Insulation in High Vacua: W. Crookes.

ROYAL INSTITUTION, at 3.—Sound: Prof. Tyndall.

LONDON INSTITUTION, at 7.—Fixed Water: Prof. Guthrie.

LINNEAN SOCIETY, at 8.—On the Genus *Oudneya*, Brown: Dr. Henry Trimen.—On the Nature of the Inflorescence: Dr. Maxwell Masters.—On some South American Genera of Plants of Uncertain Position: John Miers.

CHEMICAL SOCIETY, at 8.—Investigations into the Action of Substances in the Nascent and Occluded Conditions. Hydrogen (continued): Dr. Gladstone and Mr. Tribe.—On some Methods of Vapour Density Determination: J. T. Brown.—On the Quantitative Blow-pipe Assay of Mercury: G. Attwood.

FRIDAY, FEBRUARY 21.

ROYAL INSTITUTION, at 9.—A New Chemical Industry: Prof. Roscoe.

ROYAL COLLEGE OF SURGEONS, at 4.—Evolution of the Vertebrata: Prof. Parker.

GEOLOGICAL SOCIETY, at 8.—Anniversary.

SATURDAY, FEBRUARY 22.

PHYSICAL SOCIETY, at 3.—On a Current Regulator: Dr. C. W. Siemens, F.R.S.—On a New Theory of Terrestrial Magnetism: Professors Ayrton and Perry.—On the Spectrum of Lightning: Dr. A. Schuster.

ROYAL INSTITUTION, at 3.—Lessing: R. W. Macan.

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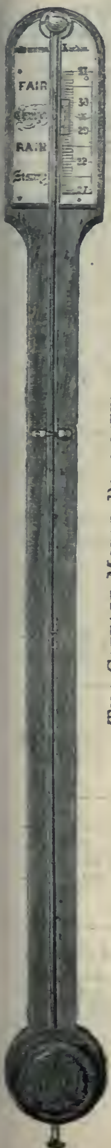
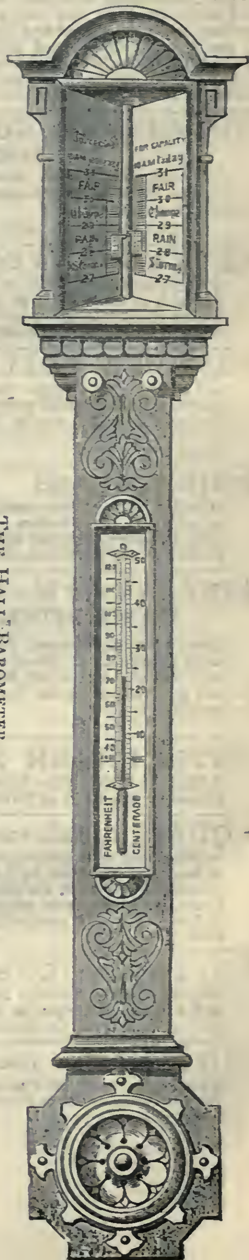


The Cottage Model Barometer.



The Hall Barometer.

DIARY OF SOCIETIES.

London

THURSDAY, February 20.

Royal Society, at 8.30.—On Electrical Insulation in High Vacua: W. Crookes.—On the Reversal of the Lines of Metallic Vapours, No. IV.: Professors Liveing and Dewar.
Royal Institution, at 3.—Sound: Prof. Tyndall.
London Institution, at 7.—Fixed Water: Prof. Guthrie.
Linnean Society, at 8.—On the Genus *Oudneya*, Brown: Dr. Henry Trimen.—On the Nature of the Inflorescence: Dr. Maxwell Masters.—On some South American Genera of Plants of Uncertain Position: John Miers.
Chemical Society, at 8.—Investigations into the Action of Substances in the Nascent and Occluded Conditions. Hydrogen (continued): Dr. Gladstone and Mr. Tribe.—On some Methods of Vapour Density Determination: J. T. Brown.—On the Quantitative Blow-pipe Assay of Mercury: G. Attwood.

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Royal College of Surgeons, at 4.—Evolution of the Vertebrata: Prof. Parker.
Geological Society, at 8.—Anniversary.

SATURDAY, February 22.

Physical Society, at 3.—On a Current Regulator: Dr. C. W. Siemens, F.R.S.—On a New Theory of Terrestrial Magnetism: Professors Ayrton and Perry.—On the Spectrum of Lightning: Dr. A. Schuster.
Royal Institution, at 3.—Lessing: R. W. Macan.

SUNDAY, February 23.

Sunday Lecture Society, at 4.—The English Rule in India: Sir Arthur Hobhouse.

MONDAY, February 24.

Society of Arts, at 8.—Dwelling Houses; their Sanitary Arrangements: Dr. W. H. Corfield.
London Institution, at 5.—Modern Chemical Theory: Prof. H. E. Armstrong.
Royal College of Surgeons, at 4.—Evolution of the Vertebrata: Prof. Parker.
Society of Antiquaries, at 7.—Enfranchisement of Copyholds of Inheritance: E. Smyth.

TUESDAY, February 25.

Royal Institution, at 3.—Animal Development: Prof. Schäfer.
Anthropological Institute, at 8.—The Primitive Human Family: C. Staniland Wake.—Notes on the Colour of Skin, Hair, and Eyes: E. W. Brabrook, F.S.A.
West London Scientific Association, at 8.
Metropolitan Scientific Association, at 7.

WEDNESDAY, February 26.

Geological Society, at 8.—A Review of the British Carboniferous Fenestellidæ: G. W. Shrubsole.—Note on a Femur and Humerus of a small Mammal from the Stonefield Slate: Prof. H. G. Seeley, F.L.S.—Note on *Poikilopleuron Bucklandi*, of E. Deslongchamps, père, identifying it with *Megalosaurus Bucklandi*: J. W. Hulke, F.R.S.—*Vectisaurus valdensis*, a New Wealden Dinosaur: J. W. Hulke, F.R.S.
Royal College of Surgeons, at 4.—Evolution of the Vertebrata: Prof. Parker.
Society of Arts, at 8.—Indian Pottery at the Paris Exhibition: Dr. Birdwood.
Royal Society of Literature, at 8.—On an Unrecorded Event in the Life of Sir Thomas More: Mr. Brabrook, F.S.A.

THURSDAY, February 27.

Royal Society, at 8.30.—Studies in Acoustics. I. On the Synthetic Examination of Vowel Sounds: W. H. Preece and Aug. Stroh.
Royal Institution, at 3.—Sound: Prof. Tyndall.
London Institution, at 7.—The Age of Dragons: B. Waterhouse Hawkins.

FRIDAY, February 28.

Royal Institution, at 9.—The Sorting Demon of Maxwell: Sir W. Thomson.
Quekett Microscopical Club, at 8.—On a Mode of Displaying Objects by the Microscope, Irrespective of their Size (the Micro-megascope): Dr. Matthews, F.R.M.S.—The Rotifers by Dark Field Illumination, Illustrated by Transparencies: Dr. C. T. Hudson, M.A., F.R.M.S.
Royal College of Surgeons, at 4.—Evolution of the Vertebrata: Prof. Parker.

SATURDAY, March 1.

Royal Institution, at 3.—Lessing: R. W. Macan.

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SECOND EDITION.

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Royal College of Surgeons, at 4.—Evolution of the Vertebrata: Prof. Parker.

SATURDAY, March 1.

Royal Institution, at 3.—Lessing: R. W. Macan.

SUNDAY, March 2.

Sunday Lecture Society, at 4.—Sabbath Superstitions: R. A. Proctor.

MONDAY, March 3.

Royal College of Surgeons, at 4.—Evolution of the Vertebrata: Prof. Parker.
London Institution, at 5.—The Moral Lessons of Physiology: Dr. Fothergill.
Society of Arts, at 8.—Dwelling Houses: Dr. W. H. Corfield.
Victoria Institute, at 8.—The Antiquity of Man: Prof. Hughes.

TUESDAY, March 4.

Zoological Society, at 8.30.—Liste des Oiseaux recueillis au Nord du Pérou par M. M. Stolzmann et Jelski, en 1878: L. Taczanowski.—On some Collections of Birds from Kina-Balu Mountain, in North-Western Borneo: R. Bowdler Sharpe.—Observations on the Characters of the Echinoidea. Part I. On the Species of the Genus *Brissus* and on the Allied Forms *Meoma* and *Metalia*: F. Jeffery Bell.
Royal Institution, at 3.—Animal Development: Prof. Schäfer.

WEDNESDAY, March 5.

Royal College of Surgeons, at 4.—Comparative Anatomy of Man: Prof. Flower.
Society of Arts, at 8.—Necessity for Popular and Practical Teaching of Sanitary Science: J. J. Pope.

THURSDAY, March 6.

Royal Society, at 8.30.—Observations on the Physiology of the Nervous System of the Crayfish (*Astacus fluviatilis*): J. Ward.—Preliminary Report upon the Comatulæ of the *Challenger* Expedition: P. H. Carpenter.—On the Character of the Pelvis in the Mammalia and the Conclusions respecting the Origin of Mammals which may be based upon them: Prof. Huxley, Sec. R.S.
Linnean Society, at 8.—On *Bacterium lactis*: G. R. Milne Murray.—Classification of the Maioid Crustacea or Oxyrhyncha: Edward J. Miers.
Royal Institution, at 3.—Sound: Prof. Tyndall.
London Institution, at 7.—English Pronunciation: E. B. Nicholson.

FRIDAY, March 7.

Royal Institution, at 9.—Prof. Huxley.
Society of Arts, at 8.—The Plants of India for Commercial Purposes: J. R. Jackson.
Royal College of Surgeons, at 4.—Comparative Anatomy of Man: Prof. Flower.
Geologists' Association, at 8.

SATURDAY, March 8.

Royal Institution, at 3.—Colbert and Richelieu: W. H. Pollock.
Physical Society, at 3.—On a New Theory of Terrestrial Magnetism: Professors Ayrton and Perry.—On some Experiments with the Quadrant Electrometer: Dr. J. Hopkinson.—On the Maintenance of Constant Temperatures and Pressures: F. D. Brown.

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LINNEAN SOCIETY, at 8.—On *Bacterium lactis*: G. R. Milne Murray.—Classification of the Maioid Crustacea or Oxyrhyncha: Edward J. Miers.

ROYAL INSTITUTION, at 3.—Sound: Prof. Tyndall.

LONDON INSTITUTION, at 7.—English Pronunciation: E. B. Nicholson.

CHEMICAL SOCIETY, at 8.—The Quantitative Blow-pipe Assay of Mercury: G. Attwood.—On Gas Analysis and Gas Apparatus: T. W. Thomas.—The Isomeric Dinaphtyls: Watson Smith.—On the Action of Isomorphous Salts in exciting the Crystallisation of Supersaturated Solutions of each other: T. M. Thomson.

FRIDAY, MARCH 7.

ROYAL INSTITUTION, at 9.—Prof. Huxley.

SOCIETY OF ARTS, at 8.—The Plants of India for Commercial Purposes: J. R. Jackson.

ROYAL COLLEGE OF SURGEONS, at 4.—Comparative Anatomy of Man: Prof. Flower.

GEOLOGISTS' ASSOCIATION, at 8.—On the Insect Fauna of the Palæozoic Period, and the British and Foreign Formations of that Period in which Insect Remains have been detected: H. Goss, F.G.S., F.L.S.

SATURDAY, MARCH 8.

ROYAL INSTITUTION, at 3.—Colbert and Richelieu: W. H. Pollock.

PHYSICAL SOCIETY, at 3.—On a New Theory of Terrestrial Magnetism: Professors Ayrton and Perry.—On some Experiments with the Quadrant Electrometer: Dr. J. Hopkinson.—On the Maintenance of Constant Temperatures and Pressures: F. D. Brown.

SUNDAY, MARCH 9.

SUNDAY LECTURE SOCIETY, at 4.—The Heroism of Non-Conforming: Mrs. Fenwick Miller.

MONDAY, MARCH 10.

LONDON INSTITUTION, at 5.—Theory of Combustion: H. A. Severn.

ROYAL COLLEGE OF SURGEONS, at 4.—Comparative Anatomy of Man: Prof. Flower.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.—The Second Circumnavigation of Lake Nyassa: Dr. James Stewart.

SOCIETY OF ARTS, at 8.—Dwelling Houses: Dr. Corfield.

MEDICAL SOCIETY, at 8.30.

TUESDAY, MARCH 11.

ROYAL INSTITUTION, at 3.—Animal Development: Prof. Schäfer.

ANTHROPOLOGICAL INSTITUTE, at 8.—The Geographical Distribution of Games: E. B. Tylor, F.R.S.—On Gaelic Mythology: Hector Maclean.

HORTICULTURAL SOCIETY, at 1.—Scientific Committee.

WEDNESDAY, MARCH 12.

ROYAL MICROSCOPICAL SOCIETY, at 8.—A Contribution to the Knowledge of British Oribatidæ: A. D. Michael.—On the Development of the Fat Cell: Mr. and Mrs. Hoggan.

GEOLOGICAL SOCIETY, at 8.—On Conodonts from the Chazy and Cincinnati Groups of the Cambro-Silurian, and from the Hamilton and Genesee Shale Divisions of the Devonian, in Canada and the United States: G. Jennings Hinde.—On Annelid Jaws from the Cambro-Silurian, Silurian, and Devonian Formations in Canada, and from the Lower Carboniferous in Scotland: G. Jennings Hinde.—The Gold-leads of Nova Scotia: H. S. Poole, M.A.—On Perlitic and Sphærulitic Structures in the Lavas of the Glyder Fawr, North Wales: Frank Rutley.

ROYAL COLLEGE OF SURGEONS, at 4.—Comparative Anatomy of Man: Prof. Flower.

SOCIETY OF ARTS, at 8.—Compensation of Watches, &c.: Edward Rigg.

SOCIETY OF TELEGRAPH ENGINEERS, at 8.—Adjourned Discussion on Mr. Willoughby Smith's Paper on the Working of Long Submarine Cables, and Mr. Jas. Graves's Paper on Curbed Signals for Long Cables.—Experimental Researches into Means of Preventing Induction upon Lateral Wires; Prof. D. E. Hughes.

THURSDAY, MARCH 13.

ROYAL SOCIETY, at 8.30.

ROYAL INSTITUTION, at 3.—Sound: Prof. Tyndall.

MATHEMATICAL SOCIETY, at 8.—On Differential Equations, Total and Partial, and on a New Soluble Class of the First and an Exceptional Case of the Second: Sir J. Cockle, F.R.S.—Discussion of Two Double Series arising from the Number of Terms in Determinants of Certain Forms: J. D. H. Dickson.—A Property of the Discriminant of the Cubic: Prof. H. J. S. Smith, F.R.S.

LONDON INSTITUTION, at 7.—English Composers for the Virginal and Harpsichord: Ernest Pauer.

SOCIETY OF ARTS, at 8.—Injurious Effects of Air of Large Towns: W. Thomson.

FRIDAY, MARCH 14.

ROYAL COLLEGE OF SURGEONS, at 4.—Comparative Anatomy of Man: Prof. Flower.

ROYAL INSTITUTION, at 9.—History of Games: E. B. Tylor.

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DIARY OF SOCIETIES.

LONDON

THURSDAY, MARCH 13.

ROYAL SOCIETY, at 8.30.—The Influence of Electricity on Colliding Water Drops: Lord Rayleigh, F.R.S.—On the Influence of Coal-dust in Colliery Explosions, No. 2: W. Galloway.—The Contact Theory of Voltaic Action, No. 3: Professors Ayrton and Perry.
SOCIETY OF ANTIQUARIES, at 8.30.—Roman Columbarium at Nottingham: S. D. Walker.—Archæology of Cumberland: R. S. Ferguson.
ROYAL INSTITUTION, at 3.—Sound: Prof. Tyndall.
MATHEMATICAL SOCIETY, at 8.—On Differential Equations, Total and Partial, and on a New Soluble Class of the First and an Exceptional Case of the Second: Sir J. Cockle, F.R.S.—Discussion of Two Double Series arising from the Number of Terms in Determinants of Certain Forms: J. D. H. Dickson.—A Property of the Discriminant of the Cubic: Prof. H. J. S. Smith, F.R.S.
LONDON INSTITUTION, at 7.—English Composers for the Virginal and Harpsichord: Ernest Pauer.
SOCIETY OF ARTS, at 8.—Injurious Effects of Air of Large Towns: W. Thomson.

FRIDAY, MARCH 14.

ROYAL COLLEGE OF SURGEONS, at 4.—Comparative Anatomy of Man: Prof. Flower.
ROYAL INSTITUTION, at 9.—History of Games: E. B. Tylor.
QUEKETT MICROSCOPICAL CLUB, at 8.

SATURDAY, MARCH 15.

ROYAL INSTITUTION, at 3.—Colbert and Richelieu: W. H. Pollock.
GEOLOGISTS' ASSOCIATION.—Visit to the British Museum. A Demonstration on Deinosauria: Prof. H. G. Seeley, F.G.S.

SUNDAY, MARCH 13.

SUNDAY LECTURE SOCIETY, at 4.—Our Puritan Forefathers: Mrs. Fenwick Miller.

MONDAY, MARCH 17.

ROYAL COLLEGE OF SURGEONS, at 4.—Comparative Anatomy of Man: Prof. Flower.
LONDON INSTITUTION, at 5.—Life in Other Worlds: R. A. Proctor.
SOCIETY OF ARTS, at 8.—Dwelling Houses: Dr. Corfield.
MEDICAL SOCIETY, at 8.30.
VICTORIA INSTITUTE, at 8.—Geological Ages and the Mosaic Cosmogony: Rev. E. Duke.

TUESDAY, MARCH 18.

ZOOLOGICAL SOCIETY, at 8.30.—On a New Species of Barn Owl, from the Island of Viti-levu: Dr. G. Hartlaub.—On Female Deer with Antlers: Edward R. Alston, F.Z.S.—Remarks on some Parrots Living in the Society's Gardens: Mr. Sclater.
ROYAL INSTITUTION, at 3.—Animal Development: Prof. Schäfer.
SOCIETY OF ARTS, at 8.—Africa: James Bradshaw.
STATISTICAL SOCIETY, at 7.45.—The Colony of Victoria; its Progress and Present Position: H. H. Hayter.

WEDNESDAY, MARCH 19.

ROYAL COLLEGE OF SURGEONS, at 4.—Comparative Anatomy of Man: Prof. Flower.
METEOROLOGICAL SOCIETY, at 7.—Dew, Mist, and Fog: George Dines.—On the Inclination of the Axes of Cyclones: Rev. William Clement Ley, M.A.—Contributions to the Meteorology of the Pacific. No. III. Samoan or Navigator Islands: Robert H. Scott, F.R.S.
SOCIETY OF ARTS, at 8.—Economical Gardens for Londoners: W. M. Williams.
CROYDON MICROSCOPICAL SOCIETY, at 8.30.

THURSDAY, MARCH 20.

ROYAL SOCIETY, at 8.30.—Note on the Arc produced by a Siemen's Machine; Further Note on the Substances which produce the Chromospheric Lines; Note on Young's List of Chromospheric Lines; Note on some Phenomena attending the Reversal of Lines: J. Norman Lockyer.
ROYAL INSTITUTION, at 3.—Sound: Prof. Tyndall.
LONDON INSTITUTION, at 7.—Living Pianoforte Composers: E. Dannreuther.
LINNEAN SOCIETY, at 8.—On the Rev. T. Blackburn's Collection of New Aculeate Hymenoptera from the Sandwich Islands: Fred. Smith.—On the Asexual Reproduction of Ferns: Thos. Sim.—On Recent Collections of Birds from South Eastern New Guinea: R. Bowdler Sharpe.
CHEMICAL SOCIETY, at 8.—On Per-Plumbic Ethide: E. Frankland and A. Lawrance.—On the Decomposition of Water by Certain Metalloids: C. F. Cross and A. Higgin.—On the Volumetric Determination of Chromium: W. J. Sole.—The Production of the Higher Oxides of Iron, Chromium, Manganese, and Bismuth: W. Foster.

FRIDAY, MARCH 21.

ROYAL COLLEGE OF SURGEONS, at 4.—Comparative Anatomy of Man Prof. Flower.
ROYAL INSTITUTION, at 9.—Detonating Agents: Prof. Abel.

SATURDAY, MARCH 22.

PHYSICAL SOCIETY, at 3.
ROYAL INSTITUTION, at 3.—Etching: Seymour Haden.

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SUNDAY, MARCH 23.

SUNDAY LECTURE SOCIETY, at 4.—Richelieu: W. H. Pollock.

MONDAY, MARCH 24.

ROYAL COLLEGE OF SURGEONS, at 4.—Comparative Anatomy of Man: Prof. Flower.
LONDON INSTITUTION, at 5.—The Life of the Plant: Prof. Bentley.
SOCIETY OF ARTS, at 8.—Dwelling Houses: Dr. Corfield.
MEDICAL SOCIETY, at 8.30.

TUESDAY, MARCH 25.

HORTICULTURAL SOCIETY, at 1.—Scientific Committee.
ANTHROPOLOGICAL INSTITUTE, at 8.—Some Particulars respecting the Native Races of Arctic Siberia. Accompanied by an Exhibition of Ethnological Objects Collected in that Region: Henry Seebohn, F.Z.S.—Some Rock Carvings found near Sydney, New South Wales: Sir Chas. Nicholson, Bart.
ROYAL INSTITUTION, at 3.—Animal Development: Prof. Schäfer.

WEDNESDAY, MARCH 26.

GEOLOGICAL SOCIETY, at 8.—On the Geological Age of the Rocks forming the Southern Highlands of Ireland, generally known as "The Dingle Beds" and "Glen-garriff Grits and Shales: Prof. E. Hull, F.R.S.—Results of a Systematic Survey (in 1878) of the Directions and Limits of Dispersion, Mode of Occurrence, and Relation to Drift-deposits of the Erratic Blocks or Boulders of the West of England and East of Wales, including a Revision of many Years' previous Observations: D. Mackintosh.—On the Southerly Extension of the Hessle Boulder-clay in Lincolnshire: A. J. Jukes-Brown.—On the Glaciation of the Shetland Isles: John Horne and B. N. Peach.
ROYAL SOCIETY OF LITERATURE, at 8.—On the Dramatic Literature of Italy: Dr. Davey.
SOCIETY OF ARTS, at 8.—Treatment of Iron to prevent Corrosion: Prof. Barff.
METROPOLITAN SCIENTIFIC ASSOCIATION, at 7.

THURSDAY, MARCH 27.

ROYAL SOCIETY, at 8.30.—On the Organisation of the Fossil Plants of the Coal-measures, Part X.: Prof. W. C. Williamson.—Observations on the Physiology and Histology of *Convoluta Schultzii*: P. Geddes.
ROYAL INSTITUTION, at 3.—Sound: Prof. Tyndall.
SOCIETY OF ARTS, at 8.—Dodé's Process of Inoxidating Iron: L. M. Stoffel.
LONDON INSTITUTION, at 7.—The Harmonium: Ebenezer Prout.

FRIDAY, MARCH 28.

ROYAL INSTITUTION, at 9.—Etching: Seymour Haden.
QUEKETT MICROSCOPICAL CLUB, at 8.—On the "Dual-Lichen" Hypothesis: Dr. M. C. Cooke, M.A.—On a Successful Method of Examining the Anatomy of *Actinia mesembryanthemum*: F. A. Bedwell, M.A.
SOCIETY OF ARTS, at 8.—Ship Canal through Ramiseran Island: S. McBean.

SATURDAY, MARCH 29.

GEOLOGISTS' ASSOCIATION.—Visit to the British Museum at 2.30.
PHYSICAL SOCIETY, at 3.—On Selective Reflection: Capt. Abney, R.E., F.R.S.—On the Fracture of Colloids: Dr. F. Guthrie, F.R.S.

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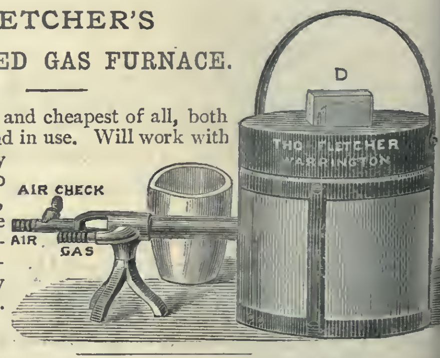

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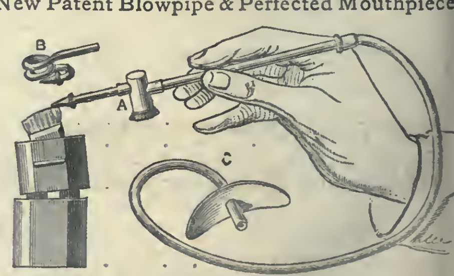

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DIARY OF SOCIETIES.

LONDON

THURSDAY, MARCH 27.

ROYAL SOCIETY, at 8.30.—On the Organisation of the Fossil Plants of the Coal-measures, Part X.: Prof. W. C. Williamson.—Observations on the Physiology and Histology of *Convoluta Schultzii*: P. Geddes.
ROYAL INSTITUTION, at 3.—Sound: Prof. Tyndall.
SOCIETY OF ARTS, at 8.—Dodé's Process of Inoxidating Iron: L. M. Stoffel.
LONDON INSTITUTION, at 7.—The Harmonium: Ebenezer Prout.

FRIDAY, MARCH 28.

ROYAL INSTITUTION, at 9.—Etching: Seymour Haden.
QUEKETT MICROSCOPICAL CLUB, at 8.—On the "Dual-Lichen" Hypothesis: Dr. M. C. Cooke, M.A.—On a Successf l Method of Examining the Anatomy of *Actinia mesembryanthemum*: F. A. Bedwell, M.A.
SOCIETY OF ARTS, at 8.—Ship Canal through Ramiseran Island: S. McBean.

SATURDAY, MARCH 29.

GEOLOGISTS' ASSOCIATION.—Visit to the British Museum at 2.30.

SUNDAY, MARCH 30.

SUNDAY LECTURE SOCIETY, at 4.—Joseph Mazzini: Wm. Clarke.

MONDAY, MARCH 31.

MEDICAL SOCIETY, at 8.30.
INSTITUTE OF ACTUARIES, at 7.—On the Construction of a Combined Marriage and Mortality Table: T. B. Sprague.

TUESDAY, APRIL 1.

ZOOLOGICAL SOCIETY, at 8.30.—To exhibit and make Remarks on a Drawing of the Dolphin (*Delphinus delphis*): Prof. Flower.—Remarks on the Bird's Eggs Collected during the *Challenger* Expedition: Mr. Sclater.—A Contribution to the Avi-fauna of the Zooloo Archipelago: R. Bowdler Sharpe, F.Z.S.
ROYAL INSTITUTION, at 3.—Animal Development: Prof. Schäfer.
STATISTICAL SOCIETY, at 7.45.—The Silver Question: Stephen Bourne.
SOCIETY OF ARTS, at 8.—African Section; Remarks upon an Old Map of Africa contained in Janson's Atlas, published in Paris in 1612, and exhibited by R. Ward.—The Submarine Telegraph to South Africa: J. Sivewright.

WEDNESDAY, APRIL 2.

ENTOMOLOGICAL SOCIETY, at 7.
SOCIETY OF ARTS, at 8.—Some Causes of the Recent Depression in Trade: B. Francis Cobb.

THURSDAY, APRIL 3.

ROYAL SOCIETY, at 8.30.—On the Thermal Conductivity of Water: J. T. Bottomley.—The Preparation in a State of Purity of the Group of Metals known as the Platinum Series, and Notes upon Iridio-Platinum: Geo. Matthey.—On the Reversal of the Lines of Metallic Vapours, No. VI.: Profs. Liveing and Dewar.
ROYAL INSTITUTION, at 3.—Sound: Prof. Tyndall.
CHEMICAL SOCIETY, at 8.—On the Transformation of Aurin into Trimethylpararosaniline: R. S. Dale and C. Schorlemmer.—On a Gold Nugget from South America: Mr. Attwood.—On the Solution of Aluminium Hydrate by Ammonia and a Physical Isomeride of Alumina: G. C. F. Cross.
LINNEAN SOCIETY, at 8.—Notes on *Moquilea*, &c.: John Miers.

FRIDAY, APRIL 4.

GEOLOGISTS' ASSOCIATION, at 8.
ROYAL INSTITUTION, at 9.—Molecular Physics: Mr. Crookes.

SATURDAY, APRIL 5.

ROYAL INSTITUTION, at 3.—Etching: Seymour Haden.

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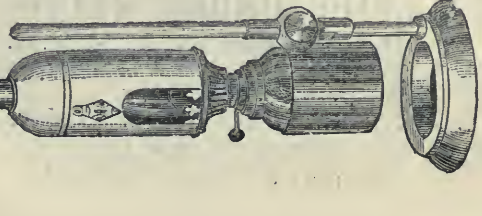

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DIARY OF SOCIETIES.

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THURSDAY, April 3.

Royal Society, at 8.30.—On the Thermal Conductivity of Water: J. T. Bottomley.—The Preparation in a State of Purity of the Group of Metals known as the Platinum Series, and Notes upon Iridio-Platinum: Geo. Matthey.—On the Reversal of the Lines of Metallic Vapours, No. VI.: Profs. Liveing and Dewar.

Royal Institution, at 3.—Sound: Prof. Tyndall.

Chemical Society, at 8.—On the Transformation of Aurin into Trimethyl-pararosaniline: R. S. Dale and C. Schorlemmer.—On a Gold Nugget from South America: Mr. Attwood.—On the Solution of Aluminium Hydrate by Ammonia and a Physical Isomeride of Alumina: G. C. F. Cross.

Linnean Society, at 8.—Notes on *Moquilea*, &c.: John Miers.

FRIDAY, April 4.

Geologists' Association, at 8.

Royal Institution, at 9.—Molecular Physics: Mr. Crookes.

SATURDAY, April 5.

Royal Institution, at 3.—Etching: Seymour Haden.

SUNDAY, April 6.

Sunday Lecture Society, at 4.—Lessons of Materialism: Prof. Maudsley.

MONDAY, April 7.

Royal Institution, at 5.—General Monthly Meeting.

Victoria Institute, at 8.—The Antiquity of Man: T. K. Callard.

TUESDAY, April 8.

Anthropological Institute, at 8.—Exhibition of Antiquities from the United States of Colombia: W. D Bowles.—On the Mummy of a Papuan from Darnley Island: Prof. W. H. Flower, F.R.S.—Rag-bushes and kindred Observances: M. J. Walhouse, F.R.A.S.—Trellech: F. G. Hilton Price, F.G.S.

Horticultural Society, at 1.—Scientific Committee.

West London Scientific Association, at 8.—The Fore-Limbs of Mammals: A. Hensman.

WEDNESDAY, April 9.

Royal Microscopical Society, at 8.—On the Thermal Death Point of known Monad Germs in Fluid: Rev. W. H. Dallinger.—An Illuminating Traverse-Lens: R. B. Tolles.

Geological Society, at 8.—On the Geological Age of the Rocks forming the Southern Highlands of Ireland, generally known as "The Dingle Beds" and "Glengarriff Grits and Shales: Prof. E. Hull, F.R.S.—On the Silurian District of Rhymney and Pen-y-lan, Cardiff: W. J. Sollas, M.A.—On some Three-Toed Footprints from the Triassic Conglomerate of South Wales: W. J. Sollas, M.A.—A Contribution to the History of Mineral Veins: J. Arthur Phillips.

THURSDAY, April 10.

Mathematical Society, at 8.

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CLIFFORD TESTIMONIAL FUND.

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At a meeting held at the Royal Institution, Albemarle Street, on Friday, January 31st, WILLIAM SPOTTISWOODE, Esq., President of the Royal Society, in the Chair, it was resolved that a Fund should be raised for the above-mentioned purpose, and that the sums received should be placed in the hands of Trustees, for the benefit of PROFESSOR CLIFFORD and his family.

PROFESSOR CLIFFORD *died at Madeira on March 3rd. The Executive Committee have now to announce that it is intended to proceed with the raising of the Fund for the benefit of his Widow and Children, who have no other provision.*

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THURSDAY, APRIL 10.

MATHEMATICAL SOCIETY, at 8.

TUESDAY, APRIL 15.

STATISTICAL SOCIETY, at 7.45.—On the Geographical Distribution of the Celtic-speaking Population of the British Isles: E. G. Ravenstein, F.R.G.S.

WEDNESDAY, APRIL 16.

METEOROLOGICAL SOCIETY, at 7.—On the Results of Comparisons of Goldschmid's Aneroids: G. M. Whipple, B.Sc., F.R.A.S.—Observations on the Temperature of the Atlantic during the Month of March: P. F. Reinsch.

CROYDON MICROSCOPICAL SOCIETY, at 8.30.

THURSDAY, APRIL 17.

CHEMICAL SOCIETY, at 8.—On the Determination of Tartaric Acid in Lees and Inferior Argols, with some Remarks on Filtration and Precipitation: B. J. Grosjean.—On Conditions affecting the Equilibrium of Certain Chemical Systems: M. M. P. Muir.

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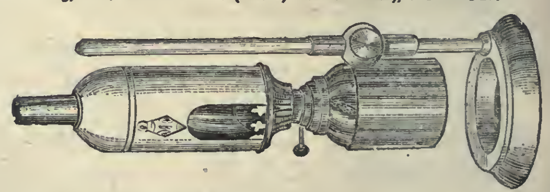

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THURSDAY, APRIL 17.

CHEMICAL SOCIETY, at 8.—On the Determination of Tartaric Acid in Lees and Inferior Argols, with some Remarks on Filtration and Precipitation: B. J. Grosjean.—On Conditions affecting the Equilibrium of Certain Chemical Systems: M. M. P. Muir.

LINNEAN SOCIETY, at 8.—New Guinea Birds: R. Bowdler Sharpe.—Zoological Memoranda, Nile Land: Dr. J. Murie.—On the Occurrence of *Gadus macrocephalus* at the Mouth of the Thames: Dr. F. Day.

SUNDAY, APRIL 20.

SUNDAY LECTURE SOCIETY, at 4.—Relation of Science to Religion: Dr. Andrew Wilson.

MONDAY, APRIL 21.

SOCIETY OF ARTS, at 8.—Recent Advances in Telegraphy: W. H. Preece.
VICTORIA INSTITUTE, at 8.—System of Zoroaster: R. Brown.
MEDICAL SOCIETY, at 8.30.

TUESDAY, APRIL 22.

ROYAL INSTITUTION, at 3.—Schubert: Ernest Pauer.
METROPOLITAN SCIENTIFIC ASSOCIATION, at 7.
WEST LONDON SCIENTIFIC ASSOCIATION, at 8.—Molecular Motion: Dr. W. M. Ord.
HORTICULTURAL SOCIETY, at 1.—Scientific Committee.

WEDNESDAY, APRIL 23.

SOCIETY OF ARTS, at 8.—English Freshwater Fisheries: J. Willes-Bund.
ROYAL SOCIETY OF LITERATURE, at 8.—What is Poetry? G. W. Moon.

THURSDAY, APRIL 24.

ROYAL SOCIETY, at 8.30.
ROYAL INSTITUTION, at 3.—Dissociation: Prof. Dewar.

FRIDAY, APRIL 25.

ROYAL INSTITUTION, at 9.—Generic Images: Fr. Galton.
QUEKETT MICROSCOPICAL CLUB, at 8.—On a Method of Resolving the Finest-lined Diatomaceous Tests: Adolf Schulze.—On the Reproductive System of Certain of the Acarina: A. D. Michael, F.R.M.S.

SATURDAY, APRIL 26.

ROYAL INSTITUTION, at 3.—Architecture: H. H. Statham.
PHYSICAL SOCIETY, at 3.

Printed by R. CLAY, SONS, and TAYLOR, at 7 and 8, Bread Street Hill, Queen Victoria Street, in the City of London, and published by MACMILLAN AND CO., at the Office, 29 and 30, Bedford Street, Covent Garden.—THURSDAY, April 17, 1879.

NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

No. 495, Vol. 19] THURSDAY, APRIL 24, 1879 [Price Sixpence

CLIFFORD TESTIMONIAL FUND.

The friends of PROFESSOR CLIFFORD, who was compelled by ill health to relinquish active work and reside in Madeira, were anxious to present him with a substantial Testimonial in public recognition of his great scientific and literary attainments.

At a meeting held at the Royal Institution, Albemarle Street, on Friday, January 31st, WILLIAM SPOTTISWOODE, Esq., President of the Royal Society, in the Chair, it was resolved that a Fund should be raised for the above-mentioned purpose, and that the sums received should be placed in the hands of Trustees, for the benefit of PROFESSOR CLIFFORD and his family.

PROFESSOR CLIFFORD *died at Madeira on March 3rd. The Executive Committee have now to announce that it is intended to proceed with the raising of the Fund for the benefit of his Widow and Children, who have no other provision.*

Contributions may be paid to the account of the "Clifford Testimonial Fund," with Messrs. Robarts, Lubbock, and Co., or to either of the Honorary Secretaries.

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THURSDAY, APRIL 24.

ROYAL SOCIETY, at 8.30.—On the Nature of the Fur on the Tongue: H. T. Butlin.—Note on the Supplementary Forces concerned in the Abdominal Circulation in Man; and, Note on the Auxiliary Forces concerned in the Circulation of the Pregnant Uterus, and its Contents in Woman: Dr. J. B. Hicks, F.R.S.—Summary of an Inquiry into the Functions of Respiration at various Altitudes on the Island and Peak of Teneriffe: Dr. W. Marcet, F.R.S.—Further Researches on the Physiology of Sugar in Relation to the Blood: Dr. Pavy, F.R.S.

ROYAL INSTITUTION, at 3.—Dissociation: Prof. Dewar.

FRIDAY, APRIL 25.

ROYAL INSTITUTION, at 9.—Generic Images: Fr. Galton.

QUEKETT MICROSCOPICAL CLUB, at 8.—On a Method of Resolving the Finest-lined Diatomaceous Tests: Adolf Schulze.—On the Reproductive System of Certain of the Acarina: A. D. Michael, F.R.M.S.

SATURDAY, APRIL 26.

ROYAL INSTITUTION, at 3.—Architecture: H. H. Statham.

PHYSICAL SOCIETY, at 3.—On Selective Reflection: Capt. Abney.—On some Phenomena connected with Magneto-Electric Induction: C. Boys.—Notes from the Physical Laboratory of University College, Bristol: Dr. S. P. Thompson.

MONDAY, APRIL 28.

SOCIETY OF ARTS, at 8.—Recent Advances in Telegraphy: W. H. Preece.

MEDICAL SOCIETY, at 8.30.

INSTITUTE OF ACTUARIES, at 7.—A Suggestion towards adding a New Feature of Usefulness to the Institute of Actuaries of Great Britain and Ireland: Cornelius Walford.

TUESDAY, APRIL 29.

ANTHROPOLOGICAL INSTITUTE, at 8.—Notes on Analogies of Manners between the Indo-Chinese Races and the Races of the Indian Archipelago: Col. Henry Yule, C B.—Relationships, and the names used for them, among the Peoples of Madagascar, chiefly the Hovas; together with Observations upon Marriage Customs and Morals among the Malagasy: Rev. James Sibree, Jun.

ROYAL INSTITUTION, at 3.—Mendelssohn: Ernest Pauer.

SOCIETY OF ARTS, at 8.—Railways in Africa: J. F. Bell and J. C. Morrell.

WEDNESDAY, APRIL 30.

SOCIETY OF ARTS, at 8.—Discussion on Mr. Hollway's Paper on a New Process in Metallurgy.

GEOLOGICAL SOCIETY, at 8.—A Contribution to the History of Mineral Veins: J. Arthur Phillips.—*Vectisaurus valdensis*, a New Wealden Dinosaur: J. W. Hulke, F.R.S.—The Cudgegong Diamond-field, New South Wales: Norman Taylor. Communicated by R. Etheridge, Jun.—On the Occurrence of the Genus *Dithyrocaris* in the Lower Carboniferous, or Calciferous Sandstone Series of Scotland; and on that of a Second Species of *Anthrapalæmon* in these Beds: R. Etheridge, Jun.

THURSDAY, MAY 1.

ROYAL SOCIETY, at 8.30.—A Magnetic Survey of the Fortieth Parallel in North America between the Atlantic Ocean and the Great Salt Lake, Utah: Prof. Thorpe.—On Certain Definite Integrals occurring in Spherical Harmonic Analysis, and on the Expansion in Series of the Potentials of the Ellipsoid and the Ellipse: W. D. Niven.—On the Origin of the Parallel Roads of Lochaber, and their Bearing on other Phenomena of the Glacial Period: Prof. Prestwich.

CHEMICAL SOCIETY, at 8.—On the Volumes of Liquids at their Boiling Points obtainable from Unit Volumes of Gases: Dr. W. Ramsay.—On a Method of Precipitating Manganese entirely as Dioxide, and its Application to the Volumetric Determination of Manganese: S. Pattinson.—On the Determination of Nitric Acid as Nitric Oxide by Means of its Action on Mercury: R. Warrington.

ROYAL INSTITUTION, at 2.—Annual Meeting.

LINNEAN SOCIETY, at 8.—Note on the Occurrence of a Restiaceous Plant in Cochin China: Dr. Maxwell Masters.—On the Structure of the Pouched Rats of the Genus *Heteromys*: Dr. J. Murie.—Nutrition in its Relation to the Fertilisation of Flowers: Thos. Meehan.

FRIDAY, MAY 2.

ROYAL INSTITUTION, at 9.—Action of Anæsthetics: Prof. McKendrick.

SOCIETY OF ARTS, at 8.—The Wild Silks of India: Thos. Wardle.

GEOLOGISTS' ASSOCIATION, at 7.30.

SATURDAY, MAY 3.

ROYAL INSTITUTION, at 3.—Architecture: H. H. Statham.

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